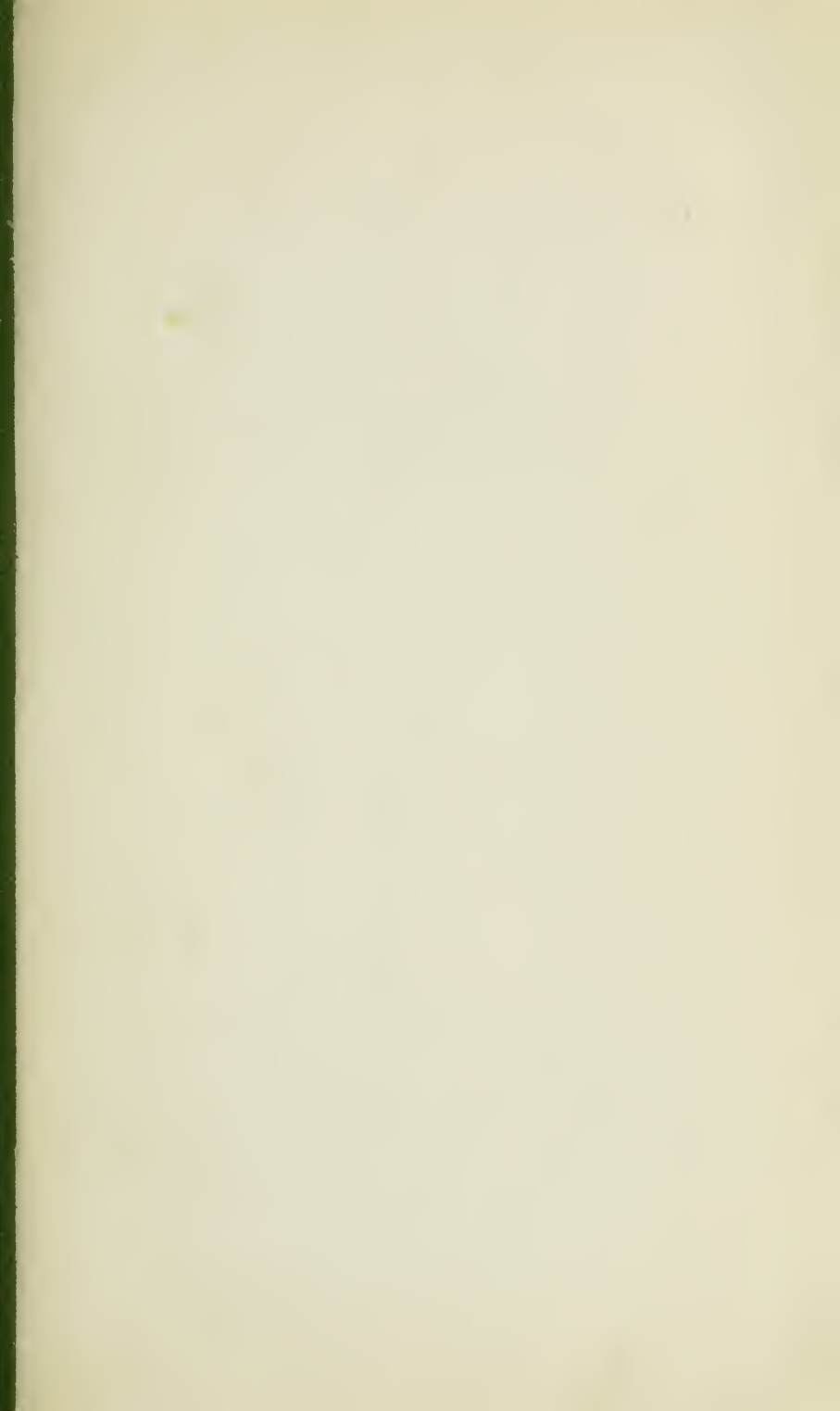


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VOL. IX.

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BEING A

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OF

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THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF
Medicine and the Collateral Sciences.

SATURDAY, OCTOBER 8, 1831.

CASES OF EYE DISEASES, WITH REMARKS.

By MR. MACKENZIE (*of Glasgow.*)

NO. I.

Differing Cases of the same Disease.

If the apothegm be true in regard to the affections of the body in general, it appears to be still more strikingly so in regard to the affections of the eye in particular—viz. that, in its strict sense, sameness can never be predicated of any two diseases. Take, for example, strumous corneitis: we meet with some cases of this disease in which the symptoms are, from first to last, not only so associated with debility of the system, but so unattended with pain, or any other sign of active inflammation, that we treat them, throughout their whole course, and very effectually, with sulphate of quina; while other cases, even in weakly strumous children, are accompanied with such severe pain (I do not mean intolerance of light, but actual pain) that we are obliged to apply leeches round the eye, perhaps even to take blood from the system, to administer nauseants and purgatives, and to put the patient on a course of calomel and opium. A little common sense will easily enable the attentive practitioner to discriminate such *differing* cases of the *same* disease, and a little experience to decide on the appropriate line of treatment.

Is it not very remarkable that the accidents which befall the eye, the same accidents, are, in different individuals, productive of very dissimilar effects—may, that even when the general effect is

the same, the concomitant effects are sometimes exceedingly unlike?

Dislocation of the Lens.

There came to the Eye Infirmary, on the 22d August, 1831, a case of dislocated lens. As this is a very serious injury, and the case interesting on more accounts than one, I shall shortly state the circumstances which attended it.

CASE I.—James Long, æt. 66, nine weeks ago received a blow, with a piece of wood, on the right eye. The blow appears to have ruptured the capsule, and the lens is lying in front of the iris. Pupil much dilated. He has been greatly distressed since the accident, with circumorbital pain during the night, so that he gets little or no sleep. Not much redness of the eye. Pulse 84. Left eye glaucomatous, and he thinks the vision of this eye has failed since the accident which has happened to the right.

The upper half of the cornea being opened in the usual way, the lens immediately escaped, followed by some dissolved vitreous humour. The lids of both eyes were brought together by strips of court-plaister, and he was desired to keep his eyes quiet, as if he were asleep.

23d.—Slept more last night than he has done since the accident. Complains little or none of the pain. There has been considerable watery discharge from the eye. Pupil remains widely dilated. It is uncertain whether he discerns light with the eye.

24th.—Edges of the incision accurately in apposition. Pupil still dilated, and retina apparently insensible to light.

On the 9th September he was dismissed, the eye perfectly free from un-

easiness, but without any return of vision.

This, then, is a case of rupture of the crystalline capsule, from a blow on the eye; a large hard lens passing through the pupil and lodging in contact with the cornea, lodging there for nine weeks without becoming cataractous or undergoing any sensible diminution of size, but causing incessant uneasiness, and during the night severe circumorbital pain. The practice? Simply extraction of the lens, which, under such circumstances, must be regarded exactly as a foreign body. The patient did not require a single opiate, and had scarcely the least twinge of pain after the lens was removed.

I shall compare this case with some other cases of dislocated lens which have fallen under my observation.

CASE II.—A stout countryman, while working in a quarry, received a blow with a piece of stone on his right eye, four weeks before I saw him. Like a wise man, he put himself under the care of a blacksmith, who allowed him to remain without any thing being done which could effectually relieve him of the incessant and excruciating pain which he suffered in the eye and head. The sclerótica was intensely inflamed, the cornea unnaturally prominent and somewhat hazy, the iris in contact with the cornea, and the lens, broken in pieces and apparently swollen, lying in contact with the iris and cornea. Immediately below the cornea the sclerótica presented a concave depression, the result of the blow.

It was evident that the capsule had burst, and that the lens, being pressed forwards, had obliterated the aqueous chambers; an accident which is always productive of severe pain. I immediately opened the cornea by a small section at its upper edge, and instantly the soft disorganized lens was evacuated. The patient had four grains of calomel and two of opium at bed-time. The pain entirely ceased in the course of the afternoon, and never returned. On the seventh day after the extraction he left Glasgow, to return into the country. There was still considerable zonular redness; shreds of opaque capsule occupied the dilated pupil; the lower part of the sclerótica still presented a concavity, instead of its natural convexity; vision, which, from the violence of the injury and long neglect of proper means

of relief, there was reason to fear had been altogether lost, appeared in some slight degree to be returning, the patient perceiving light and shadow when he looked to his right.

A remarkable circumstance in his case was the permanent flattening given to the sclerótica by the momentary pressure of the stroke. We are not surprised that the eye should be bent for a moment from its natural shape, but that this effect should continue, is a thing not easily explained. We see this, however, happen to the eye not unfrequently; and some time ago the cornea presented itself in this deformed state in a patient of the Eye Infirmary.

CASE III.—An angular fragment of iron, about half an inch long, chipped off by a chisel, penetrated the right cornea of Samuel Lamont, within a line of its upper margin. Though immediately removed, it produced cataract and dislocation of the lens, with very violent inflammation, which continued unabated for five weeks before the patient came to the Eye Infirmary, notwithstanding bloodletting, leeching, and blistering. The lens was extracted by Mr. Rainy, with the same striking relief which extraction generally affords in such cases. The cornea, at his admission, was more convex in its horizontal than in its vertical section, as if it had been compressed laterally. This form, which it permanently retained, served materially to disturb the patient's vision. The retina appeared to be sound, and he saw considerably more distinctly when he viewed objects through a four-inch convex glass.

CASE IV.—On the same day that the countryman came to me, whose case I have already related, Mr. James Brown brought to me, for consultation, a lad of 17, a potter, under the following circumstances. His right eye had been destroyed in childhood. For a considerable time he had been troubled with *muscæ volitantes* before his left eye. On the 1st September he received a very slight blow on this eye, with a bit of potter's clay, thrown at him in sport by one of his fellow-workmen. On the morning of the 4th he found, on rising out of bed, that he saw indistinctly, and went out to wash his eye at a well, supposing that the dimness of sight arose from matter adhering to the eye-lids. This produced no difference in the sight; on which one of his neighbours looked

at the eye, and told him that he saw something wagging in the inside of it. He immediately came to Mr. Brown, who discovered the lens lying at the bottom of the anterior chamber. It seemed quite transparent even on the 5th, when I saw him, and was so much reduced in size as to move freely about on every inclination of the head. Its lower edge, where it rested on the floor of the anterior chamber, was somewhat square, as if absorption had been going on more rapidly there than elsewhere. The upper edge of the pupil was not covered by the lens, so that he saw over it. Belladonna was applied on the afternoon of the 5th, the pupil dilated, and that evening the lens fell back through the pupil into the posterior chamber. Next day he had no pain, and saw well, but the iris was evidently tremulous. The lens will, I have no doubt, dissolve; but the rest of the eye is, unfortunately, not in a sound state, and will probably become amaurotic.

CASE V.—We have an illustration of another variety of dislocation of the lens, in the right eye of Mary Mains, aged 50, whom we admitted at the Eye Infirmary on the 7th September.

A month before her admission, she received a blow with the fist on the right orbital region. The integuments had been divided above the eyebrow, but the wound at her admission was perfectly healed, although the cicatrice was still tender to the touch. The right pupil was dilated, the iris tremulous, the humours glaucomatous, the sclerótica and conjunctiva slightly injected with blood, and she had violent hemicrania. She could, with the affected eye, still distinguish the fingers and other large objects. Pulse 78; much thirst; bowels bound.

On the admission, then, of this patient, there was no appearance of dislocation of the lens. There was amaurosis, and, from the tremulousness of the iris, there was strong reason to suspect a dissolved state of the vitreous humour. I took the opportunity to remark to the students, that this was one of that class of cases which are apt to be mistaken for examples of sympathetic amaurosis, arising from an injury of some of the branches of the fifth pair. She had received a cut above the eyebrow, and, had she not been conscious that her eye was struck as well as her brow, we might have been led to suppose, perhaps, that the cut on the brow

was the cause of the failure of sight. I believe that many, perhaps most, of the cases set down as examples of sympathetic amaurosis, originating in injury of the branches of the fifth pair, have actually been cases in which the eye, as well as the edge of the orbit, had been struck, and the amaurosis, in fact, direct, and not at all sympathetic.

This patient was bled at the arm, to the extent of twenty-five ounces; had six grains of calomel and two of opium at bed-time, and next morning a dose of sulphate of magnesia. On the 9th, the hemicrania was much relieved. She was ordered a pill of two grains of calomel and one of opium every night. On the 10th, she told us that the pain had returned in the afternoon of the 9th, and that it still continued very severe. The conjunctiva and sclerótica we found more inflamed. She was cupped on the right temple, and ordered to take two of the pills at bed-time. The pain was relieved by the cupping. On the 12th the salts were repeated. On the 13th the pain continued easier, and the redness was less. On the 14th, she told us that the pain had set in again on the afternoon of the 13th, and had continued severe till about eight in the evening, when it ceased, and she found that her vision had become much better than it was the day before. At the visit, we found the eye free from pain, and she saw so much better as to be able to distinguish one individual from another. The tremulousness of the iris was not so observable, but the pupil was more dilated. The lens had, from the previous day, become moveable, and was seen, of a glaucomatous appearance, bobbing about, at a considerably greater distance than natural, behind the pupil, on every motion of the head. The upper part of the dilated pupil was clearer than the rest, the lens being partially sunk.

This, then, was a case of displacement of the lens, although it still remained behind the iris. This dislocation, however, we may call *secondary*, as it did not result immediately from the injury, but from the vitreous humour undergoing a farther degree of dissolution than it had done at the patient's admission, a month after the injury.

It must not be supposed that these five cases illustrate all the varieties of dislocation of the lens. That in which the lens passes, through a rupture of the

choroid and sclerotica, so that it is under the conjunctiva in the sixth; and several other varieties might be still mentioned.

Glaucomatous Cataract.

The case which I have first related (that of Long) is interesting in another point of view—it illustrates the nature of glaucoma. I have mentioned that the uninjured eye was glaucomatous. It presented a light greenish opacity, apparently seated in the posterior part of the lens. It is extremely probable that the right eye was in the same glaucomatous state before the accident.

The changes which I have met with in examining glaucomatous eyes after death, are the following:—The lens of an amber colour; the vitreous membrane dissolved; the retina destitute of limbus luteus and foramen centrale; and the choroid deprived of its pigment. Upon this combination of changes I have been led to conclude that the symptoms in glaucoma depend.

In Long's case we had a good opportunity of ascertaining the state of the lens in this disease. The circumferential portion was exactly of the colour and transparency of a bit of common amber. When the lens was held up to the light, its central portion appeared of a deep amber colour, inclining to reddish-brown. On dividing the lens into halves, by a section perpendicular to its surfaces, it was evident that it was the posterior lamellæ which were chiefly affected with the reddish-brown discoloration. There was no appearance of the whitish opacity constituting common lenticular cataract; but the section displayed a want of lustre at the reddish-brown part, as if it were beginning to suffer desiccation, while the light amber part was moist, shining, and pellucid.

The patient with glaucoma sees ill, partly from the retina being unsound, partly from the choroid being unable to absorb the rays of light, partly from the light not being freely transmitted by the central dark-coloured portion of the lens; but still he sees, and sees for many years after the glaucoma has become observable, a sufficient quantity of light for the perception of objects being transmitted through the circumferential portion of the lens.

Glasgow, September 26, 1831.

CASE OF GANGRENE OF THE LEG

OCCURRING IN MEASLES:

Amputation—Recovery.

Communicated by JOHN PAUL, M.D.

SAMUEL CLARKE, 21 years of age, was admitted into Gray's Hospital, under my care, on the evening of the 4th of July 1831, with the leg, from the knee downwards, in an entire state of sphacelus. From the account given of him, it appeared that he had been taken ill of measles on the 17th of May, till which time he had enjoyed perfect health, was active in his habits, and without any vice of constitution; that the fever, preparatory to the appearance of the rash, had run very high, and that the rash had only appeared and disappeared for about eighteen hours, never coming out in the usual manner. That on the 29th of May he was seized with a most acute pain near the junction of the tendon and body of the gastrocnemius muscle, the fever continuing all the while with great severity, accompanied with delirium; that along with this pain he complained of a sense of coldness in the leg and foot, but more particularly in the sole of the foot; and that black spots, with vesications, appeared on the leg on the third day from the seizure of the pain; and on the sixth day the line of separation began, when there was a good deal of swelling in the leg and about the knee. Measles were raging epidemically at the time, in a mild form, and several members of Clarke's family had taken them.

He had not had the benefit of medical advice till gangrene was considerably advanced.

On his admission, it was found that ulcerative absorption had made great progress in separating the mortified limb; the line of demarcation appeared to be passing through the knee-joint. The skin, cellular membrane, and fascia round the joint, over the upper part of the tibia, and upon the calf of the leg, were removed; the ham-string muscles were also divided, and some portion of the capsular ligament laid open. The heads of the gastrocnemius, popliteus, and plantaris, together with the blood-vessels and nerves in the ham, remained undivided, as well as the crucial ligaments and ligament of the patella. On

the living side of the line of demarcation, the edges of the integuments were tumid, hot, and presented some appearance of granulations. The articular cartilages of the condyles of the femur were thickened and partly detached; the bone underneath was soft, spongy, and porous. All below the line of demarcation was black; every thing which constitutes vitality was totally annihilated. The integuments of the leg were dry, shrivelled, and adherent to the muscles. From the ulcerated excavations around the knee there was a profuse ichorous discharge; the fœtor was horribly offensive; in the ham there was a sinus running up along the bone for about two inches; the thigh was a good deal swelled. There was at this time no hiccough, no delirium, yet the powers of life were reduced to the very lowest state of existence, but he was cheered at the prospect of something being now done to save him; he was firm, resolute, and anxious to have the limb removed. In his face there was a circumscribed hectic flush, and his pulse was upwards of 145. In this case the question of practice evidently resolved itself into two considerations, namely, either to rely on the resources of nature, giving her at the same time all due assistance, or without delay to free the system of the local irritation by amputating the limb above the knee-joint. Adopting the first alternative rather than the latter was, in my opinion, subjecting the patient to the greater hazard; because whilst the system was labouring under the highest possible degree of irritative typhoid fever, the cause of irritation was allowed to continue in full operation, the patient in the meantime running the greatest risk of losing his life before separation would take place; and if the undivided parts at the line of separation were cut through by art, the immense surface then to be dealt with, and the profuse suppuration that must have ensued, as well as exfoliations from the femur, with the attendant febrile disturbance, were, under all the existing circumstances, matters fraught with no ordinary danger; whereas the latter alternative not only relieved the system at once of the offending cause locally, but removed from the patient's sight a spectacle most appalling to his feelings, and so offensive to his sense of smell that it could not fail to act injuriously through the medium of the

olfactory nerves. It might be objected to the operation, that the patient in his enfeebled state would not rally from the shock of it and the consequent loss of blood; and that it was uncalled for, nature being adequate to accomplish the process of separation. The loss of blood at this stage of gangrene was, without doubt, dangerous in the extreme, and to be most scrupulously guarded against; but independently of this, any apprehension of danger from the shock of the operation itself, if expeditiously done, appeared to be groundless. That nature would throw off the limb there was no doubt, provided the patient lived long enough, but that was the question, and a very difficult one it was to answer. From the views, then, which I took of the case, there was no doubt on my mind, and I think it will be conceded, that the comparative chance of safety for the patient was much greater by amputation above the knee than trusting to the process of natural amputation, whether aided or not; and such procedure is, if I am not greatly mistaken, in accordance with the most approved principles of surgery.

Having decided on the propriety of amputation above the knee, it was performed by the circular operation, after allowing the patient a few hours of rest. I preferred in the present instance the circular to the flap operation, on account of less surface being exposed, as I entertained some apprehension of union not taking place kindly; but in this I was mistaken, for the patient at the end of three weeks was able to be on crutches, so well did the stump unite, and so rapidly did his general health improve, no untoward symptom having occurred farther than a slight disposition to diarrhœa. The femoral artery and vein were observed to be plugged up with coagulated blood, which in the artery evidently extended high up, as all traces of pulsation were lost a few inches below the groin. The vessels which I had to secure were unusually numerous, large in size, and highly active, so that the profunda and its branches must have been prodigiously enlarged, as well as the anastomosing communications, with the sciatic and gluteal arteries. In this condition of the blood-vessels was displayed a beautiful and wonderful provision of nature; but in her operations intelligent design can seldom fail to be seen, though

never more unequivocally apparent than in the present instance. I have mentioned that the blood was coagulated in the femoral artery, and nearly as high as where the profunda is given off, and the reason of this is at once obvious, because by this means the supply of blood to the leg through the principal vessel was not only cut off, but through every anastomosing channel, the muscular branches of the femoral artery below the point of coagulation being rendered useless; and in this manner the danger of hæmorrhage was effectually guarded against. Nothing, indeed, could be more grand than the adaptation of means here employed to prevent hæmorrhage, in the event of natural amputation taking place; that to maintain the supply of blood to the thigh was equally so; for, as already mentioned, the profunda and its branches were so greatly enlarged that the circulation in the thigh was energetically kept up, and that by vessels not carrying any blood beyond the knee, unless we take into account the minute anastomosis of the extreme branches of the profunda, with recurrent branches from the arteries of the leg.

It would not now be uninteresting to inquire into the causes which produced this gangrenous affection of the leg, if there was any likelihood of arriving at accurate conclusions; but little, I am afraid, can be advanced beyond speculation, which in every inquiry after truth is very unsatisfactory. Gangrene in a patient so young as Clarke, to the same extent, and under the same modifications, is remarkably rare. That it depended on constitutional causes there cannot, I think, be any reasonable doubt, and most likely was attributable to the rash not coming out; for in eruptive complaints it is thought that the specific virus on which they depend is eliminated from the body by the rash appearing for a definite time: and when it does not come out, the virus being in that case locked up, as it were, in the system, an aggravated form of fever is kept up, nervous power is diminished, and gangrene, as in the case under consideration, may ensue. Eruptive fevers, too, predispose the system to gangrene, as we see exemplified in a blistered surface in measles, not unfrequently becoming gangrenous. The occurrence of gangrene in the throat in scarlet fever proves the same thing; and as this

affection of the throat is greatest when there is the least rash on the skin, it naturally follows that it must be through the medium of the rash that the morbid matter is thrown off.

Elgin, Sept. 23d, 1831.

PREMATURE LABOUR.

To the Editor of the London Medical Gazette.

SIR,

IN a case of premature labour, brought on by puncturing the membranes, which I communicated to you some months since, and which you did me the favour to publish in the Gazette, I alluded to a previous instance, in which the same operation had been performed by Mr. Day, with equally good success. In both cases there existed a large projection of the sacrum, which so narrowed the antero-posterior diameter of the pelvic brim, that it was considered impossible for a child of ordinary size, at the natural period of gestation, to be born entire.

The latter of these women, having become again pregnant, was earnestly desirous that the means which had formerly saved her so much suffering, as well as preserved the life of her infant, should once more be had recourse to. This was done by Dr. Day, on the 22d ult. at which time she was about thirty-four or thirty-five weeks advanced in pregnancy. Thirty hours after the membranes had been punctured, labour pains came on, and she was soon delivered of a living child.

Those who have had the misfortune to be engaged in cases where, after waiting for hours, perhaps for days, in the most dreadful perplexity—after beholding the mother's strength gradually sapped by violent, yet ineffectual, pains, it has in the end been found necessary to perform embryotomy; a measure to which not even its necessity can reconcile a feeling mind—will be able to appreciate the advantage of this operation, by which we have it in our power, without suffering to either party, to preserve the life both of the mother and of the child.—I am, sir,

Your obedient servant,

JOHN M'DIVITT.

Regworth, September 26, 1831.

ON THE
JATROPHA CURCAS (THE TUVA TREE
OF THE PHILIPPINE ISLANDS),

AND ON

The Medicinal Properties of its Seeds, &c.

By GEORGE BENNETT, Esq.

Corr. Mem. of the Medico-Botanical Society of
London, &c. &c.

[With an Engraving.]

Jatropha Curcas.

Natural family Euphorbiaceæ.
Class, Monoccia. Order, Monadelphæa.

Generic Character.

Masculi. Calyx nullus seu pentaphyllus.
Cor. monopetala infundibuliformis. Stamina, 10: alterna breviora.
Feminei. Calyx nullus. Corolla pentapetala, patens. Styli. 3 bifidi. Caps. trilocularis. Sem. 1.

Specific Character.

Jatropha foliis cordatis angulatis.—*L. Hort. Cliff.* 445.

Jatropha. Assurgens, ficus folio, flore herbaceo.—*Brown, Nat. Hist. of Jamaica.*
Ricinus ficus folio, flore pentapetalo, &c.—*Sloan, Cat.* 40.

Flos calyce et corolla pentapetala utrisque. Stamina decem. Fructus carnosus, globosus, nec angulatus.—*Willdenow Species Plantarum, Vol. 4, page 561.*

THIS tree is found abundantly at the Philippine Islands, where it is named Tuva; at Ceylon, where it is named Rata-endaru; in the East and West Indies; and, indeed, in all tropical countries. It is usually cultivated for fences, &c. being a tree of rapid growth, and readily propagated by cuttings. At the Philippine Islands, (as well as in India and the Polynesian Islands) the Dap-dap, or Erythrina corollodendron, which bears a profusion of brilliant scarlet flowers, is used for a similar purpose. The Tuva tree is branchy, but does not attain a greater height than from ten to fifteen feet, and is of small circumference; its wood, being of a very soft quality, is useless except as fire-wood.

Similar to all the euphorbiaceæ family, it contains a milky, acrid, glutinous juice, which, when dropped on white linen, produces an indelible stain,

which is at first of a light blue or bluish green colour, but which, after being washed, changes to a permanent dark brown colour; it might, consequently, be employed as a marking ink.

The fruit is globular and fleshy, containing three seeds in distinct cells, and grows in clusters of from four to six; when immature it is of a green colour, which changes to yellow when ripening, and becomes black when quite mature. The seeds are oblong, and of a black colour; the husk being removed, a white kernel remains, which contains much oil, and has an agreeable taste.

The leaves and seeds of this tree are used medicinally by the natives of the Philippine Islands; the former being rubbed over with slaked lime, are used as an external application for headaches, &c.; but as I have observed the leaves of other trees used for a similar purpose, the benefit of their application is probably to be attributed merely to the sensation of coolness they impart in common with other leaves, and which is so agreeable when there is much determination of blood to the head.

The seeds are used as an aperient, but they also possess an emetic property; they do not act speedily as an emetic, an interval of from an hour to an hour and a half elapsing previous to actual vomiting taking place; a nausea, however, is felt soon after taking the seeds, attended by an unpleasant burning sensation in the stomach and bowels, and in some instances, also, in the throat. This emetic property is considered to exist in greater power in the embryo; the husk, and thin membranaceous covering of the kernel, does not contain it, as no difference is perceived after their removal. The modus operandi of this remedy being mild, and the seeds of an agreeable sweetish taste, it would form a pleasant medicine to administer, could its emetic property be separated.

Respecting the active principles contained in the seed, it appears from a chemical analysis by Pelletier and Caventon, (in the Journal de Pharmacie, 1818, No. 7, et Essai anal. sur la graine du médecin cathartique) to contain, besides other principles, an oil and a peculiar acid.

The albumen seems mild; the embryo highly active, which is to be attributed to a volatile principle, as in many of the euphorbiaceæ. Piso says, that

three or four seeds eaten, purge, and violently, if the interior membrane (embryo) is not removed, and therefore it should be always taken away;" and again, "that the nuts should be roasted, that they might operate more gently."

It is stated by Adrien Jussieu *, that "the emetic effects are produced either by the oil or farina, when taken internally or applied externally, but much more speedily from the oil, and still more violently by the acid, to which the strength of the oil appears attributable, for when made into a saponaceous substance its effect is lost."

The seeds are collected by the natives of the Philippine Islands for the purpose of expressing the oil, which they use for burning in their lamps, as well as for medicinal purposes.

The seeds are used by the native doctors at the Philippine Islands, and from one resident at the village of St. Roque, near Cavité, I collected the mode of administration, dose, &c. He described them as being an excellent purgative, and he gave them in doses of from one to four, (the latter number being seldom exceeded) according to the age of the patient; one for the age of three or four years; two at ten or twelve; three at fifteen or eighteen years; and four as a full dose for an adult. The effects which result from an overdose are violent vomiting, purging, a burning sensation in the stomach and bowels, with a determination of blood to the head. The only antidote used by the native practitioners is repeated draughts of cold water; warm water, they observe, would be very injurious. When administered, the kernels of the nuts are extracted from their shells, and given in that form; but more usually by pounding them in a mortar, to which water is added, and after being well mixed together, it is strained, and given as a draught to the patient. It operates in a few hours after it has been taken. It was said to be an excellent purgative in venereal cases.

One afternoon, whilst gathering some of the seeds of this tree from a fence which surrounded a native dwelling in the vicinity of Cavité, I was accosted by a little Indian girl, of about eight years

of age, but possessing a large share of "secretiveness." She exercised that portion of her cerebrum in endeavouring to persuade me that the seeds I was collecting were very agreeable to eat. "Do taste them, señor, they are so very nice." I requested, as they were so agreeable, that she would eat one. She refused with a sly look, alleging as a reason, "that she had eaten so many yesterday, that she could eat no more this day." I at last told her that I was aware of the effects of the seeds, and was certain, that had she eaten so many yesterday, she would have been too ill to have eaten any this day. She laughed when she found her trickery discovered, and said it was true, that if she eat them she would have vomiting, purging, and perhaps die; but still, if I wished to know whether her statement was correct, I had better eat some; saying this she scampered away, leaving me to collect my specimens, and try the effects if I pleased.

I occasionally administered these seeds, but always found their effects (although in a recent state) very irregular among Europeans, some requiring a dose of from six to eight, others requiring only the usual dose of four; but in all an uneasy burning sensation in the bowels, with nausea and vomiting, followed. I took myself four of the seeds, and experienced a very unpleasant burning sensation in the stomach and bowels, with nausea, which after an interval of nearly two hours, terminated in vomiting; their purgative effects followed soon afterwards, and operated mildly; the sickness had then nearly passed away, but the burning sensation continued for some time longer.

Labillardiere states, that at Amboyna "most of the gardens were surrounded by shrubs, among which I remarked the *Jatropha curcas*, the plants of which grew so close together as to form good palisades: its seeds have the taste of a filbert, and are far from disagreeable. The natives cautioned us, that although eaten even in small quantities, they occasioned a *great drowsiness*; they were not aware that the narcotic quality of this fruit is seated in the part known to botanists by the name of the *embryo*. I had the satisfaction of informing them that after taking this out they might

* De Euphorbiacearum Generibus Medicisque, garumdem viribus tentamen, pages 93, 94, 410.

eat the kernel in perfect safety*." This is not a correct statement: the seeds of the tree are not possessed of any narcotic property. The effects intimated by the natives must be that illness would be produced by eating them: the active purgative and emetic principles being more powerfully in the embryo than in other portions of the seed, would, of course, by the removal of the embryo, be materially diminished, if not in the greatest degree lost.

At Chanti, on the coast of Sumatra, (which place I visited on the 29th of

November, 1830) I observed the fences formed of this tree, which was named *jarah* by the Malays. The castor-oil tree (*ricinus communis*) is named by them *miniak jarah*; and by the Javanese, *linga jarah*. Is the word *jarah* applied by the Malays to all seeds which have a purgative quality?

The accompanying drawing was made from a recently gathered specimen at Manilla. (Island of Luçonia).

(a) Represents a seed detached from the capsule.



In Hughes's Natural History of Barbadoes, it is named the physic-nut tree, (*ricinoides*); and he observes on the fruit, that its "outward tegument is green and husky; this being peeled off, discovers the nut, whose shell is black, and easily cracked; this contains an

almond-like kernel, divided into two parts; between this separation lie two milk-white, thin, membranaceous leaves, easily separable from each other, (embryo). These have not only a bare resemblance to perfect leaves, but have, in particular, every part of the stalk, the middle rib, and transverse ones, as visible as any leaf whatever. The kernel is

* Labillardiere's Voyage in Search of Perouse, vol. i. page 264, 8vo, edit.

not only edible, but has a delicious almond taste." He also observes, that "it is the common opinion, that the *purgative* or *emetic* quality lies in the two membranaceous leaves (embryo) that separate the kernel into two partitions; but this (he further observes) is a mistake, for I have, as well as others, tried it both ways, and its physical effects were with or without these, always the same." "The leaves and tender buds emit a milky juice, which, either by exposure to the sun, or being rubbed on the palm of the hand, turns to the consistence of a salve, which is looked upon to be very good to heal any green wound. The nut likewise, when ripe, pounded, and boiled, will yield a considerable quantity of oil. A spoonful of this, swallowed whilst fresh, is considered a good purge to abate swellings in dropsical disorders*."

Brown observes (Natural History of Jamaica, folio, 1789, page 348) on this tree, that "it is very common in all the sugar colonies, and cultivated frequently in inclosures. It grows sometimes to the height of seven or eight feet, or better, but dies after a few years. The leaves are much used in resolute baths and fomentations, and the seeds sometimes as a purgative; but they operate very violently, and therefore are now but little used."

Some croton oil which I administered from a ship's medicine chest had similar emetic effects, attended by a burning sensation in the stomach, bowels, &c. as the Tuva seeds. From that circumstance it may be inferred that an adulteration had been made with oil extracted from the seeds of the *Jatropha curcas*, which is, I believe, a frequent fraud in the croton oil of commerce.

London, Sept. 18, 1831.

POWER OF THE COMPANY OF APOTHECARIES OVER THE MEMBERS OF THE COLLEGE OF SURGEONS, &c.

To the Editor of the London Medical Gazette.

SIR,

CAN you inform your readers, who are interested in the subject, whether the act of 1825, 6 Geo. IV. which was

passed for *one year*, in order to amend the Apothecaries' act of 1815, has been re-enacted, or whether any provisions have been made relative to two points which that act contained?

1. Have the Apothecaries the power of refusing examination to any student who has not been apprenticed to one of their own body for five years? The act I allude to gave them power to examine students "who shall produce proof of having served an apprenticeship of not less than five years, to a member of the Royal College of Surgeons in London, or Edinburgh, or Dublin—or to a surgeon in his Majesty's army or navy; together with proof to the satisfaction of the Court of Examiners, of a sufficient medical education, and of good moral conduct; in like manner as by the said act is provided with regard to persons who have served an apprenticeship of not less than five years to an apothecary."

If the amendment of this act is not in force at the present time, it is unjust to a large body of men, as a great proportion of the general practitioners throughout the country have been in the army or navy, and have not been licensed by the Apothecaries, and all their apprentices, as well as those of the pure surgeons, are, by the act of 1825, excluded from examination, however competent they may be.

2. The same act of 1825, by the fourth clause, provided that all who held at that time, or should thereafter hold, commissions or warrants as surgeon, or assistant-surgeon, in his Majesty's navy, or as surgeon, or assistant-surgeon, or apothecary, in his Majesty's army, or as surgeon or assistant-surgeon in the service of the Honourable the East India Company, should be entitled to practise as apothecaries in any part of England or Wales, without having undergone any examination or received any certificate from the company of Apothecaries. Are any of these surgeons, after the excellent examinations they undergo at their respective Boards, obliged again to submit to examination before they can practise? Or do the surgeons in the army or navy receive as a courtesy what they did and ought to enjoy as a right, while the Company refuse even this courtesy to the surgeons in the East India Company's service?

These are points of some importance

* The Natural History of Barbadoes, by Rev. G. Hughes, folio, 1750, page 115.

to many of the members of the College, relative to which I know that many of your readers would be glad to receive information.

I am, Sir,
Your obedient servant,
A MEMBER OF THE COLLEGE OF
SURGEONS.

Oct. 5, 1831.

ANALYSES & NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abrégé."—D'ALEMBERT.

Reports of Medical Cases, selected with a view of illustrating the Symptoms and Cure of Diseases by a Reference to Morbid Anatomy. By RICHARD BRIGHT, M.D. F.R.S. &c. Lecturer on the Practice of Medicine, and one of the Physicians to Guy's Hospital. Vols. II. and III. £9. 9s.

THE great expense of these volumes will unavoidably preclude them from being very generally purchased, and for a knowledge of their valuable contents our professional brethren will be mainly dependent on the medical journals. In some of our early numbers we laid before our readers an account of Dr. Bright's first volume, and as those now before us fully maintain the character of their predecessor, it may be supposed that we are anxious to give an equally faithful digest of their contents. For this purpose we shall take up, in a succession of articles, the subjects of the second and third volumes, though not perhaps exactly in the order of their occurrence. We may remark, by the way, that our objection to the plates in the first volume, "that the author and the artists, in their anxiety to preserve the full force of that vivid and varied colouring which nature presents, have in one or two instances gone too far*" is here done away—the science of the anatomist, and the cunning of the draftsman, having united to produce a fidelity and chastened brilliancy in the delineations which are not exceeded in any plates which we have ever seen.

The arrangement is in some degree different in the present volumes from that adopted in the former; for in-

stance, instead of bringing together dissertations on particular diseases of different organs, the derangements of one individual organ are grouped together. Thus the second volume is almost entirely given to the brain and nervous system, the diseases of which are considered under the heads of INFLAMMATION, PRESSURE, and IRRITATION, according to the prevalent morbid condition; while with the second of these *Concussion* is associated; and with the third, a state to which the author gives the name of *Inanition*. The symptoms indicative of these several states of disease are thus described:—

"The symptoms of inflammation of the brain and its membranes are a general febrile condition, a quick and often a hard pulse, pain in the head, and sometimes in the limbs, sickness, convulsion, injected conjunctiva; sometimes simple confusion of ideas, at other times active delirium, with a quick though frequently faulty perception, sometimes intolerance of light and sound, sometimes a careless or wild contempt of all external objects or all ordinary sources of excitement; and all these symptoms suffer considerable modifications both from the situation and acuteness of the inflammation, and from the stage of its progress and the consequent changes which the parts are undergoing.

"The symptoms indicating the state I have designated pressure, are loss of voluntary power, from the most momentary, trivial, and partial, to the most fixed, general, and complete paralysis; and so likewise all degrees of depraved and impaired sensation, dullness of intellect, failure of memory, oppressed circulation, coma, sopor, apoplexy: but these symptoms of interrupted function probably arise likewise from other causes besides actual pressure, which it is not easy to appreciate.

"The symptoms of concussion are, loss of consciousness, sickness, headache; pulse irritable, often quick and irregular, or easily excited; pupils unequally dilated and contracted; and sometimes convulsion and delirium, or idiocy.

"The symptoms more particularly marking irritation in the brain are restlessness, discontent, screaming, agitation of manner, convulsion, and a pulse generally frequent, but variable both in frequency and strength.

"The general symptoms of inanition

* Gazette, vol. I. page 127.

are a pallid countenance, flaccid skin, and weak circulation, as marked by a feeble pulse with or without sharpness in the beat. The symptoms more immediately affecting the head and nervous system are, dull pain, sense of noise, indistinct vision, despondency of mind, irritability, wandering delirium, epileptic syncope, and coma.

"1. Inflammation of the brain and its membranes may be idiopathic, or the result of the febrile action excited in the system, as in fevers, whether continued, remittent, intermittent, or exanthematous, and in some inflammatory affections of other parts, as of the different viscera of the abdomen; or it may arise from the extension of inflammation, as in erysipelas, and in cases where inflammation has been set up within the cavity of the ear, and in cases of diseased scalp or cranium; or it may depend on some other diseased action going on in the system, and thus be considered metastatic, as in gout or rheumatism; or, again, it may be excited by external injury from violence.

"The appearances which result where the membranes chiefly suffer, and death takes place early, are undue vascularity in various degrees, unusual dryness of the membranes, and rather a deficiency of fluid in the ventricles, effusion of serum, often assuming a gelatinous appearance from being held in the fine tissue of the pia mater, unnatural adhesions between the membranes, and between these and the brain, the deposit of fibrin forming adventitious membranes, and the formation of pus, more particularly where the membranes have suffered from external violence. Inflammation of the substance of the brain may present increased vascularity pervading the whole or a part, and unusual adhesions between the brain and its membranes, or may go on to suppuration and the formation of ulcer or of abscess, and occasionally destroys the consistency of the brain without actual suppuration. But inflammation both of the brain and of its membranes will frequently destroy life by sudden exhaustion, leaving very doubtful proofs of the inflammatory nature of the disease.

II. Pressure of the brain may depend, and that even in its most urgent and sometimes fatal forms, simply on the state of the circulation within the head, as in cases of venous congestion giving rise to head-ache, vertigo, lethar-

gy, and even apoplexy. Disordered circulation may depend upon the quantity and the mode of distribution of the blood; which are the only derangements that can immediately produce pressure, and this deranged circulation may arise from local causes, or from some more general cause, as disease in the heart or the lungs: but the circulation may also be disordered in another way, by the morbid condition of the circulating fluid;—thus in cases of jaundice, diabetes, anasarca with renal affection, and ischuria renalis, and bronchitis, and in asphyxia, there is some reason to believe that, independently of the pressure which the brain suffers by the retarded circulation, the quality of the blood acting not alone on the brain, but on the nerves of the whole body, adds to its injurious effects; and it is possible that the peculiar loss of power consequent upon the external as well as the internal use of lead, may depend on a condition of the brain in which congestion is combined with a diminution of nervous energy, from the immediate sedative action of the lead.

"2. The rupture of vessels in the brain or spinal cord, and consequent effusion of blood, as in cases where single large vessels are ruptured, sometimes in consequence of disease, as aneurisms or ulceration; sometimes from accident; and in cases where several small vessels are ruptured, throwing out blood separately, and either coalescing into one clot, or forming many small lodgments of blood.

"3. Effusion of serum without obvious inflammation, as in those cases of fatal apoplexy where no other visible evidence of disease remains, but serum effused either into the ventricles or under the membranes of the brain: so likewise in chronic hydrocephalus, and in cases of great debility, as phthisis, or other diseases where the free transmission of blood through the lungs is prevented, as in cases of death from hanging or from exposure to intense cold. Serum is occasionally found in considerable quantity under the arachnoid, when slight drowsiness and wandering of intellect have been the only symptoms during life.

"4. The various results of common inflammation, as effusion of serum or pus, or the thickening of membranes, or the softening of portions of the brain, which acts by allowing pressure

to be made by one part falling unsupported on another, or possibly only produces symptoms of pressure, by interrupting the nervous communication.

"5. The results of peculiar diseased actions; as scrofula, carcinoma, fungous disease, melanosis, and osseous deposits, either in the cranium, on the membranes, or in the substance of the brain: to which may be added certain organic changes, producing partial or general induration, without being referable to any specific action. But in all these the symptoms of pressure are usually combined with those of irritation.

"III. With regard to concussion, the causes producing it appear so decidedly mechanical, that there is much reason to believe the symptoms depend upon the mechanical derangement. Frequently this is so slight, that the vessels of the brain being previously healthy, a few hours or days repair the injury; in other cases it is not so, and the lacerated fibres and vessels mark the injury after death. Perhaps the ultimate morbid condition on which the symptoms of concussion depend, arises from interruption to the circulation and to the natural communication between the fibres and particles composing the nervous tissue.

"IV. Irritation of the brain or its membranes may arise from disease within the cranium or from disease in distant parts; and although the irritating cause may frequently be the subject of anatomical demonstration, and the results of continued irritation may sometimes be shewn, yet the existence of this condition must chiefly, if not altogether, be traced in the symptoms which it produces.

"First: in some instances the irritation arises from causes within the cranium; as,

"1. The paroxysm in certain forms of mania, where the irritation appears to be connected with a peculiar and inexplicable irritability of the nervous structure itself; frequently, however, depending for the temporary increase of the habitually morbid state upon increased circulation, and in such cases giving evidence of the fact by the appearance of vascularity after death.

"2. Other kinds of fixed mania depending on the derangement of the mental functions from original or acquired conditions of the brain, respecting which we are quite ignorant, but which

are occasionally connected with appreciated organic changes.

"3. Paroxysms of epilepsy, chorea, or neuralgia, dependent on disorganization acting independently of pressure, and without producing the symptoms of that condition. As in cases where the disorganization has been slow, the brain having gradually acquired unusual hardness or softness, or become the seat of some tumor or morbid growth; each paroxysm in these cases generally depending on some change in the state of circulation, and sometimes on distant irritation, when the mischief within the brain may be considered as little more than a predisposing cause.

"4. Headaches, and various nervous feelings connected with a diseased condition of the vessels of the brain, as in some of the premonitory symptoms of apoplexy.

"Secondly: irritation of the brain may depend upon remote morbid agencies, or diseases going on in some distant part; as,

"1. The delirium in certain states and periods of fever from irritation of the abdominal viscera; and it is not quite obvious whether the delirium attendant on some acute cases of inflammatory disease, as peritonitis, should be classed with these or with the truly inflammatory affections; it probably commences in simple irritation, but inflammation is afterwards set up; and the same may be said of those cases of delirium, arising when the functions of the liver have been greatly deranged.

"2. Epilepsy and convulsion coming on at the commencement of eruptive and other fevers, or in cases of suppressed gout.

"3. Epilepsy and convulsion from worms or other irritation in the alimentary canal, or from teething.

"4. Hysteria from uterine sympathy.

"5. Choreia from uterine or other irritation.

"6. The peculiar tremulous and spasmodic action produced by inhaling the fumes of mercury.

"7. Tetanus and hydrophobia, possibly from the irritation of the nerves of the injured part.

"8. The effects of certain poisons, such as strychnia, which, according to the late ingenious experiments of Dr. Addison and Mr. Morgan, appear to act through the medium of the nervous system.

“9. To this division may likewise be referred those various affections of the head, as well as those mental aberrations which depend on the state of the stomach, sometimes shewing themselves in confirmed hypochondriasis.

“V. Inanition or deficient circulation is a very powerful cause of many derangements in which the brain and nervous system bear a prominent part: it shews itself in that general want of power which depends on an insufficient supply of nourishing and stimulating blood to the brain. It may arise from excessive depletion; in which case it may be but a temporary effect, having amongst its symptoms, intense headache, a sense of singing in the ears, deafness, confused vision, or total blindness, syncope and convulsion. If depletion have been carried further, or very frequently repeated, the effects may be more permanent, marked by paleness of countenance, by unusual effort of the heart, throbbing at the temples, headache, confirmed lassitude, despondency, and all this occasionally terminating in convulsion or coma. The same state may arise from insufficient nourishment, or from a deficiency in the supply of wholesome air or exercise. It may also arise from internal causes weakening the power of generating blood, and giving birth to all the symptoms connected with chlorosis and *anæmia*.

“Examination after death proves the remarkable deficiency of red particles which has existed in the blood during life; and the effused serous fluid which is discovered both in the ventricles and under the arachnoid, often affords a satisfactory explanation of the symptoms of pressure which have immediately preceded death.”

INFLAMMATION.

The first section is devoted to inflammation, and contains illustrations of the various forms of *arachnitis*, *phrenitis*, and of softening of the brain. In the present notice our space will not admit of our attempting more than to illustrate

Arachnitis and Delirium Tremens.

Of late years attempts have been made to carry diagnosis to a pitch of excellence which it had not previously attained; and it cannot be denied that the improvements in this respect have been prodigious. Perhaps, however,

though much has been attempted, less comparatively has been accomplished with regard to the head than other regions of the body, and those who study symptoms in the sick chamber, and morbid anatomy in the dead house, will be the most ready to suspect that the attempt to fix with mathematical precision the exact and individual spot in which inflammation has existed, or fluid will be found within the cranium, is but among the refinements of the enthusiast. While Dr. Bright admits that the symptoms are to a considerable extent modified by the situation as well as degree of inflammation, and from the precise changes which the part is undergoing, he attempts no fine drawn, or bewildering subdivisions, but gives his description of inflammation of the contents of the cranium generally.

The manner in which cerebral inflammation makes its attack varies much in different instances, being sometimes sudden, and very speedily running its fatal course; at other times being slow and gradual, stealing on with a stealthy and unsuspected pace. Various cases are given in illustration, but we must confine ourselves to those in which some modification existed, so as to divert attention from the actual seat of disease, or otherwise render them deserving of particular notice.

“Arachnitis, with excessive Irritability, in an intemperate Man.”

“Thomas Surnian, aged 35, was admitted into Guy’s Hospital, November 27, 1827. During the last ten years he has worked at a distillery, and has drunk freely both of porter and spirits. He has always had good health, with the exception of a general tremor in the morning after he has been intoxicated. Five days ago he received a blow on the upper part of the neck, which occasioned only a feeling of stiffness; and as he felt ill on the next day, he took aperient medicine, and rubbed a liniment on his neck. After this time he had only a slight headache, and appeared to be doing very well till the day before his admission, when, without further obvious cause, about three o’clock he became delirious, talking incoherently, but was not violent. At seven o’clock p.m. he was bled in the recumbent posture to a pint and a half, when he became faint; six leeches were applied to his temples; a blister to the nape of his neck; and vinegar as a lotion to his head: since this he has been more quiet, though delirious. At the time of admission there was a general tremor; a hurried man-

ner, constant motion of the eyes; he fancied he saw objects before him, and occasionally picked the bed-clothes; his answers, though quick, appeared rational; a slight flush on the cheeks; no headache. Pulse 98, rather sharp, but very compressible. Skin hot and perspirable. Bowels open the day before.

Hiæbeat Hydrag. Submur. gr. v. statim, et Olei Ricini, ℥ss. post horas quatuor; Applicetur Embrocatio communis capiti raso.

"He became so violent in the afternoon as to require restraint. His bowels were moderately open; and about nine o'clock, having immediately before been singing, he fell suddenly into a state of collapse, and, although stimulants were as freely administered as possible, he died in a quarter of an hour.

"When moving the body on the following morning, a considerable flow of blood from the mouth was observed.

"*Sectio Cadaveris.*—This was performed fifteen hours after death. The sinuses of the dura mater contained much fluid blood. There was a small quantity of serum underneath the arachnoid. The veins of the pia mater were large, as if habitually distended; but the flow of blood which had taken place from the longitudinal sinus on first removing the calvaria, had apparently emptied them. The pia mater peeled off naturally and exposed healthy convolutions; it was a matter of doubt whether the bloody points in the substance of the brain could be considered more than perfectly natural. About two drachms of fluid in the ventricles, and nearly an ounce about the basis of the brain.

"The lower and posterior part of both lungs, especially of the left, presented an appearance as if blood had found its way into the cells, being of a dark red colour, and in some parts having a greenish tinge, and even a putrid odour, which could scarcely be ascribed to decomposition in so short a time after death; some old adhesions on the right side. The heart was very flabby, and there was an ecchymosis on the left ventricle, just below the semilunar valves of the aorta.

"The liver large, of a drab colour, interspersed with yellow mottling. Ducts of gall-bladder pervious, and bile good. Spleen healthy, but adherent to the diaphragm.

"Kidneys, particularly the left, pale and flabby. Intestines generally distended; and an arborescent vascularity on the mucous membrane of the stomach."

This case illustrates that peculiar morbid condition commonly known to us by the name of delirium tremens; wherein the inflammatory action is so modified by constitutional irritability as to render a very guarded, if not apparently contradictory treatment, imperatively ne-

cessary. In the present instance the local injury can scarcely be regarded as sufficient to account for the fatal attack, which was probably entirely dependent on the previous habits of the patient. The extreme irritability, the absence of headache, the weak pulse, and perspiring skin, would seem to have indicated the use of opium, and this we are told was to have been adopted after the bowels were sufficiently opened had it not been for the sudden collapse by which the patient was meantime cut off. This is just the mode in which we have repeatedly seen such cases come to a sudden end, the patients sinking after even a moderate bleeding, and dying in a few hours. The effects of an opposite mode of treatment are strikingly shewn in the following case:—

"*Arachnitis, with excessive Irritability, in an intemperate man.*

"In the year 1823 I was requested to see a gentleman, residing at a distant part of the city, who had unfortunately contracted habits of intemperance, and had become suddenly delirious the day before. Leeches had been applied to his temples; and his delirium, so far from being relieved, seemed to be greatly aggravated, so that he passed the night in most violent agitation, requiring the strength of two or three persons to restrain him; and on one occasion he had nearly leaped out of the window. Such still continued to be his state when the message came to me; but when I arrived two or three hours afterwards, a most striking alteration had taken place; for on his expressing a strong desire in his delirium for a mutton-chop and some porter, the medical man who was attending him thought it not improbable that it might do him good to have some solid food on his stomach, and at once granted his request;—the effect was almost instantaneous. His mind became calm and collected; and when I saw him, little but general nervous agitation and a hurried manner of speaking remained; and a few doses of opium with calomel was all that I saw occasion to recommend. He afterwards told us, that the state of his mind during his delirium was most peculiar; it seemed, as he expressed himself, as if he were two separate persons, for he knew those who were around him, but still he went through a kind of clear connected dream, in which he was tried for murder, and condemned; and when on the point of leaping from the window, it was in his attempt to escape from the officers of justice.

"It is certainly very difficult in a case like this to recognise the existence of any actual inflammation; yet the predisposing causes of this attack, the character it assumed, and the treatment to which it yielded,

bore every mark of being immediately allied to those inflammatory affections, coupled with excessive irritability, of which we are speaking, and which sometimes terminate almost as suddenly in death as this did in recovery."

The consideration of such facts leads to a reasonable doubt as to the nature of this morbid condition being really inflammatory; at the same time it is unquestionably allied to inflammatory affections. In other instances, indeed, a state of irritability is evidently co-existent with acute inflammation within the head. The eighth case, unfortunately too long for extraction, is an example of this kind. A woman, of intemperate habits, had violent delirium, a wild eye, sharp pulse, and furred tongue, but without pain in the head, at least she denied having any. She was bled twice, and the blood was much buffed the first time and partially so the second. Dr. Bright, from the circumstances of the case, feared to employ farther depletion, and therefore gave calomel and hyoseyamus in large doses.

As soon as the mouth got sore, (which was not till she had taken nearly a hundred grains of the latter, and quite that of the former) all her symptoms disappeared.

"A cautious review of the foregoing cases cannot fail to impress upon the mind the very important peculiarities attendant upon inflammation of the membranes of the brain, in all those spontaneous cases which arise so frequently where the nervous irritability has been greatly increased by intemperance or distress of mind. It cannot certainly be laid down as a rule, that in such cases bleeding must never be had recourse to; but that it should be adopted with much caution cannot be doubted, and it should be immediately followed by the administration of opiates combined with calomel. The very early use of tonics, and even stimulants, will in many cases be advisable; and improvement in the diet, more particularly the substitution of some solid food, together with a limited quantity of malt liquor, instead of slops, will be beneficial; and in all cases the strictest attention must be paid to the removal of every source of mental disturbance and excitation; and with this view, all restraint, except such as is absolutely necessary for the safety of the patient and his attendants, should be avoided. The head should be constantly

cooled by evaporating washes, and the atmosphere should be kept as cool and as pure as possible. The calomel, in conjunction with more or less opium or hyoseyamus, according to the degree of irritability, should be continued till the symptoms have completely subsided, or the mouth is affected.

Maingault's Illustrations of the different Amputations performed on the Human Body. Represented by Plates, designed after Nature, with Alterations and Practical Observations. By WILLIAM SANDS COX, Lecturer on Anatomy and Surgery at the Birmingham School of Medicine, and Surgeon to the General Dispensary. Longman and Co. London.

WHEN we look over the delineations of anatomical subjects in times past, (mere caricatures, if published at a moderate price, or if well executed, their price so high as to limit their sale, and therefore their usefulness, to a small number of persons,) and then turn to some of those offered to the public at the present day, combining the advantages of fidelity and cheapness, we must acknowledge the benefit which anatomy has derived from the introduction of lithography. The work before us is intended to facilitate the performance of amputations, not by giving minute or lengthened descriptions, but by "reducing to a small number the general rules to be observed in their performance; by representing the different modes of operating, by lithographic engravings designed after nature, the form and extent of the wounds—the principal points to be observed, and by enumerating every circumstance with the greatest simplicity."

The delineations are so arranged, that the member amputated is placed beside the same in its natural state, with a view to indicate clearly the processes of bone corresponding with the line of the incisions; and the precise point at which the operation should be performed is marked by dotted lines, drawn over the surface, so as to indicate the extent and direction of the incisions. The author remarks, that the perusal of his work will recall to the surgeon the memory of facts which time may have effaced; and the student, without the aid of a teacher, will be able to com-

prehend and perform the different operations in the dissecting-room.

This useful publication has not been merely "done into English," as our fathers used to say. Mr. Cox did not take upon himself the office of a mere translator; he has made alterations and additions wherever they appeared necessary, and with the more confidence as "he has been in the habit of demonstrating them to his surgical class."

The value of the work, in its English garb, is thus very much enhanced; so that we have no hesitation in recommending it to the student and the young surgeon.

MEDICAL GAZETTE.

Saturday, October 8, 1831.

"Licet omnibus, licet etiam mihi, dignitatem *Ar-tis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

THE CONTROVERSY ON THE DECLINE OF SCIENCE IN ENGLAND.

WHEN Mr. Babbage, supported by an able writer in the *Quarterly Review*, undertook, some months ago, to assign the *causes* of the neglected state, the positive decline of science in these kingdoms, we must confess we were considerably dazzled by the splendour, and carried away by the weight, of the great authorities which we found engaged in the inquiry. Mr. Herschel, the highest name we now have in the list of our men of science, seems to have begun the complaint: he grumbled, chiefly indeed about the neglect of certain novel investigations in chemistry. "Who can tell us here (in England)," said he, "any thing about the sulpho-salts; or of the laws of isomorphism? Who among us has verified Thenard's experiments on the oxygenated acids; or Oerstadt's and Berzelius's on the radicals of the earth; or Balard's and

Serullas' on the combinations of brome, &c.?" Hence, by rather a questionable species of argument, he drew sweeping conclusions unfavourable to the state of English science generally, and, what was still more serious, drew after him, into the same conclusions, a number of eminent persons, who all deemed the authority which they followed paramount, and the opinions which he dictated incontrovertible. Mr. Babbage was first in his wake, and put the *argumentum ad verecundiam* so strongly in the work to which we have alluded*, that though "great names were among us," no man could assume to himself the high privilege of playing the champion in his own and his country's behalf. The example of such high authorities, voluntarily coming forward to maintain their own inferiority to their continental brethren, or, perforce of certain modest warnings of acquiescence, remaining silent, though not content under the imputed degradation, had almost the efficacy of an axiom with the public; and the fact of the decline of science amongst us was vulgarly considered to be as unquestionable as are the existence and increase of our national debt. All eyes were directed to the discovery of the *causes* of this supposed fact, and many and ingenious were the immediate antecedents pointed out by distinguished inquirers. Dr. Brewster, who is supposed to have written the article in the *Quarterly Review* to which we drew the attention of our readers soon after it was published, and who, by the identity of opinions expressed in his late biography of Sir Isaac Newton, would seem to warrant that supposition, has summed them up succinctly, under three or four heads; among which we recollect "the ignorance and supineness of government" was one, and the "indirect

* Remarks on the Decline of Science in England.

persecution of our men of science, by their exclusion from all honours of the state," was another. Now it is curious enough, that the defence of our national character, from the disgraceful imputation of the decline of science, has come from a quarter, whence, of all others, it could least have been anticipated :—

“—*Vin prima salutis,*

Quod minime reris, Graia pandetur ab urbe.”

If a prophet were to divine to us, a month ago, that from Holland was to come the first gleam of our redemption from the stigma which was nearly impressed upon us by consent, we would not have believed him—we would have repudiated him as a meddling fool; yet, without being either an *Œdipus* or an idiot, his word might have been taken for good; his veracity would have proved fully borne out, and Mr. Faraday would have come forward to put in lawful bail for him. In short, here is an honest Dutchman come to judgment; and as we have hitherto had an opportunity of stating but one side of the question, chiefly owing to the modesty, or rather the unaccountable want of nationality, which seems to have befallen our men of science on this side of the German Ocean, we shall take leave to hear this learned foreigner's decision, and detail some of its more important points and leading features for our own and our readers' edification.

A pamphlet has recently been published, bearing for its title, “*On the Alleged Decline of Science in England; by a Foreigner.*” It was professedly sent to the press by Mr. Faraday; and we must honestly avow, that our first impression upon its falling into our hands, and even after a hasty first perusal, was, that its ostensible foster-father was its real parent. Nothing so natural, we thought, as for a man like Mr. Faraday, so well acquainted with our intellectual resources, and himself

occupying so distinguished a place among the first chemists of the age, to make use of a little *ruse* of this sort—to throw over himself the modest disguise of a “foreigner”—and to enter the lists boldly against Mr. Babbage, and “all manner of folk.” But our hypothesis, unfortunately, was not doomed to be long lived; the author of such a pamphlet could not long remain concealed. Inquiry was set on foot with unremitting activity; and the foreigner now all as good as “stands confest*.”

The usual course of proceeding in demolishing an antagonist's arguments about matters of facts, is, as is well known, to *remove* the cause, after which the consequence follows, as it naturally should. Our “foreigner” has not deemed so much formality necessary: he comes at the *effectus* at once; and, with extraordinary valour, declares it to be a nonentity, or at best, but a phantom—a breath—a mere assertion. “Before we can follow Mr. Babbage,” says he, “in his long list of complaints, we must pause a moment in the consideration of the assertion, that ‘science is declining in England.’ This harsh sentence, however, admits of several interpretations, and Mr. Babbage has not informed us which is that which he adopts. Is it his opinion that science is stationary in England, whilst it is making rapid strides on the continent? or does he wish to give us to understand that really a retrograde motion takes place in England; and that although, upon the whole, science is more widely diffused at present in England than formerly, there is a lack of scientific men of the first eminence able to be put upon a par with the most renowned foreigners? Mr. Babbage must excuse me for believing, that it is not sufficient thus confidently to assert the inferiority of his own country; he ought, in my opinion at least, to have pointed out dis-

* He is Dr. Moll, we understand, of Utrecht.

tinctly where the inferiority exists. Are only some branches of science affected, or does it spread widely over all the departments of human knowledge? It may be, that England should be found deficient in some particular instance, whilst in others, perhaps, it far surpasses other nations. The scale of merit ought to be carefully handled, to determine to which side the balance is leaning. But if the real meaning of Mr. Babbage, on a question in which the national honour is so deeply involved, is labouring under an obscurity which we have no means to dispel, we may attempt at least to canvass the authority by which he supports his opinion. The names of Sir Humphry Davy and of Mr. Herschel are, of course, of the highest authority; but it would appear rather strange that any one should attempt to couple *those* names with a complaint of a decay of science. This sounds pretty much as if, when speaking of Wellington and Nelson, one would argue on the inferiority of the British army and navy. The mere mentioning of the names of Davy and Herschel would furnish a proof that science *was* flourishing in that country which gave birth to these highly-gifted individuals."

We regret that our space will only permit us to allude to the arguments, whereby he shews that if, as a writer in the *Encyclopædia Metropolitana* asserts, our chemists neglected their chemistry in consequence of Oerstadt's discoveries, it was only a temporary suspension of their labours in one branch of science that took place, in order that they might be directed to another. And what redounds more, in his opinion, to the credit of English chemists is, that *they* took a leading part in these new investigations. Dr. Wollaston and Mr. Faraday made their several discoveries in electro-magnetism; while, amongst the first promoters of that novel branch of

science, we do *not* find Berzelius, Thénard, Gay Lussac, and other great chemists of the continent.

But we must give some of our author's excellent remarks on the alleged want of *efficient patronage* of scientific men in England:—

"Sir Humphry Davy complains of the little interest which noblemen take in science, and of the necessity which compels many persons, inclined to scientific research, to look for other means of sustenance; but both the evils of which Sir Humphry complains do not belong to England alone. Nowhere noblemen care much for science, and pecuniary wants deter many all over Europe from pursuing a scientific career. No great penetration is wanted to see merit struggling with want and poverty in every country in the world; and, perhaps, it might be said, that science has a better chance in England than elsewhere, of securing an honest independence to its possessor. Dr. Wollaston, by his scientific exertions, procured himself that pecuniary supply, without which the greatest genius and the ignorant are alike unable to support themselves. Now, I will tell Mr. Babbage, that in no other country of Europe could Dr. Wollaston, unassisted by kingly favour, have been able to earn by his scientific discoveries, that independence which gave him the necessary leisure to apply all his mental force to the pursuits of science. I know countries where the high-minded Wollaston might have been obliged to fawn and bow in the anteroom of some lawyer in office, where his discoveries would have been submitted to the examination and criticism of some official underling; of having his writings and experiments appreciated by those who neither can conduct an experiment, nor weigh the force of an argument; and, after submitting to all these indignities—after having felt the full weight of the insolence of office—he might have had the humiliation to see preferred to his just claims, the unceasing importunity and the shameless effrontery of the impudent quack, and of the subservient sycophant. This, I will tell Mr. Babbage, is the real state of scientific men in those countries, whose manner of managing scientific concerns he affects

to rate so much above that of his own country, and *nisi exempla essent odiosa*, we might bring convincing proofs of the truth of our assertion from the scientific history of almost every country in Europe."

Our limits will not allow us to lay before our readers the masterly exposure of the fallacy maintained by Mr. Babbage and his backers, that either the interests of science or society at large are likely to be promoted, by calling upon abstract philosophers to discharge the duties of officers of state. The following calculations, however, of the comparative pecuniary exertions of France and England in behalf of science, incidentally introduced, are too striking to be passed over:—

"A very material difference between the Royal Society and the Institute consists in this, that the fellows of the former pay for their admission, whilst the members of the latter enjoy a pension. Now, there are at present 685 fellows of the Royal Society who used to pay about 4l. a-year for the honour of their fellowship, making in all an aggregate sum of 2740l. or 68,5000 francs. The members of the French Institute receive, or received formerly, each 1500 francs, making 10,250 francs of the public money. Now, we will ask Mr. Babbage, in which country the honour of belonging to the first scientific society is held in greater estimation—in that in which the fellows pay the honour with 68,000 francs, or there where they receive 10,250 francs, and the honour as an addition into the bargain?"

"But although France has a population of thirty-two millions, and England of twenty-two, and notwithstanding the high, flourishing state of science in the former country, we would undertake to insure the utter impossibility of finding seven hundred persons in France willing and capable of contributing for the benefit of science, and for the honour of belonging to a scientific body, an annual sum of 3000l.

"Mr. Babbage favours us with a table of the costs of fellowship of different learned societies; it would have been very curious if he had stated the annual sum thus contributed both by

the English and French nation towards the encouragement and benefit of science, and from the result a fair estimate might be drawn, in which of the two countries science is held in the greatest honour. We are not at all afraid that, in this case, the odds would be in favour of England. To imagine that there are six hundred Frenchmen amongst thirty-two millions willing and capable to pay 50l. or 1250 francs, in order to become members of a learned society, appears to us an idea so very ludicrous, that we cannot help smiling at the very thought."

In another number we shall resume this subject, and direct our attention more particularly to the concerns of *medical science*.

DISTINCTIONS CONFERRED ON MEN OF SCIENCE.

THE honour of being made Knights Commanders of the Guelphic Order, has been conferred on the following scientific men:—Messrs. C. Bell, Herschel, Babbage, Brewster, Ivory, and Leslie. The same honour was lately conferred on Dr. Chermiside, of Paris, long attached to the British Embassy in France.

PROGRESS OF THE CHOLERA.

JUST as we were going to press, we received the following intelligence on the engrossing subject of cholera. Accounts from Vienna reached London on the 4th instant, bringing our information down to the 17th ultimo. It appears that cholera is ascertained to have existed in that city so early as August 31st, but only in solitary instances, and apparently unconnected with contagion. On the 14th, ten persons died out of forty-one who were attacked; while on the 15th, of one hundred and thirty-nine attacked, sixty-four died. On the 16th there were one hundred and twenty-seven new cases, and fifty-eight deaths,

and one hundred and seventy remaining ill. These returns, however, only include the hospital patients, and we lament to say that many others, and those, too, among the higher classes, and whose names are well known at Vienna, have already fallen victims to the disease.—None of the remedies tried have been of any use—some have been decidedly pernicious, especially bleeding.

The death of one of the physicians, who had been indefatigable in his exertions, is mentioned by our informant with particular regret; and, in fact, an official account of the circumstances connected with it has been published. He was attacked with slight sickness on the 6th; on the 7th, unequivocal symptoms of the disease manifested themselves, and he died the same evening. A gentleman residing in the same house, and who had been out shooting all day, ignorant of his friend's illness, was attacked with diarrhœa within an hour after his return home; on the 10th the characteristic symptoms of the malady appeared, and he died on the 11th. A boy, son of the preceding, was seized at three o'clock in the morning of the 12th, and was dead at three in the afternoon. On the 13th, a child, 22 months old, was taken ill, and died the same day; and, on the 14th, a maid-servant, 18 years of age, was also seized with cholera, but was likely to recover.

By letters from Memel to the 20th of September, it appears, that between the 16th and 19th only two new cases had occurred, and one death. The proportion altogether has been 49 cases, and 26 deaths, in every thousand of the inhabitants—that is, of the cases reported; but it is known that here, as elsewhere, many persons had the disease without notice being given to the public authorities.

AN INTRODUCTORY EXTRAORDINARY!

To the Editor of the London Medical Gazette.

Tavistock, Covent Garden,
Oct. 3, 1831.

SIR,

I AM a country practitioner, settled for some few years in the neighbourhood of —, in Yorkshire; but I am not so settled as to have lost all my old town associations. I have still my yearnings after the resources of the metropolis; and particularly about this period every year I make it a point to be in London. A good introductory lecture is my chief delight; and that alone, if I can find one announced from some man of eminence, or to be delivered upon some important occasion, is always a sufficient inducement to bring me up to town. My old preceptor, "glorious John," is no more; and Sir Astley has retired from the business of lecturing, much to my disappointment. Yet, since I last heard either of these, I have listened to Charles Bell and to Mr. Lawrence with satisfaction undiminished. The occasion upon which I heard Mr. Bell was one of peculiar interest; it was at the opening of the London University in 1828; and I think I shall never lose the favourable impression left upon my mind of the bright prospects of that institution by the eloquence of the learned professor. My feelings were enlisted in what I was taught to conceive the "good cause" of supporting the University; and not even all the unseemly proceedings which have since occurred in that establishment—not all the changes and chances that have come upon its career, the dismissals, resignations, the breaches of discipline, and the disgraceful deficiencies of the governing body—can entirely wean me from the interest which I take in it, and which I can only refer to the imposing effect of the said introductory lecture. I have ever since been attentive to the progress (if I may call it so) of the London University, and have taken every opportunity of making myself acquainted with its arrangements; but I have never been able to attend on any of the annual opening days since the one just mentioned, until this very day, when, attracted not by the name of the individual who was to deliver the address (though I probably did expect

something from a person so permitted to take precedence), but by the peculiar circumstances under which I knew the establishment was about to open for the session, I took care that nothing should prevent me from being present. And such a scene as I have witnessed! Pained as I am by the general impression which it has left upon me, I would not have missed the absurd pleasantry of it for a great deal; nor would I wish the readers of the Medical Gazette, who were not present, to be deprived of all knowledge of this singular exhibition. Sir, I keep a regular journal or log-book, of such curious occurrences as befall me from time to time; and here are what I consider to be materials for a leaf or two. If the communication of them be acceptable to you, you are, I can assure you, most welcome to them, and I shall be at the same time much gratified by seeing them in print; so here they are in their original rough state, just as they were written upon my return to my lodgings:—

Oct. 3, 1831, *London University*.—The Professor of Midwifery of this place has just delivered himself of an opening lecture; and a troublesome job the poor man has had of it. In coming into the world the babe was little better than still-born; it was a poor, starved, sickly creature—any thing but creditable to the doctor and patient.—The gossips were astonished at the throes and efforts which were made in the delivery, and the constant recourse which was had to cordials by the accoucheur ever and anon. But allegory apart, I have heard one of the flattest, and, under the circumstances, one of the most stale and unprofitable introductory lectures from Dr. Davis this day, that I ever remember to have been an ear and eye-witness to. What a silly thing of his brother professors to allow *him* to open! The school of medicine in this establishment is understood to be remodelled and re-cast; great things are evidently expected from it, and I was glad to see so large an assemblage gathered in the theatre, to hear what the medical faculty had to say in its own behalf. Dr. Davis I had never heard lecture; but I expected much from the man who was permitted to stand in the breach in the time of doubt and difficulty. At length, in the “dab-chick waddled” (I could not help recollecting Pope’s line), and after a

few preliminary adjustments of putting on his spectacles, drawing forth his MS. &c. opened his mouth and began. He had an ample field before him, wherein to discuss the objects and advantages of a University established in a great metropolis; but it seemed more his wish to account for the absence of *religious* forms of education in this place, and to shew how the London University was open to all sects and denominations alike—“to Jew and Gentile, to Christian, Mahometan, and Hindoo.” He was pleased to be severe upon the old English universities, and alluded in terms, which he no doubt conceived to be pointed and pregnant, to the dangers of subscription; and clenched his argument upon the horrible consequences of perjury, with a long rignarole Latin quotation about the nature of an oath. The poor man was by this time in a pitiable state of exhaustion, and required recruiting. *He swallowed a glass of wine*. I have often before seen public lecturers have recourse to a glass of water for refreshment sake on the occasion of some great delivery, but this was the first time I ever saw the water-bottle literally set aside, and a decanter of wine, with a glass, introduced in presence of a large audience. But the midwife is the best judge; glass after glass was regularly thrown off after each heavy paragraph was delivered, and the audience seemed to feel for the jolly Professor’s necessities by looking round at each other, on the quaffing of every bumper, and venting their sympathies in a half-suppressed titter. I was particularly amused at one time, by observing his eye wander for a moment, in the midst of a long passage, from the paper to the wine—the lecturer’s lips smacked audibly—and the hand instinctively moved to the bottle, which it actually half-inverted! It is needless to say, if the Professor was given to perspiration before he touched the wine, in what a state he was when finishing the lecture. But to proceed:—Things went on flatly—very flatly—after all. No symptom of applause was elicited for the first half-hour—no well-turned phrase, or judicious sentiment, invited the approbation of the audience; and probably so it would have gone on to the end of the chapter, had not Professor Pattison’s dismissal been alluded to as “a change that would prove bene-

ficial." This drew from some score of juvenile auditors, who seemed waiting for the signal, a distinct peal of hand clapping; but there was nothing of the same sort demonstrated for the remainder of the lecture. And, by the way, speaking of Mr. Pattison, how strangely did the lecturer acquit himself! He positively spoke of the ex-professor as a distinguished anatomist, and one endeared to him by habits of intimate private friendship—yet he threw his intimate friend—his distinguished and able friend, overboard, in acquiescence with the publicly-condemned decision of the Council! The proceedings of the Council and the Professors he illustrated by the novel and original image of the going of "a piece of *mechanical machinery*." "Some," said the lecturer, "are of opinion that we cannot go on." (Here he took a glass of wine.) "Cannot we indeed?—Wait a few days!" It would be impossible to describe on paper the *sotto voce* point with which the doctor uttered these last two phrases; they were so irresistibly comic. But when he came to speak of the new arrangements, and the actual state of the school, much of course was expected: any person, however unskilled in lecturing, could have made here a hit for himself, and done some credit to his brethren of the faculty: but poor Dr. Davis did his best, and that best was—nothing. Hear his eloquent panegyric on Dr. Elliotson. After mentioning that this gentleman had been newly elected to the chair of Practical Medicine, the lecturer added, with inimitable grace, "He is *rather a young man*,—but he is an old lecturer and an old physician!" Little said is soon mended. Of Dr. Turner he only ventured to say, "He is very assiduous to his pupils."!! But Professor Lindley is immortalized by the niche he got along with the Lecturer himself in this eloquent prelection:—"Mr. Lindley is the Professor of Botany. I don't well know what to say about *him* (a laugh.) Whatever remnants of botany I ever had, I have lost them all; indeed, *I never had more than was sufficient to save me from rejection when I went in for my diploma!*" Good God! what a confession to proceed from the occupant of a medical chair! and a chair, too, in the University of London! What an encouraging example to be set before the pupils in a medical school!

Nor was the man in the slightest degree ashamed to make this confession before a public audience:—I wonder how he will like to see it in print.

Enough of this most unpleasant affair. I am grieved from my soul for the paltry predicament in which the University is placed by such an exhibition as this—not a soul who was present, I am bold to say, will forget it as long as he lives. I felt for the professors, who were face to face with the ungracious opening-lecturer, and hung their heads abashed at his foolish display. Often have I heard of "damning with faint praise," but never did I see the operation of sinking with laborious imbecility performed before. How unfortunate (though I should hope it may be otherwise) for the business of the establishment, with all its new arrangements, to have received its first impulse from such a hand? The opening lecturer, indeed, said something about this duty having been allotted to him in the order of the *rota*. When I recollected the auspicious first movement of that *rota*—a movement to which I have already alluded—and compared it with what I now saw, I could not help quoting the interrogatory of the poet:—

—————"Amphora cæpit
Institui: *currente rotâ cur urceus** exit?"

Sir, you now have the leaf from my log-book, which I promised to give you. Make what use of it you please, and believe me ever

Most obediently yours,
RUSTICUS.

LECTURES
ON
THE THEORY AND PRACTICE OF
MEDICINE;

Delivered at the London University,

By DR. ELLIOTSON.

— — —
Introductory Lecture—Oct. 5, 1831.

In appearing for the first time as Professor of the Principles and Practice of Medicine

* *Urceus*—vas angustum et ventricosum, cadi fere formâ.—VER. SCHOL.

in this University, it is impossible for me to enter at once upon the subject which I am destined to teach.

The novelty of seeing myself in another institution than that in which I have hitherto lectured, and in which it is still my happiness to attempt rendering myself useful to the rising generation of my profession by practical example and illustration, while I increase my own knowledge and endeavour to add to the general stock—the novelty of teaching in the first and only medical school founded in England upon the full and extensive plan of the celebrated and systematic schools of the continent and of Scotland—the novelty of teaching in a medical school constituting a department of an University, and that University established in the great city of the empire, in the greatest city of Europe, fresh and young, and therefore inexperienced, and sometimes possibly injudicious, but unencumbered by monkish habits and antique fashions, bent not down to support any one tribe or party, or sect, however dominant, nor proud and unkind enough to turn its back on any, but standing erect in all the beauty of a young and noble form, destined to a full growth and mighty strength, and a development of endless powers and excellencies; smiling and holding out its hand to all, of whatsoever sect or party, or tribe, or language, or nation; and intending to bestow upon the meritorious of all sects, sorts, and conditions, and upon the meritorious only, not upon those who have the accidental distinction of birth as well as upon the meritorious, those honours which it must one day have the privilege of bestowing; willing itself to learn, anxious to repeat no errors, and having the improvement and welfare only of mankind at heart;—all these circumstances produce feelings and reflections to which I must give utterance.

That I should have discontinued to lecture on the Practice of Medicine in St. Thomas's Hospital, may be a matter of surprise, and even appear to require explanation, to those who know the struggle which I made successively with a series of my predecessors, to obtain permission for the physicians to lecture within its walls, as the surgeons, and even the surgeons of an adjoining hospital in co-partnership with them, actually did; who know the dark persecution which, on account of my efforts, I underwent—the secret insinuations of incompetency and bad spirit—the attempt to remove me from the hospital after some years of gratuitous service, unless I would sign a promise that, although forbidden, like all the physicians, to lecture within the walls, I would not lecture even anywhere without; who also know, that, after all this, the extorted promise was torn to pieces, through the virtuous feelings of a general meeting of the Governors, and a

message sent me officially that I was free to lecture wherever I thought proper; that a regular medical school was next established, in which, up to the present moment, I took a principal part—which still flourishes, and long, I trust, will flourish—and that the medical officers are now allowed to attach themselves to any other schools they desire, the principle being acted upon by the liberal committee and governors, that the physicians and surgeons are elected to the charity, for which only the funds were given, and not to the school; and who know, moreover, that I not only had the satisfaction, at length, of seeing these results, but am on the happiest terms with all the officers, and all my colleagues, who are men of peaceful minds and high gentleman-like feelings, and that those whose ears were formerly poisoned are now my most cordial friends.

I determined upon the resignation of the lectureship of the Practice of Medicine before any idea was entertained that this chair would be vacant in the University; and even in the summer of 1830 I actually resigned, and consented to lecture last winter only at the solicitation of my principal colleagues. The great distance of the hospital from my residence had now become so serious an inconvenience, on account of the increase of my private practice, that I found it impossible to go more frequently to the institution than to make my clinical visits. I was merely a joint lecturer, and I fancied that, while I remained thus yoked, great success was impossible. But one single circumstance made my resignation a duty. I discovered that my colleague, with whom I am on the very best terms, and who is a most amiable man, in the complete course, which, according to our arrangement, he delivered during the season, and I in mine, upon the same subject, inculcated both principles and practice diametrically opposite upon almost every subject; that, to use his own expression, “we must have differed *toto cælo* both as physiologists, pathologists, and therapeutists, in many most essential points of doctrine and practice.”

To arrest teaching so unprofitable, so perplexing, so disgusting to the student—to arrest so ridiculous an exhibition, required no consideration. It was necessary for one to withdraw, and, as the junior, I felt it my duty to be the one.

Thus disengaged, I heard that this chair was vacant, and I instantly desired to occupy a post so honourable—a post affording so ample an opportunity of disseminating a knowledge of my profession. The plan of all the other medical schools of the metropolis has always appeared to me far too limited, and the instruction imparted to be of necessity superficial. Through the want of an university in London, medicine and surgery were long scarcely taught at all, notwith-

standing the great means presented by the population and the hospitals. The student was compelled to travel to France, Holland, or Italy, or, in later times, to Germany or Scotland, for the purpose of acquiring a true professional education; our own two Universities neglecting almost entirely to teach medicine, whether from the want of anatomical and clinical means, the hopelessness of rivalling the great continental and Scottish schools, or the idea that their proper end was to teach general knowledge, to impart only what all educated persons should know, or from the circumstances mentioned in a powerful and remarkable article in the last number of the *Edinburgh Review*, upon the Universities of England,—The ascendancy of the Colleges over the University—the subversion of the professional system by the tutorial—since (says the reviewer) time was when the Colleges did not exist, and the University was there; and were the Colleges again abolished, the University would remain entire; and when the University was paramount, the cycle of instruction was distributed among a body of teachers, all professedly chosen from merit, and each concentrating all his ability on a single object, and the whole youth of the University of equal standing, daily collected into large classes under the same professor;—whereas the colleges and halls are now elevated into so many little universities, into which houses the students are distributed, with little regard to ability or standing, among some fifty tutors, who are chosen from the fellows, notwithstanding that the fellowships were not founded for the purposes of teaching, and the fellow rarely owes his eligibility to merit alone, but to some fortuitous circumstances also, in the majority of cases.

The opportunities of teaching anatomy, surgery, and medicine, however, in London, became at length too great to be always thrown away; and the surgeons of the hospitals began to deliver lectures on anatomy and surgery, and afterwards the physicians upon medicine. These lectures were of limited extent, nor were systems of courses, for a time, delivered so as to constitute regular schools. The time, the trouble, and the expense of travelling to an University of medical reputation, rendered the emolument fairly to be expected on establishment in practice too high for the mass of the people to afford; and as the population increased, and more practitioners were demanded, home medical instruction was sought after by greater numbers every year—and this the more, as any one could practise without the license or authority of any University.

The lectures improved, and at length regular schools were established; yet, as they were all private, and could confer no privileges or honours, and those who re-

quired degrees went consequently to the old celebrated schools, while those whose only resource was the private schools had too small pecuniary means to devote much above a year to public study, these metropolitan schools remained scanty and superficial. The whole of anatomy and physiology, and morbid anatomy together, were disposed of in a course of little more than three months; the practice of medicine, and the science of chemistry, each in the same period, with only a lecture of one hour's duration three times a week; much of general pathology and forensic medicine was altogether omitted; and the whole of medical police, and the literature of the profession, were passed over; and things remain still in the same state, except that courses on forensic medicine are now about to be delivered.

It was left for this University to be bold enough to establish courses of six months' duration, in which full scope would be afforded the professors of laying before the student the whole body of medical science, to add the charms of varied illustration, to inculcate important points earnestly and repeatedly, and to introduce the student to an acquaintance with medical literature. These opportunities I not only did not enjoy in my former school, but could never hope to obtain; and I gladly attached myself to this new school of medicine (loud applause).

The mode of instruction by lecture, though by no means sufficient of itself, is in my mind of high importance. When a whole subject can be taught in a solitary course of lectures, and the exhibition of nothing is required, so that a mere delivery of statement constitutes the whole task of the professor, there can be no doubt that a good work, containing all the same information, might be studied in private with equal benefit; and, indeed, with this advantage, that it would probably, after costing less, remain in the possession of the student, to be consulted by him whenever he thinks fit. But when a circle of instruction is required,—when the subjects are numerous, and demand many courses of lectures and many professors, the student cannot be committed to himself. Without lectures he would become almost to a certainty bewildered, study at irregular intervals, and wander from one subject to another, getting little fruit from any; whereas the attendance on various lectures, at stated hours, creates an excitement and interest, as well as a regularity of habit, which are of incalculable utility.

In our science and art, however, lectures are, on another account, of superior advantage—are indeed indispensable. One part of the office of every medical lecturer is to describe objects of sense. The attempt to learn anatomy, physiology, chemistry, materia medica, or botany, would be ridiculous, without the inspection of the material

objects, and the principal phenomena. Of the practice of medicine and the practice of surgery, the same is true. Neither the professor of the practice of physic, nor the professor of surgery, can give his pupils a full idea of the phenomena of diseases, or of the administration of remedial means, without patients exhibiting the various phenomena of disease, and subjected to the exercise of our art. As cases, however, cannot be had at pleasure, so as regularly to illustrate a systematic course of lectures, it is indispensable to deliver the general description and history of diseases and their treatment in a distinct course, and to afford the illustrations from life in a totally separate manner, as they incidentally present themselves in a collection of patients: it is indispensable that there should be both a general course and clinical instruction. Yet, in the general course, material illustration is both possible and necessary. Representations of the various morbid changes by copious and well-executed drawings, engravings, and models, and actual specimens of these, and all the material products of disease, it is the duty of a teacher to present and explain; although, when I was a student in Edinburgh, the professor of physic never exhibited a single drawing, nor a morbid specimen. In these respects the University affords me great advantages. The museum is excellent, and with the assistance of the profession, may become unrivalled. And here I entreat the profession to contribute to it,—to preserve what morbid specimens they can, and deposit with us; remembering that we have no hospital to supply us, and must depend entirely upon individual contribution; remembering, likewise, that our museum is of indescribably greater utility than any others unconnected with a school;—that, while those are open only to the inspection of the profession, not merely is this open to the profession, but every preparation in it is laid distinctly before the students, and employed by the professors as a means of instruction: I therefore entreat the practitioners of this vast metropolis to render it, as they readily can, and at no expense, one of the first pathological museums in Europe. The drawings possessed by the University are, I believe, unrivalled,—at least I have no where else seen such a collection. Their accuracy and beauty, their scale, and their numbers, are the admiration of all who have seen them, and reflect such credit upon Dr. Carswell that he requires no panegyric of mine; and when the short period in which they were executed is considered, his industry becomes equally a subject of admiration with his talents. In the other great mode of instruction in the practice of medicine and surgery the University fails; and our school can never deserve the name of a great medical school while the defect exists:—a

hospital is imperiously necessary. It is true that we have a dispensary; but a dispensary must always be a miserable substitute to the student for a hospital. No medical university would grant the degree of doctor in medicine upon any who had frequented merely a dispensary. At a dispensary the majority of patients are so little indisposed, as to be able to go about. When seen, they necessarily pass in rapid review before the practitioner, giving him an opportunity of showing how practice may be dispatched, rather than how disease should be investigated: for, if the time demanded for the scientific and patient investigation of each case, and for demonstration and explanation to the pupil, were given, the poor creatures would be compelled to lose far more time than their condition in life or their strength would allow in attending at the charity. There is, besides, no certainty among the patients of a dispensary that medicine is regularly taken; no possibility of fixing their diet; no certainty of their continuing to attend, so that each case may be regularly followed up to its conclusion; no possibility of making daily observations. When cases are severe, and the patients confined at home, there is not only the same uncertainty of strict attention to the injunctions respecting medicines and diet, but the student must lose a great deal of time in running from one house to another; and the physician or surgeon will not visit all such patients daily with his pupils; and, indeed, his pupils cannot accompany him in these visits with any degree of regularity. In an hospital the patients are compelled to obey every injunction; being under the same roof they may be seen at pleasure any hour of the day, without loss of time, so that long and repeated observation of their cases is easy. When death occurs to a dispensary patient, and an examination is permitted, the student loses still more time; for he not only must go to the house of the family, but prepare the body, and afterwards restate it; and, in all probability, will seldom be accompanied by the physician or surgeon. At hospitals the examination takes place without any loss of time; the student has no where to go; servants prepare, and sew up, and wash the body; and the physician or surgeon usually superintends the inspection. So superior are the advantages of hospitals, that, as I have already remarked, certificates of attendance upon them are required at all medical Universities previously to examination for the doctorate; and although certificates of attendance at dispensaries are admitted by the Apothecaries' Company of London, and may be obtained at a cheaper rate than those of hospitals, the majority of English medical students voluntarily enter themselves to the medical practice of hospitals. The inferiority of dispensaries is, indeed, acknowledged

by their medical officers in the greater cheapness of their terms; and the Apothecaries' Company declare it in their demand of a longer attendance at them than at hospitals. It is, therefore, much to be lamented that the Apothecaries' Company admits dispensary attendance at all; for if, of the short period enjoyed by the medical pupil for the public study of his profession, a certain portion is necessarily devoted to witnessing cases, that portion should evidently be spent in witnessing them in what is acknowledged to be the most efficient manner, and if eighteen months can be spared for witnessing dispensary practice, they can be spared for witnessing hospital practice. Those who cannot see the necessity of hospital attendance, for the purpose of witnessing, without any loss of time, the most important diseases, and their minute investigation, and accurate and unrestrained treatment by the physician, cannot but admit the necessity of hospital attendance for the purpose of witnessing the great accidents and operations of surgery. The College of Surgeons can receive no certificates of attendance upon the surgical practice of dispensaries. If, therefore, the student must attend a hospital for half his experience, it is indisputably best that he should acquire the other half within the same walls. In speaking these, my sincere convictions, I feel some delicacy, because I am a hospital physician. But those who know me will, I am sure, acquit me of any interested motive, and, in truth, the medical clinic of St. Thomas's is frequented to the utmost of our desire, and I am not likely to have it in my power very long to remain attached to that great Institution. I will not scruple to add, that I think the student pays too much for hospital attendance in London, that the entrance should not be to medical or surgical practice alone, but to the whole range of the hospital, and that the sum which is paid to witness the surgical practice only would be a sufficient remuneration for the surgeons and physicians together; and I should rejoice to see the most meritorious among the senior students rewarded with the appointments of dressers to the surgeons, as well as of clinical assistants to the physicians, gratuitously. The last argument I will urge in favour of hospitals is, that they are also dispensaries; that they have a dispensary department, in which patients to the same amount are attended as at mere dispensaries. Besides all the inmates of St. Thomas's, the greater part of ten thousand out-patients are attended; and, although none are visited at home, this might readily be done without any farther trouble or expense, by the senior and best-informed students, to whom such a trust would be an incalculable benefit.

It is thus certain that the pupils of this

university must frequent hospitals for the purposes of surgery, and that many will frequent them also—and I think all ought to frequent them—for the purposes of physic; that they must be indebted to other practitioners than the professors—to others who are unconnected with the University, and possibly are lecturers in other schools, for a most important part of their education; that this cannot be celebrated as a medical school—that it cannot be said to afford a perfect medical education, till it possesses a hospital. Without a hospital, the Professors, it must be added, who teach the practical part of the profession, not only are deprived of the means of properly instructing their pupils, but lose the great advantage of proving to them the truth of their statements and the propriety of their precepts. One great source of my pleasure in teaching at St. Thomas's Hospital was the opportunity of shewing the pupils that I practised what I inculcated—of rendering probable, or proving to them, that what I advanced was true, and of referring from time to time for illustrations of my descriptions to cases which they had witnessed with me in the institution.

For the greater part of these important objects a small hospital would be sufficient—a hundred medical and a hundred surgical beds. This number of well selected cases, assiduously studied, would answer all ordinary purposes as well as the 458 beds of St. Thomas's, one half of the cases in which are imperfectly, or altogether unknown, to the pupils; and a hospital of this extent might, I am inclined to believe, be conducted at less expense than is generally imagined. But although such a limited extent might be prudent, and even necessary at first, I trust that the students of this University will one day enjoy the benefits of a large hospital—that the plan of the charity will be such as to admit of the greatest extension. For an important advantage is inseparable from a large hospital; not that of affording a large number of examples of the same affections, but of giving a probability of the occurrence of the more rare, nay, indeed, of the rarest diseases, accidents, and operations, during the period every student frequents it. Besides the opportunity of becoming fully acquainted with all the ordinary cases of medicine and surgery, it is indispensable to a full and perfect education that instances of every disease and accident which he may afterwards have to treat, should be witnessed by the student. Not only does the most elaborate description of a disease, or mechanical derangement, fall short of conveying an adequate idea, but the conception gradually fades away, or, if retained, is frequently not recalled when what was described chances to occur after a lapse of time; and probably not till another person

names the nature of the affection, do we see that it is what we formerly heard of; or if we suspect its nature, we still have our doubts. On the other hand, if diseases or accidents have once been seen, *oculis subjecta fidelibus*, when they again present themselves, they are immediately recognized. A striking exemplification of these remarks came under my observation when a student. A poor man, labouring under true scurvy, applied to a surgeon on account of the horrid state of his mouth, his gums being swollen and spongy, and bleeding, his teeth loose, and his breath offensive. The surgeon, not having seen a case of scurvy, supposed the disease of the gums arose from a bad state of the teeth, and extracted several in succession. He was then sent to another, of high eminence and enormous practice, who pronounced it a case of fungus hamatodes of the gums, and admitted him into his hospital, intending to resign him to his fate. Being visited, however, by a practitioner who had witnessed scurvy at a naval hospital, the nature of his disease was at once recognized: some lemon-juice, and fresh meat, and vegetables, were prescribed, and he was well in a week or two. The surgeons, who were not aware of the nature of the complaint, were well-informed men, and the disease was one which all have heard of, and the leading symptoms of which are universally known. But though formerly very prevalent and fatal in London, it now is uncommon, and from the want of having witnessed it, these two gentlemen thus committed a serious error. It is of great importance to the student that he should have witnessed also all the rarer accidents, and the rarer surgical operations. For all this a large hospital is indispensable, and I trust that we shall one day see a hospital connected with this institution, containing lying-in wards, for the purpose of an obstetrical clinique, and wards for the reception of mental diseases. If we but make a beginning, I feel confident that the public will support us. For to say nothing of the advantages of an hospital to the University, one is absolutely wanted in this situation. The eastern extremity of the metropolis, and the southern side, have the two great Borough hospitals, St. Bartholomew's and the London. The western extremity has St. George's and the Westminster. But between the east and west, on the northern side, there is merely the small Middlesex Hospital, and in this immediate most populous neighbourhood none at all; so that not only it, but the adjoining county, has not the advantages of any other parts within or around the metropolis. Were our University not in existence, the benevolent, on having these circumstances pointed out, would come forward; and if once a fund were established, legacies would be left as to

other institutions, which have already more than they can advantageously spend; and when the benevolent see that by their support they would do more than further the ends of charity—that they would, at the same time, contribute to supplying their country with a succession of highly-informed practitioners, the double incentive, I am certain, would realize our most sanguine hopes.

While I rejoice in the establishment of an English school of medicine, in a city, and on a plan, which will render it, if well conducted, equal to any, and far superior to most others, in the civilized world, I rejoice also that this school is a department of an university. Nothing is more to be desired, than that those who go forth into the world to practise the healing art should be men of excellent education. In most countries of the continent this is the case. The great body of practitioners there receive a general education at an university, previously to commencing the study of their profession. They in truth receive what should be the education of a physician, and are physicians. In this country those who carry on the great mass of practice—those who, in addition to their original occupation of apothecary, have risen to the office, though not to the name, of physicians—are indebted chiefly to their own exertions for even their professional knowledge and skill. Not only have all the lectures, excepting perhaps those on anatomy and surgery, been too scanty and superficial, and the clinical instruction conducted too generally in a most careless manner, so that clinical lectures were actually unknown in London a few years ago, and an extremely small number of those whose chief business in life was to practise medicine ever thought of entering to the medical practice of an hospital, or of even a dispensary; and no such systematic method of practical instruction, whether medical or surgical, as is proper—no organization of the students with more or less charge of patients, is yet established; but no better education is given by parents to those children intended for medical practitioners, than to those whom they destine for trade; and at the time when the best part of general education should begin, the youth, with all his capabilities of literary and philosophical attainment, is hurried off to mix draughts and weigh powders the greater part of the day, for several years. The portion of time thus consumed, and the sum of money thus expended, would enable the youth of our profession to attain every acquirement of a high education. The young men who come to our schools to prepare themselves for general practitioners are, in the majority of instances, as gentleman-like in their sentiments and manners, as intelligent, as anxious to learn every thing that their instructors are disposed to teach them, as those who are sent to Oxford or Cam-

bridge—as young men of whatever station in society, and in the previous enjoyment of whatever advantages. I trust that ere long the five years' apprenticeship to learn the business of the dispenser, or, as that might be learned in as many months, I should rather say, the five years' apprenticeship to waste their time as shopmen, will be abolished; and instead of the present preposterous regulation, of five long years in the shop before a limb is dissected—before a lecture on anatomy, chemistry, or any other subject is heard, and two years only in systematic public study, in dissecting and attending so many lectures, and in gaining the experience afforded by the medical and surgical wards of hospitals, and in reading, which previously would have been fruitless, the youth will, through the medium of such establishments as the London University, acquire a good classical education, a knowledge of the three great continental languages—the French, the German, and the Italian—and no inconsiderable acquaintance with mathematics, physics, and mental philosophy. By a change of plan, and by practicable facilities, I am confident that all this may be accomplished with no great increase of expense, and the young practitioner be ready for the commencement of his active career at the age of two or three and twenty; and no one will deny, that a given expenditure of money and of time ought to be made to produce the greatest possible advantages. These facilities will all be amply furnished by this University; and I hope that the Apothecaries' Company will one day be better pleased that those young gentlemen who go before them should adduce testimonials of a respectable classical and mathematical education, a capability of reading the German, French, and Italian writers in the originals, and the possession of a share of physics and mental philosophy, than if these young gentlemen proved they had consumed five years, when the mind thirsts most ardently for knowledge, in drug-mixing, or such poor and desultory instruction as a private practitioner has time to bestow, and which must be as nothing, before they begin to learn anatomy, physiology, and chemistry, systematically and practically at a public school.

An eminent scientific professor at Utrecht, in a pamphlet, just published in English by Mr. Faraday, in answer to Mr. Babbage's complaint that science is on the decline among us, says, "There are countries in Europe where no young man could think of studying medicine, mathematics, or natural science, with the help of Latin only, and without being prepared, before entering the University, with a sufficient knowledge of German, English, and French. The ignorance of foreign science in England cannot be attributed to want of zeal, but to a defect

in English education—the ignorance of foreign languages, which it was perhaps the business of the London University to amend." I believe that all the philosophers of the continent hailed the foundation of the London University with delight, as an institution which would spread among the general youth of England a far more useful education, and far more liberal principles, than the two old Universities of England have considered it their business to furnish to their clerical and more aristocratical alumni, and in which every science would be fully taught.

If I am told that this improvement in education will make the general practitioner equal in knowledge to the physician, I reply, that this will rejoice me beyond measure. I have no desire that the importance of physicians should be maintained by the depression of the general practitioner. If the physician is to continue as superior to the general practitioner as he formerly was, he must advance in the same proportion—he must not presume to think of preventing the general practitioner from stepping towards him—he must also step forwards himself, to maintain his advantage. The physician, whose means have allowed him to spend more time than the general practitioner in his literary and scientific education, in the subsequent study of his profession, and has spent it industriously; and who, when in practice, and exempt from the toils of midwifery and the other distractions which beset the general practitioner, and having not the whole of the art, but one branch only to cultivate, employs these advantages as his sense of duty must dictate, will always acquire every reasonable advantage, and general practitioners will gladly avail themselves of his assistance, and will be proud of him as a supporter of the character of the profession. The mass of the middle classes of society has begun a great movement in the acquisition of knowledge, in the discovery of rights, in the correction of absurdities, and in virtue and liberality of sentiment. The higher orders see the impossibility of arresting this progress, and titles and decorations will no longer command even notice, unless supported by real excellence. Among the higher ranks there are some, who, so far from viewing this movement of other ranks with dread, step forward to promote it; and the share taken by some of exalted rank and office in the foundation of this University, demonstrates a liberality and generosity which will ever do honour to England, and is worthy of all imitation. So ought physicians to act towards the general practitioner;—to make every effort to improve his general education and his professional knowledge. In fact, all those who lecture or practise at public institutions, act, whether they will or not, contrary to the narrow views of interest; for

they are occupied in imparting knowledge to the rising generation of general practitioners—occupied in endeavouring to make them as learned as themselves.

In me such narrow views would be the height of baseness, and would be the height of ingratitude ; for I hesitate not to avow—I rejoice in this public opportunity of declaring it—that to the general practitioners of England, Scotland, and Ireland, I am indebted for my success in my profession. When I commenced my professional career, I determined upon seeking for success by working hard, and by conducting myself as well as the infirmity of human nature would allow. I determined, however long I might wait for success, never to fawn upon and run after my superiors, nor to stoop meanly to my inferiors : never to intrigue for an advantage, nor to employ trumpery artifices for making myself known to the public. For many years I toiled, and saw most of my contemporaries, nay, of my juniors, who worked less, but were wiser in their generation, pass by me. I published work after work—edition after edition—and paper after paper was honoured with a place in the Transactions of the first medical society in Europe. I was physician to a large metropolitan hospital, and had attended there, and gratuitously out of doors, above 20,000 patients. But in vain. In 1828 my profession was no more lucrative to me, was as short of my actual expenses, as it had been in 1818. At that time the *Lancet* was pleased now and then to publish a clinical lecture delivered by me at St. Thomas's, and my practice at once doubled. The following year it published the greater part as I delivered them, and my practice doubled again. Last season the *Lancet* published them all ; the *Medical Gazette* followed its example, and my practice has now doubled a third time. This astonished me, the more as my clinical lectures were generally delivered with little or no premeditation, while all that I published myself had cost me great labour, many a headache, and much midnight oil. It was through the general practitioners, in the large majority of instances, and through general practitioners, for the most part, with whom I had not the honour of any acquaintance, that the publication of those lectures accomplished my success. To the body of general practitioners, therefore, I owe a debt of gratitude ; they have called me forth spontaneously from no interested motive, and I cannot exert myself too much in the education of their successors.

Conscious though I am that my lectures will not be without defects, I still can promise diligence, and the omission of no anxious care. The pupils will never find me maintaining a professorial distance, but always bearing myself as their friend, and only

as a student of longer standing than themselves. I thus have always conducted myself towards the students of St. Thomas's, and never met with rudeness, or even with neglect ; but, on the contrary, have ever experienced the most affectionate respect. In the ancient statutes of Oxford it was enjoined, that " after lecture the professors should tarry for some time in the schools ; and, if any scholar or auditor may wish to argue against what may have been delivered from the chair, or may otherwise have any dubiety to resolve, they should listen to him kindly, and satisfy his difficulties and doubts." In the spirit of this good rule I shall always act, and, in season, and out of season, not only during lecture but after lecture, and in my own house, whenever it may be desired, the student will ever find me ready to impart any information, and give him any counsel in my power. (*Reiterated applause.*)

EXTRACTS FROM JOURNALS,

Foreign and Domestic.

DIFFERENCE IN THE EMPLOYMENT OF BLEEDING IN ENGLAND AND FRANCE.

BLOODLETTING is a remedy very differently employed in England, from what it is on the Continent. In this country, if a strong person be attacked by acute inflammation of an important organ, a full bleeding, from fourteen to twenty ounces, generally succeeds in checking the progress of the disease, and in many cases the necessity of its repetition is obviated ; in those cases where the inflammation is not subdued, the bleeding is repeated, the quantity of blood abstracted being proportioned to the violence of the disease and strength of the patient ; this, together with the use of purgatives, antimonials, &c. procures the speedy recovery of the patient in an immense majority of cases.

On the Continent, when bloodletting is employed, the quantity of blood abstracted at the outset very rarely exceeds ten ounces : this, in a large proportion of cases, is not sufficient to arrest effectually the progress of the inflammation ; hence the more frequent necessity of further depletion, which is effected either by the abstraction of a smaller quantity of blood from the system, or more commonly by the repeated application of leeches ; the patient at the same time taking an acidulated beverage, or a decoction of simple herbs, with, perhaps, an occasional dose of cas-

tor oil; so that although the continental patient may lose as much or more blood in the aggregate than the English patient, it is less calculated to produce the desired effect; while the neglect of the means of producing increased secretion from the bowels necessitates the more frequent abstraction of blood, and the patient, in the favourable cases, has to support a tedious convalescence.

LARREY'S METHOD OF TREATING COMPOUND FRACTURES.

Barron Larrey treats compound fractures of the leg in the following manner, and, as he states, with very successful results; although, from its not being more generally adopted, it may be inferred that the practice is not so successful in the hands of others. A junk is made with a piece of strong linen, and of pieces of straw bound firmly together, as in the London hospitals; on this are placed transversely three broad pieces of bandage; the limb is then placed on the junk, the foot resting on the heel; the Baron then covers the wound with a piece of rag, with holes cut in it, (*linge perforé*,) to allow the escape of any matter, which is absorbed by a quantity of charpie placed over the rag. A compress, wetted with a lotion composed of the white of three eggs, oil camphorated spirit, and oil. *lotionis plumbi*, is then put over the whole; the transverse bandage, wetted with the lotion, is bound firmly round the limb, the straw splints are approximated and tied with tape, as in a common junk; the limb is then left to itself, and the apparatus never removed or opened, on any account, till the expiration of sixty days, when the cure will be found to be effected.

ABSCESS OF THE LUNG, POINTING ABOVE THE CLAVICLE.

A lad, aged 18 years, was lately admitted at the Hospital of St. Louis, who had frequently been affected with cough during the winter. About eighteen months previous to his admission, he had become affected with pain of the back and under the left-shoulder blade. Five or six months after this, a tumor appeared on the left side of the neck, for which he went to the Hotel Dieu: this was in September 1830, when M. Breschet punctured the swelling and evacuated some pus. On his admission

at St. Louis, a fistulous opening was discovered above the clavicle. When the patient coughed, purulent matter was ejected by the aperture. The discharge, after a short time, became fetid, and the strength of the current of air through the opening was sufficient to extinguish a candle. He died June 6th. On post-mortem examination, a very large cavity was found partially filled with pus and communicating with the bronchiæ, as well as with the external surface. Several ribs were carious.—*Revue Médicale*.

LEUCORRŒA TREATED WITH SCALE CORNUTUM.

Dr. Bazzoni has recently detailed some cases in which he used the ergot with apparent advantage. About a scruple of the medicine was infused in eight ounces of water, of which one-half was taken the first, and the other half next day, in divided doses. Sometimes this sufficed to cure the disease, but if not, it was repeated.—*Annali Universali*.

REPORTS OF CASES OCCURRING AT PUBLIC INSTITUTIONS.

LONDON HOSPITAL.

Vesico-Vaginal Fistula cured by Operation.

ISABELLA PIGOT, ætät 28, a healthy married woman, admitted December 2, 1830, with a fistulous communication between the bladder and vagina. She stated that four months since, after a protracted labour of eighty-four hours, during which the bladder had not been relieved, she was delivered of a still-born child at the full period. Her recovery was speedy; but from the time of her confinement she had been unable to retain her urine, the constant discharge of which occasioned excoriation and great distress. On examination per vaginam, an opening sufficiently large to admit the point of a forefinger was discovered communicating with the bladder, at its neck, and apparently also involving a small portion of the commencement of the urethra. The opening was of an oval form, with its long diameter lying transversely about $1\frac{1}{2}$ inch from the orifice of the meatus urinarius. Although the flow of urine was constant, the retentive power of the bladder was not wholly destroyed, for during the examination several ounces of urine which had accumulated in it, escaped. The edges of the aperture were callous only to a slight extent, and circumstances seemed

favourable to obtaining a closure by operation.

Dec. 29th.—She was brought into the operating theatre and placed on the table, and the hands were fastened to the feet as in lithotomy. Mr. Luke introduced a silver female catheter into the bladder, through the meatus urinarius. Weiss's two-pronged dilator for the female urethra was passed per vaginam, and opened to its full extent; this exposed the fistulous orifice. Tenacula were attempted to be passed through its sides, but were found to be too little curved. Two hooks were therefore used; their points were introduced about an eighth of an inch from the internal side of the opening, and brought out at the same distance on the external; the sides were next pared off by two elliptical incisions made transversely in the vagina. To make the internal it was necessary to use a scalpel, with one-half of the blade bent at a right angle with the other. The whole of the preceding parts of the operation were performed with the utmost facility. There was some difficulty experienced in passing the ligatures for closing the wound. At first small curved needles were used, held in a *porte-aiguille*, but without success. Mr. Luke then tried larger ones, less curved, and succeeded in passing one on each side of the urethra, at a little distance from the edge of the wound, which when tightened perfectly closed the orifice. The dilator was withdrawn, and an elastic catheter was fastened in the urethra. She was sent to bed, and ordered to lie on her face, and allow the urine to drain on a cloth.

January 1st.—From the time of the operation no water had passed by the wound until to-day, when it was found that the ligatures had ulcerated from one side, and the edges of the wound were so far separated as to allow the escape of the whole of the urine.

3d.—All the urine passes by the orifice, which is larger than before the operation. The ligatures were removed, but the catheter was allowed to remain.

6th.—The elastic catheter was found to be stopped, and was withdrawn. Another ligature was passed in the centre of the opening, including more of the surrounding substance than before, which closed it completely. The silver flat catheter was fastened in, and she was placed on her face.

11th.—No water has passed by the wound, which appears closed. The ligatures produced ulceration and were removed, but the wound still adhered. A clean catheter was introduced.

15th.—The catheter had become stopped and the wound re-opened, and it is now nearly of the original size.

March 19th.—She submitted to another operation. Mr. Luke pared the edges of the opening by carrying a single hook through its sides and excising them with a common scalpel; a single ligature was also sufficient to keep them in contact. She was again

directed to lie on her face, with a catheter fastened in the bladder, through which the urine drained.

26th.—The ligature had produced ulceration, and was therefore removed. The sides of the opening were adherent, and all the urine flowed by the catheter, which was allowed to remain in the bladder, and the patient was directed to retain the same position.

April 9th.—The catheter was removed, and the fistulous orifice was perfectly closed.

After the removal of the catheter, there continued a *stillicidium urinæ* from the urethra; as, however, the parts regained their tone, this subsided.

25.h.—She was discharged cured.

BANDAGES AND SURGICAL APPARATUS.

Our attention has been directed to a course of practical demonstrations on surgery, which, we think, is calculated to be of considerable use to pupils. Mr. Chapman proposes exhibiting the various bandages, apparatus for fractures, dislocations, &c. as well as various instruments, comprising the stomach-pump, and such-like contrivances, in addition to those employed in operative surgery. The proposed plan is to give the pupil an opportunity of applying the apparatus, &c. on the living body.

BOOKS RECEIVED FOR REVIEW.

A Brief Sketch of the Progress of Opinion upon the Subject of Contagion; with some Remarks on Quarantine. By William Mac-michael, M.D., F.R.S. Fellow of the Royal College of Physicians, &c. &c. &c.

Elements of Surgery. By Robert Liston, Fellow of the Royal Colleges of Surgeons in London and Edinburgh; Surgeon to the Royal Infirmary; Senior Surgeon to the Royal Dispensary for the City and County of Edinburgh; Lecturer on Surgery, &c. &c. Part Second.

A Singular Case of Extensive Thickening of the Abdominal Integuments; dedicated to John Abernethy, Esq. F.R.S. late President of the Royal College of Surgeons in London. By Patrick Darbey, Surgeon, Member of the Royal College of Surgeons in London; Licentiate in Pharmacy, *Materia Medica*, and Pharmaceutical Chemistry, of Dublin; and in Midwifery, of the Lying-in Hospital. Second Edition.

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SATURDAY, OCTOBER 15, 1831.

LECTURES

ON

THE THEORY AND PRACTICE OF
MEDICINE;

Delivered at the London University,

By DR. ELLIOTSON.

LECTURE II.—Oct. 6, 1831.

General Observations — Circumstances which produce Disease—Explanation of Terms—Acute and Chronic—Active and Passive—Definitions of Disease—Different senses of the word Cause in medical language.

It appears, gentlemen, to be a law of the universe, that every thing shall undergo incessant change. Whether the universe be eternal or not; whether we believe it has existed, according to the opinions of some ancient philosophers, from eternity, and is to exist to eternity; or whether we are so to understand Scripture, as that not only the arrangement of all things, but all things themselves, had a beginning, and must have an end; yet while the universe is in existence, it appears to be the law of the Creator that every thing in it shall undergo incessant change.

The material and the animate world are in the same predicament. The most minute aggregate of matter, and the largest material bodies, appear only destined for certain periods of continuation in their existing state. We see with respect to our own earth, that countries are continually lessened and destroyed by the encroachments of the sea and by eruptions; and on the other hand, from similar circumstances, other countries are produced; so that it is probable that the appearance of the earth at present is totally different from what it once was, and that after a period it will be totally different from what it now is. Even with respect to other worlds, there is every reason for astronomers to believe, that many have been destroyed, and that some worlds which

are now seen are but fragments of others which have been broken to pieces.

With respect to the animated part of the creation, every vegetable, every animal, has but a limited period destined for its existence. This period is exceedingly various; some animals are destined to live but a few hours, others again would appear to live for centuries.

But besides this definite period, every animated being is liable to be cut off long before its natural period has arrived. Animals and vegetables are all exposed to violence of every description, not only to mechanical violence, but to the destructive operation of ten thousand causes—ten thousand circumstances; and to disturbance from the cessation of the circumstances which are necessary to their existence. One animal is destined to devour another while in its prime; nay, to destroy the element of what would be the young—to destroy the eggs, for example, of another; and myriads of insects are continually destroyed in storms. Constantly, therefore, the limit which providence appears to have placed to every thing is curtailed—every animal and vegetable is subject to be cut short long before that limit is reached; so that there appears to be a perpetual struggle throughout nature—the Creator having, on the one hand, invested every animated being, whether vegetable or animal, with a power to exist for a certain time, and power to resist injurious agencies; and having provided, on the other hand, injurious agencies, without number, to injure and to cut them off before their time.

It has been said by some philosophers, that to look at man one would suppose that he was destined to live to all eternity; that, at first sight, such a machine as the human body, unless destroyed by external violence, would appear capable of lasting for ever, and it is by experience alone that we are taught the contrary. *Primo statim aspectu talis machina, nisi forte vi externa corrupta, in omne ævum durabilis videtur; et quidem solâ experientiâ contrarium docemur.*

However, there is nothing in the appearance of the human machine which would lead us to suppose that it could last for ever. Whatever we know of nature can only be learned from experience, and it would be as correct to say that because it is summer we should believe it will be summer for ever, or because it is day we should believe it will be day for ever, unless we have contrary experience, as it would be to say, that the human machine in itself appears capable of lasting for ever. We can learn nothing from taking a partial view of things—it is impossible to form a judgment of nature except by taking a continuous and enlarged view. It would also be contrary to all analogy to suppose that the human machine is capable of lasting to eternity.

With respect to the injurious circumstances which are positive, and to which animals are exposed, and by which they may be cut off before their full period has arrived, they are those in the first place of mechanical violence, and likewise those of a specific nature. Many animals and many vegetables contain within them materials which are destructive to the life of others.

They are liable to be destroyed, also, from a want of suitable external circumstances, for a series of action is incessantly going on in every animated machine, and those actions are maintained by external circumstances—by a certain degree of temperature, by a certain surrounding atmosphere, and also by a certain supply of matter to be taken into the mouth or stomach, or something equivalent. The deprivation of any of these may put a stop to existence, or may impair it; the excess, on the other hand, of some of these, will have the same effect. The excess of food may destroy or injure life, and the excess of heat may cause serious injury, or absolutely consume the body. Besides this, the depraved quality of those external matters which are necessary to the continuance of life, will have an injurious effect: the unwholesomeness of bad food is a circumstance particularly illustrative of this.

One of the most common causes—indeed, I may say, the most common cause, of diseases—is a deviation with respect to external temperature. A certain degree of heat is required for the support of life, and a deviation in that particular is the most common cause of the diseases that we see here. An excessive degree of heat produces one set of effects, an excessive degree of cold produces another, and an alternation of the two from the body being suddenly exposed first to the one and then to the other, more especially if this exposure be partial, is attended by the most serious effects. The greater number of diseases are inflammatory, and the most common cause of inflammation is sudden alternations, or other unfavourable circumstances, in regard to temperature.

Bad air is also a common cause of disease,

but this does not arise from the want of oxygen in particular, as has been imagined, but from the presence of certain substances of deleterious quality. I believe it is found that the quantity of oxygen in the air is the same in almost every country and every situation; and it is the presence of other things, in addition to the oxygen, which produces the injurious effect. We see the effect of the bad quality of the air in unhealthy situations, where persons are sallow and bloated, and carry about with them marks of disease visible enough to others who come from a healthy spot, although these are not allowed by the parties themselves to exist, simply for this reason, that they are so accustomed to what they witness that they conceive it to be their nature.

The ill effects of unwholesome food are seen every day. The body is rendered feeble; and when this takes place, whether from bad air or bad food, it becomes an easy prey to any other cause of disease which may present itself. Want of the rest necessary for the refreshment of the body has also the effect of producing disease. Excessive action, too, either of the mind or any part of the body, has likewise the same tendency.

Thus disease is continually produced from mechanical violence, from the presence of injurious agents, and from errors in regard to those agents which are absolutely necessary to the support of life, whether it be the error of excess, deprivation of their proper quantity, or deprivation of their qualities.

This, however, is not all. When a disease has been induced, it frequently happens that a tendency to that disease is transmitted to the offspring of the sufferer, so that it requires no exposure of such offspring to the particular cause of the disease in order to become its victim; but at a certain period of life, unless placed in very happy circumstances—and perhaps in spite of the happiest circumstances possible—the disease under which the parent laboured appears in the child, or in still later offspring, and possibly cannot be controlled. Again, independently of the external circumstances to which the offspring is exposed, and inherent disposition to disease, original malformation of certain parts of the body, at last so disorder its functions, that sooner or later disease appears which may end in death.

Now when disease takes place, the functions of the body go wrong. A series of actions is continually taking place in the body, and some of these, when what is called *disease* occurs, become deranged. The circulation of the blood is altered; more is accumulated in some parts than there should be, and less in others. The temperature of the body is altered; it is too hot either partially or generally, or it is too cold. The feelings of the body are altered; they become too acute or too dull, or they become otherwise

wrong. The strength of the body likewise is changed; in some few instances it is increased, but in the great majority it is impaired. What is done naturally in the body before this change takes place, is now either done too little or too much, or in a depraved manner. The products of the body are likewise altered; the sweat is not natural in its quality any more than in its degree: the urine becomes altered in its quality, and the secretions poured forth from the intestines likewise in theirs; and I have no doubt that the excretion from the bronchial tubes is likewise changed in its constituent proportion. At last even the structure of the body will become affected. Not merely are the actions of the body altered, and the products excreted from it, but the very substance of the body itself will become changed.

With respect, however, to all the changes I have just enumerated, they must evidently be in a certain intensity, a certain degree of them must exist, for us to say that disease is present. A change, although, strictly speaking, you might say it was a disease, may be so inconsiderable, that one would be laughed at for calling it a disease. A slight spot upon the surface, a single point not larger than a flea-bite, no one would think of calling a disease; and yet, if there were a large number of these, though the only difference consisted in amount, the nature and degree of each being exactly the same, we should not hesitate to say that disease was present. Therefore, when a deviation with respect to function or structure takes place, there must be a certain extent of it, or a certain intensity of it, one or the other, to justify us in saying it is really a disease. Logically and strictly speaking, disease is said to be "an aberration of function or structure of one or more organs from a sound state." You will find this mentioned in some books as the definition of a disease. But this is hardly sufficient; because, with respect to structure a part may be sound, and even with respect to function, and yet be in a state of disease. A part may grow to a much greater size than natural, there may be diseased bulk of the organ, although the structure is healthy. This is a rare occurrence, but sometimes it does take place. Generally, where there is increased bulk there is diseased structure; but it is possible for a part to be enormously large without being altered in structure, and yet the person may be said to be labouring under disease. A person with an enormous liver, or brain, though no unhealthy change could be discovered in its texture, would nevertheless be pronounced to be in a state of disease.

So with respect to the fluids; they also suffer very great changes, and yet there may be no fault of function. In the disease called *anæmia*, where the blood is deficient in quantity, the disease may really be quite independent of any fault of the body itself.

A mere want of nourishment may cause the blood to be of a diseased quality—that is to say, not to contain its natural elements, but to be very watery—to be thin and pale; the whole body becoming white or tallowy in appearance, and yet no function can be said to be diseased, at least excepting through this. The body is ready to do all that is required of it, if proper food be given. Therefore this definition of disease, you find, is more or less exceptionable. Besides, in a large number of diseases, the quality of the fluids is altered, and it is essential to a correct definition that this should be taken into account.

With respect to the extent of affection which is necessary to exist in order to constitute a disease, there must be a great difference in various individuals; for the health of different persons is exceedingly various. What is health to one man, would be disease to another; the degree of strength which is natural to a delicate person would be considered downright weakness in another. To obviate any difficulty of this kind, you will sometimes find added to the definition of disease which I have already given, "rendering assistance necessary." The definition given in Dr. Gregory's *Conspectus* is—a disease is present when the customary actions of the body are either altogether impeded or performed with difficulty and pain. "*Morbus adest, quum corpus tantum a statu sano deflexcrit, ut solitæ actiones vel prorsus impediuntur vel ægre aut cum dolore perficiuntur.*"

Disease, therefore, may generally perhaps be defined to be, "an aberration of function, or structure, or size, of one or more organs; or of the quality of the fluids, from the sound state, rendering assistance necessary."

This, however, you will observe, is only a definition of disease in general, and I shall have to make some remarks upon the definition of each particular disease hereafter. You will see thus that diseases are neither more nor less than alterations in the physiology or anatomy of the body—that they are morbid physiology and morbid anatomy. This morbid state of physiology and this morbid state of anatomy, are together called *pathology*. The doctrine of diseases, whether regarding function or composition, is called pathology. There is an inclination at the present day to limit the word pathology to morbid anatomy. That, however, I think, is quite unjustifiable; for pathology is the doctrine of disease, whether it relates to function or structure, or both. Pathology, therefore, is an unhealthy state, if I may so speak, of physiology and of anatomy; the composition of the fluids being comprehended under the term physiology as well as function—size being comprehended under the term anatomy as well as structure.

Now diseases are often divided into those which are functional and those which are

structural, but it is very common for the one to terminate in the other—for diseases which are at first purely functional to become at last structural; or at any rate many organic diseases do not at first exhibit themselves as such, but are ushered in by a change of function. It is possible that a minute change of structure may exist long before it is discovered, and that minute change is enough to explain the alteration of function; but many diseases exist long, to all appearance, as mere functional disturbance, before they become structural. Many diseases remain functional, at least they go through their course as a disturbance of function, never affecting the structure; but structural diseases almost always disturb function. It is evident that if the structure of an organ which is to perform a certain duty be much altered, that duty cannot be well discharged.

Structural diseases are sometimes called *organic*. I believe that the words structural and organic are used synonymously, to express those diseases which consist in a change of composition and arrangement of the organ itself, and not a mere alteration in the mode in which it does its duty. Perhaps the word organic is preferable to the word structural; and on this account, that some diseases of this description do not appear to consist in any alteration of structure, but merely in a diminution or in an excess of bulk. Now in those cases which, though they are rare, nevertheless do sometimes occur, in which an organ wastes, or becomes larger than natural, and yet appears otherwise healthy, the affection cannot exactly be called structural—the word organic better applies to them, and therefore, altogether, the term organic may be preferred to structural.

There has been an attempt by some to call the one set of complaints supposed to be merely functional—*disorders*; and the others which are structural or organic—*disease*. For instance, to call dyspepsia, or diabetes, a “disorder;” and ulceration or cancer of the stomach, or ulceration of the kidneys, “disease.” You will, however, not find this generally adopted. It might be very well if all persons agreed to adopt the terms in this sense. But that is not the case: indeed, so far from it, that, in most books, the expression organic or structural *disease* occurs, which would not be necessary if every one adopted the word disease to signify in itself a structural affection. Nay, so extensively is the word disease employed by some persons, that even accidents, as dislocations or fractures, are called disease. You will find, in Cullen’s Nosology, that dislocation and fracture are arranged under the class of local *diseases*. The word disease, then, is used by some nosologists very extensively; they employ it to signify accidents, mechanical affections, dislocations, hernia, and every thing of that description; and it is

used by almost all writers to signify both functional and structural affections. I shall not, therefore, attempt to use the word disease to signify organic affections exclusively, but all depraved conditions, whether functional or organic.

Disease may be considered accordingly as it is local or general. Some diseases affect only one part, and others are supposed to affect the whole system. Fever, for example, is supposed by many to be a general affection, and it has been instanced as such; others, however, now deny that there is any such thing as a general disease, and assert that fever is only an inflammation of one part or other of the body. You will find that Broussais maintains the latter doctrine. However, I can entertain no doubt that there are such things as general diseases; and it appears to me that scurvy is of this description, for every fluid of the body seems vitiated, at least the blood throughout the whole body is so, and all the solids appear to be in a similar state. There is no one part of the body that can be fixed upon as the seat of the malady, and therefore I think scurvy a fair illustration of general disease. So with respect to ague. I cannot discover any reason for ascribing the disease to any one part of the frame, and I therefore think that it also appears an instance of a general affection.

Diseases are also different from each other accordingly as they are acute or chronic. Some diseases come on suddenly, last usually but for a short time, terminate either in health or death, *cita mors venit aut victoria lata*, and are called *acute*. Other diseases last for a long time, perhaps come on slowly; but whether they come on slowly or suddenly, if they last for a long time they are called *chronic*. Acute diseases, however, will sometimes, instead of terminating in health or death, become chronic—that is, become less violent in their degree, and go on for a length of time. Diseases, therefore, are to be considered acute or chronic according to their duration.

You will find that diseases are also considered by some accordingly as they are active or passive—that is to say, accordingly as they are attended with a degree of violence or excitement of the system, or no excitement at all. You will find some persons, indeed a large number, use the terms active and passive, and acute and chronic, synonymously; but this is an error against which I am particularly anxious to guard you. It does not follow because a disease is acute—that is to say, exists for a short time, and then terminates—that it is necessarily attended by violent symptoms; nor does it follow, because it lasts for a length of time, that these symptoms should not be active. This is every day illustrated in the case of rheumatism; it is daily spoken of as acute or chronic, and active or passive; the term

acute being used indifferently with active, and chronic with passive, so that, in the chronic form of the affection, a person would not think at first of applying such remedies as in an acute attack, taking it for granted that, as the disease is chronic, there is no violence, but that a slowness of mischief is going on, and that the remedies for the active acute state are improper. But you see cases of rheumatism every day, which have lasted for six or twelve months, or even longer, attended with all the symptoms of an acute disease—that is, attended, if not by quickness of pulse, by heat of the parts, and if you take away blood you find it buffed; it would, therefore, be a serious error if you were to consider active and passive as synonymous terms with acute and chronic, because in acute diseases, the word acute refers simply to their short duration, and in chronic diseases, the word chronic simply to their long duration; whereas the word active refers to the violent disturbance going on in the system or part, and the word passive to slow changes, changes not of a violent description. A chronic disease may be active, and an acute affection may be passive. A person with an acute affection may lose all his powers, and the whole functions of the body may go on in the most languid way; and therefore acute and chronic are one kind of thing, active and passive are another. An acute affection cannot be chronic, but it may be passive; and a chronic affection cannot be acute, but it may be active.

You will find diseases differ also according to their uniformity of duration. Some diseases continue with uniform tenour during their whole course—without any very great deviation from the beginning till the end, and these are called *continued*. Other diseases, again, undergo a very great remission at regular periods, or at least pretty regular periods, and then they are called *remittent*, the symptoms not going off entirely till the disease ceases altogether, but undergoing such a relaxation of severity at such periods, as to acquire a character from this circumstance. Other diseases, again, will cease entirely from time to time, and generally too at stated times, and recur again at certain intervals as long as the person is affected with them; these are called *intermittent*. This is particularly seen in the case of fever. Fever may hold a pretty uniform course, never ceasing at all, or particularly diminishing, during the twenty-four hours, till its close, and we then call it “continued” fever; it may last for a day, a week, or several weeks. Others are characterized by a sudden diminution in the degree of heat, violence of pulse, of thirst, and all the other symptoms, at a certain period in the twenty-four hours, and perhaps at longer intervals, but not ceasing entirely: such is the character of “remittent” fever.

Others, again, cease entirely; the patient will be attacked at a certain hour; he ill a certain number of hours, and then be perfectly well, with the exception perhaps of languor: the fever then receives the name of “intermittent.”

Diseases differ, therefore, accordingly as they affect function or structure; accordingly as they are local or general; accordingly as they are acute or chronic; accordingly as they are active or passive; accordingly as they are continued, remittent, or intermittent.

Diseases likewise differ exceedingly in their true inherent nature, independently of any other circumstances. They differ also as to their causes. Some are produced merely by an aberration as to some of the circumstances which are necessary to the support of life, such as the excess of temperature or food, or a deprivation of food or air, or a diminution of either food or air. Others, again, are produced by specific causes, by the agency of particular substances, the nature of which cannot be comprehended; such, for example, is ague, which is produced only by a certain exhalation, and such is small-pox, and all those diseases which acknowledge a peculiar specific poison for their cause. Diseases differ likewise according to the organ which they affect. There are some diseases which can affect almost every organ in the body. Inflammation is one which affects every part of the frame which has vessels; but there are certain diseases that can only affect particular organs; diabetes, for example, can affect, so far as we can judge of it, only the kidney; and the tendency to produce gall stones can evidently affect no other part than the biliary system. Therefore diseases differ, in addition to all the circumstances previously mentioned, accordingly as they may affect any part of the body at large, or are necessarily confined in their operation to particular organs.

With respect to the *causes* of disease, you will find them generally divided by authors into two, the *remote* and the *proximate*. The remote causes, again, are divided by systematic writers into two more, *predisposing* and *exciting*. The word cause, however, is in these cases used in a totally different sense. When we speak of remote causes, a very different idea presents itself to you than when we speak of proximate causes; in fact, the cause of the disease, properly speaking, can only be remote. For example, the remote cause of fever may be bad food, and the depressing passions; and the exciting cause may be, according to some, a specific contagion. Now the remote causes of the first description are called *predisposing*; they render the body liable to become the prey of something which has a tendency to excite the disease. The exciting cause of the disease might have had no effect unless the body had been predisposed, and the predisposition

might not have had the effect unless the exciting cause had occurred. A circumstance, therefore, inclining the body to become the subject of disease is called a predisposing cause—*causa prædisponens*; that which actually excites the disease, exciting—*causa excitans*; and both together are called *causæ remotæ*. They are called remote, I presume, because they are a little distance from the disease itself, and because the proximate cause is, to all intents and purposes, close to and all but the actual disease, and, in one sense at least, it is the disease itself.

The application of this term gives rise to a great deal of confusion to the student. You would imagine that disease could be nothing more than disease; and to call the disease the proximate cause would be thought absurd; but the reason of this I will explain. In defining any particular disease we are obliged to take into our definition only what we observe, just as the naturalist defines any flower, or any mineralogical specimen, merely taking what is the object of sense, and describing it with all the marks together; and that is called the definition of a disease. For example, in jaundice you take the yellowness of the skin, the yellowness of the eyes, the yellowness of the nails, the whiteness of the faces, the high colour of the urine, together, and you say that the patient certainly has jaundice. The jaundice is not the disease, strictly speaking; but the collection of symptoms under which the patient labours is called jaundice. The word disease, by nosologists, is thus applied not to the single cause of all these symptoms, but to the collection of symptoms. Take epilepsy: a person falls down foaming at the mouth; struggling in every limb; perfectly unconscious; and afterwards, when he ceases to foam and struggle, he lies senseless; such a person is said to labour under epilepsy. These particular symptoms, blended together, occurring in succession, are called epilepsy. Now in Cullen's, and other methodical nosologies, you will find the disease defined according to mere symptoms; all opinion, all cause, is excluded; you take merely the symptoms, and you call them the disease, and it is perfectly right that it should be so. But these symptoms must have a cause—there must be a cause for this disturbance in epilepsy—there must be a cause for the bile going into the blood in jaundice, appearing in the urine, and not being able to find its way to the fæces. The cause of the epilepsy, or of the jaundice, is the circumstance that produces all these effects, and that is considered to be the "proximate" cause. If the epilepsy arose from a piece of bone depressed upon the brain, you would say the proximate cause of the epilepsy was the spiculum of bone. If the bile were obstructed because a calculus was impacted in the hepatic duct, you would say the impaction of the calculus was the proximate cause of the jaundice. You therefore

observe that the proximate cause is, in fact, the disease itself—the actual disease from which the symptoms arise. The remote causes are those that predispose to the disease, or actually excite it; the proximate cause is the circumstance to which the predisposing and exciting causes have given rise; and this circumstance, when present, produces all the external symptoms—the visible changes.

But the word disease, when you come to any specific affection, is, in a nosological sense, applied merely to a set of symptoms—to that which we can comprehend—which is discernible; and as nosologists, we should not apply the word disease to the cause of all these symptoms—but say it is the proximate cause. You will find, however, when speaking as pathologists, and not as nosologists, that persons inquire, what is the actual disease in this epilepsy? What is the actual disease in this jaundice? Is it an enlarged pancreas pressing upon the ducts? Is it a calculus sticking there? Is the disease a contraction of the sides of the ducts, or what is it? But that is a different sense of the word disease, and it is necessary that you should know that the term is employed by methodical nosologists, and must be so by me, to signify a collection of symptoms, and not the true cause which is at the bottom of those symptoms; for though we might be inclined to say that this was the real disease, yet it is not so considered, but has received the appellation—proximate cause.

I trust, therefore, from the explanation now given, you will hereafter escape any confusion which might otherwise arise, because for the want of understanding this, you will hear persons quibble about what is a disease and what is not. It is necessary to know that a set of symptoms, following in a certain order, or united in a certain collection, is called a disease; and the immediate cause of all these symptoms—which some say is the real disease—is called by the best writers the proximate cause.

DR. PROUT ON THE APPLICATION OF CHEMISTRY TO PHYSIOLOGY, PATHOLOGY, AND PRACTICE.

In Answer to certain Strictures of

DR. WILSON PHILIP.

To the Editor of the London Medical Gazette.

SIR,

HAD I not promised to reply to Dr. Philip's observations, I am not sure if I should have now done so, as after a second perusal of them, I find them still less to the purpose than they first appeared to me to be. Having made this promise, however, I cannot now well

retract, and accordingly I proceed, at the risk, I fear, of being considered a little tedious, to notice a few of the more prominent details.

1. Dr. Philip commences by alluding to a conversation which took place between himself and me, and which he calls "private." To obviate any misunderstanding on this point, I may say that the conversation alluded to took place in the apartments of the Royal Society, in the presence of two witnesses, who, I dare say, well remember the substance of what passed on the occasion.

2. On the subject of metaphysics, I shall only observe, that the sense in which this term was used, whether right or wrong, could hardly have been the cause of the Doctor's mistake, as he evidently saw this. My authority for this assertion is a note appended to the first page of his first observations:—"It is remarkable that Dr. Prout here uses the word psychology as synonymous with living action, while in the foregoing sentence he uses it as synonymous with metaphysics. *Is then the term living action synonymous with metaphysics?*" If these words do not convey the idea that he saw the matter in question, I am quite at a loss to understand them: the disapproval of the sense, is quite another question.

3. The next observations I think it necessary to notice are the following:—"Can Dr. Prout assert, that the laws of the living animal body are not as legitimate a subject of experiment as the composition of the fibres of which it consists? Are they less important, less conclusive, or placed more beyond our reach, than those of Lavoisier?" To this I reply, that I have no where asserted, nor dreamed of asserting, that these laws are not legitimate subjects of inquiry; and as for their importance, I have always considered this to be first rate. What I am disposed to assert is, that these laws are infinitely more difficult and beyond our reach, than those of chemistry, and, consequently, that experiments made to investigate them, are infinitely more likely to be erroneous*. Every body knows that

the metaphysical department of physiology (I use the word in the same sense

familiar illustration of the principal grounds upon which human knowledge rests, may not be deemed altogether superfluous:—

1. *Mathematical grounds.* Two and two must make four; we cannot conceive the contrary.

2. *Universal and perfect experience.* Sulphuric acid and potash, when mixed together in certain proportions, have in all ages formed a salt, called sulphate of potash, and from this universal experience we conclude that the same thing will always happen in future.

3. *Partial and imperfect experience.* When we apply a mechanical stimulus to a certain part of the brain of an animal, a distant set of muscles will be convulsed; say nine times in succession. When a person swallows a dose of calomel, he will be purged, perhaps as often. But the tenth time, from some reason unknown to us, the distant muscles will not be convulsed, or will be differently convulsed; and the person who swallows the calomel will not be purged but satiated; and this single failure is sufficient to throw an air of uncertainty, with respect to the future, over these points; that is to say, we are not absolutely certain, in any instance, what will take place in our next experiments. Hence, all knowledge founded upon partial and imperfect experience, is likewise imperfect, and amounts only to the *probable* (the probability being greater or less as the experience on the particular point, is more or less perfect), and this unfortunately is the case with by far the greater proportion of physiological and pathological knowledge.

The above applies to the experience of an individual himself; but the greatest part of our knowledge must be necessarily derived from others. Hence, it becomes necessary to inquire, how far the above grounds of our knowledge are liable to be influenced by the character of the observer himself; or, in other words, to inquire into the value of testimony:—

1. Mathematical knowledge is independent of the operator, and consequently not liable to be influenced by testimony. No one supposes that two and two ever made any thing else than four, whether the author of the addition was a knave or an honest man, an idiot or a philosopher.

2. In knowledge founded on universal experience, it is often necessary to take the character of the operator or witness into account, not, indeed, usually with respect to the essential fact, but with respect to many of its concomitant circumstances. Thus, an expert chemist on mixing sulphuric acid and potash in the proper proportions, will obtain the sulphate of potash in beautiful crystals; a clumsy or inexperienced chemist will obtain no crystals at all, or very imperfect ones; and these two operators on becoming narrators of their experiments, though agreeing about the fact of the formation of sulphate of potash, will give very different versions of the concomitant circumstances.

3. In knowledge founded on imperfect experience, the character of the witness, and consequently the value of his testimony, is of infinitely greater importance; for here he may not only be wrong as to minor points of little value, but essentially and radically wrong as to the principal fact. A clever and faithful operator will obtain certain results, and narrate them accordingly; but your clever operator may be a knave, and narrate what he does not obtain; or he may be a prejudiced man, and narrate only half the truth; or he may be a bungler, or a compound of the bungler and knave, &c. &c. all which may happen from the barely probable character, of the point in question, without our being able to prove the contrary; and if we have no means of forming a just estimate of the degree of credit to be attached to the witness, we must be content to remain in doubt on the point in question.

* Physiologists and pathologists are exceedingly apt to lose sight of the grounds upon which their department of knowledge is principally founded, and to argue and assert with all the confidence of mathematicians and chemists. For the sake of the younger members of our profession, who are most apt to fall into this error, the following

as before) has always been considered as the region of fancy and hypothesis, and as uncertain, even to a proverb. The reason of this lies in the very nature of the thing itself, which, for the most part, is beyond our comprehension. Were the laws of the living animal economy as capable of being understood, and of becoming the subject of demonstration, as the laws of mechanics, or even of chemistry, the study of them would be independent of these and all other auxiliary branches of knowledge; but as this, from the nature of things, never can be the case, I have always agreed with those who think it their duty to call in the assistance of any auxiliary means that promise to throw light upon the subject. Of these auxiliary sciences, mechanics (anatomy) and chemistry are by far the most important: the study of physiology, therefore, will comprise, according to this view,—1st, a knowledge as perfect as can be obtained of the laws of the living animal economy—laws which, like the living agent itself, are *sui generis*, and only comparable with one another; 2, a knowledge of general anatomy, or the mechanism by which the living agent operates, and is, in short, manifested to us; and 3, a knowledge, as far as can be obtained, of those important chemical changes constantly going on in the living animal, and apparently necessary to its existence. Now, though these two latter branches of knowledge throw no light whatever upon the abstract nature of the living principle, or even of its laws of action, yet, like the index to a complicated piece of mechanism, or the chemical action of a galvanic battery, they often enable us to influence, and even regulate them to a very considerable extent, in an indirect manner; and, in fact, they constitute our only legitimate and rational means of doing so.

4. The next point which I deem it necessary to notice is Dr. Philip's opinion on the nature of the nervous principle. "Does Dr. Prout, then, consider the term nervous and living principles as synonymous? Principles that have not one quality in common?" To which I answer, that I *do* consider the nervous principle to be a living principle, and beg to ask who besides Dr. Philip does not? Indeed, what the doctor meant in this place I was for some time at a loss to comprehend, till

I recollected that he entertained some whimsical notion that the nervous principle was identical with galvanism—a notion which I not only do not admit, but have always considered as absolutely absurd. Let us examine this matter a little.

It is a remarkable feature in all Dr. Philip's observations, that *words* rather than *things*, seem to occupy his mind. This is particularly the case with the word galvanism, or electricity, which he seems to consider as representing some abstract agent or energy, uniform in its character and effects; instead of viewing the *thing* galvanism as it really exists—that is to say, as consisting of two distinct but inseparable energies, directly opposed to each other, which so far from being capable of being substituted for one another, or employed indiscriminately, act directly contrarily, the one invariably undoing what the other does*. From these well-known properties of the galvanic energies, in all experiments made with them, it becomes necessary, in the first place, to consider the object we have in view, and to arrange the polarities accordingly, otherwise, so far from accomplishing our object, we shall do just the reverse. Now I cannot find that Dr. Philip in his experiments with galvanism on living animals, has at all taken this important point into account; whether, for example, the action of the nerve he operates upon be positive or negative; nay, I do not observe that he even alludes to the subject, but seems to consider it a matter of perfect indifference whether (supposing, for example, the action of the 8th pair of nerves be positive) he attempts to supply its nervous influence from the positive or negative side of his battery. Now this has always appeared to me to constitute, at the very outset, a fatal objection to the doctor's conclusions; but there are other objections, if possible, still more formidable than this. Let us suppose, for the sake of argument, that the action of the 8th pair of nerves be positive, and that at the termination of the nerve in the stomach it forms a union, or state of equilibrium, with its opposite or negative energy, the galvanic circle will thus be completed within the limits of the animal's stomach and the nerves

* For the sake of easier illustration, I adopt the hypothesis of two energies; but the objections are equally valid on the Franklinian hypothesis.

supplying it, or, at least, *within the body* of the animal. Now let us consider how the doctor completes his substituted galvanic circle, after the division of the said nerve. We shall suppose that, by accident, the positive wire of the battery is attached to the divided end of the nerve (supposed to be positive), and that thus a current of positive electricity is sent along the course of the nerve—what happens then? *Instead of terminating in the stomach*, like the nervous energy for which it is substituted, it is directed on through all the intervening muscles, integuments, &c. to the *outside of the animal's body*, and is then connected with a train of wires to the other end of the battery. Now I appeal to any one acquainted in the least degree with the principles of galvanism, whether the two cases can be considered as similar, and whether, supposing the nervous influence to be galvanic, and the polarity even to be rightly adjusted, the galvanism supplied can be considered as acting identically with, and therefore forming a proper substitute for the natural action of the nerve.

But, leaving this part of the subject for the present, let us view the matter in another light. The galvanic energies, as we understand them, are not cognizable to our senses in that state of equilibrium in which they are supposed to exist every where, and only become known to us when this equilibrium is destroyed. Now let us suppose them to exist in a state of equilibrium in the nervous system of a living animal, and some occasion arises for galvanism in an active state, to obtain which, of course, the assumed state of equilibrium must be destroyed. How is this to be effected? The galvanic energies have not the power of destroying their own equilibrium; and if this be done at all, therefore, it must be done by a power existing in the nervous system different from these energies, and superior to them. Nor is this difficulty got rid of by supposing, as some have done, that during the life of an animal, the galvanic energies are always active in the nervous system. Every body knows that they are not always *equally* active; and as we can no more suppose them capable of regulating their own activity, than of destroying their own equilibrium, we are still driven as before to the necessity of supposing the existence of a su-

perior principle. Now this superior principle, whatever it may be, residing in the nervous system, ought, I think, to be considered as the real nervous principle; and as it exists in the nervous system during the life only of the animal, it ought to be considered as a living principle: for I need scarcely observe, that the galvanic energies exist in the same state of equilibrium in the *dead* animal as we supposed them to do in the *living* one, but their equilibrium can no longer be destroyed, even though the nervous apparatus remains quite perfect, because the proper living nervous principle no longer exists.

Several other difficulties with respect to the doctor's opinions might be pointed out, if my time admitted; but I presume what has been said is quite sufficient to shew how very ill grounded they are; I hasten therefore to consider the point alluded to in my last letter, viz. the doctor's notable project of supplying artificially, in various diseases, the nervous principle, by sending a current of galvanism through the body; and I do this at the risk of being considered guilty of a little tautology, with the view of preventing others from falling into a similar absurdity. Let us suppose the disease to be asthma, and that, according to the doctor's notions, the nerves influencing the lungs in this disease be deficient in action, and that it is intended to supply artificially this deficiency by its presumed equivalent, galvanism; how is this to be effected?

"Two thin plates of metal," says the doctor, "about two or three inches in diameter, dipped in water, are to be applied, one to the nape of the neck, the other to the pit of the stomach, or a little lower. The wires from the different ends of the battery (containing from eight to sixteen four-inch plates of zinc and copper) are brought into contact with the plates, and as great a galvanic power maintained as the patient can bear without complaint. In this way the galvanic influence is sent through the lungs, as much as possible in the direction of the nerves; and" (mark this particularly) "*the relief seems to be the same whether the positive wire is applied to the nape of the neck or the pit of the stomach.*" Now the nervous principle acting in the lungs, supposing it be common galvanism, must be positive or negative, at least *it must move in one direction*. The doctor applies the poles

of his battery indiscriminately, and thus *reverses the current at will*, and, wonderful to relate, *the same effects take place*. In the patient the entire galvanic circle is completed *within the body*; the doctor's substituted galvanic circle is completed, under totally different circumstances, *without the body*, the body forming only a small portion of the circle. The simple statement of these facts, as before observed, are quite sufficient to satisfy any one acquainted with the principles of galvanism of the absurdity of the doctor's conclusions; and not only this, but even to shew that the *agent employed* by the nervous principle can hardly be considered as *identical with galvanism*, as we are acquainted with it.

With respect to the point under consideration, my own notion on the subject (formed now nearly twenty years ago,) is, that the galvanic energy, (or energies) in some state or other, is *employed as an agent* by the nervous principle; though I never have been able to form any probable notion as to the mode in which this is effected. From the beginning I have been puzzled to dispose of the polarities; it is very easy to suppose that a given nerve acts positively, but then how is the other polarity to be disposed of, (for every body knows, as before observed, that in galvanism, as we are acquainted with it, the positive energy cannot exist without its corresponding negative, and *vice versa*); do the different energies exist in a different set of nerves, or in the same nerve—that is to say, are there two currents, the one of positive and the other of negative electricity, in every nerve? On account of these, and other difficulties that might be mentioned, I long ago came to the conclusion, that the galvanic energies existing in the nervous system of animals could not be exactly identical with those we are acquainted with; and the experiments of Davy and other philosophers, seem, in my opinion, to satisfactorily prove this. Probably the galvanic agents, as they exist in living beings, have the same relation to matter in a merorganized condition, that common galvanism bears to matter in its pure or homogeneous state. The subject is one of great curiosity and importance, and, in the hands of a master like a Wollaston or a Davy, would, I have no doubt, lead to the most unexpected results.

Lastly, I by no means wish to be understood as denying the results stated by Dr. Philip as happening in his experiments; nor do I doubt the efficacy of galvanism in various diseases, but, on the contrary, think rather highly of it when judiciously applied. Were it necessary, these might all, perhaps, be satisfactorily explained on principles very different from those assumed by Dr. Philip.

5. The objection in the last paragraph of page 738 is too subtle for my poor understanding; if it has not, therefore, been already replied to, I must be content to leave it to its fate.

6. The objection in the next page is founded principally on an accidental error. Instead of "merely," I wrote, or intended to write, "chiefly," as might indeed have been inferred from the context. I give the doctor the full advantage of this slip.

7. I pass on now to the next paper, and the first point I arrive at is the doctor's remarks on the term "merorganization." His objections to this term, as far as I can understand them, are two-fold; first, as a mere chemical term, "because it implies a knowledge of different degrees of organization;" and, secondly, as a physiological term, "because it involves me in considerable difficulty." Let us inquire a little into these points.

With respect to this term, I have stated, in a quotation given by the Doctor, that it is used "*provisionally, and to prevent circumlocution**," to express a very numerous set of phenomena apparently connected with the presence of minute portions of foreign bodies, some how or other associated with bodies formed on definite principles; and that these phenomena, if not absolutely identical with those of organization, at least appear to constitute their ground-work—that is to say, that organization cannot exist without the presence of these foreign bodies. As to the term itself, therefore, this will of course be superseded the moment the doctrine it is intended to designate be established or refuted; for, in the former case, the old term, organization, must necessarily convey the same meaning, and in the latter it will of

* And I may add, to prevent the necessity of giving a new meaning to the simple term organization, by encumbering it with what must be considered at present as an hypothesis.

course become superfluous. The term, therefore, in this general sense (or the old term organization, if any one prefers it,) necessarily includes a vast variety of phenomena differing from one another, not only in degree but in kind. My opponent, however, (still doomed to be the slave of a word) will not, as far as I can understand him, admit that organization *does* differ, either in kind or in degree; and upon this assumption his objections are chiefly founded. But he must speak for himself. After alluding to the different senses in which the term has been used, and observing that it sometimes expresses the composition of dead as well as of living animal and vegetable substances, he observes, "In this latter sense of the word, the more perfect animals, I conceive, *have no organizing power*—they can only exist in matter already organized;" and "if the more perfect animal was capable of organizing its food, like the vegetable, or, as it would appear, some of the more imperfect animals, it could live on air and water alone!" The more perfect animals, says the Doctor, have no organizing power. Does, then, the man who dines off potatoes, and converts them into human muscle, not further organize, or at least differently organize, the already organized potatoes? Does the organization of the zoophyte not differ in kind or degree from that of the human being?

Again, I have said, speaking of the merorganizing power of the stomach, "From long attention to the subject, I am satisfied that the merorganizing principles are chiefly derived from the living animal itself, *at least the more essential ones*, and that they are contained in those products of secretion furnished by the stomach and other organs." The Doctor here cuts short the quotation, but I go on to observe, "the merorganizing principles, therefore, already existing in the aliments, though they undoubtedly render them better adapted to the purposes of the animal economy, are not sufficient; and unless the economy furnishes the materials, properly prepared, the future work of assimilation will be imperfect." The meaning of this, admitting the existence of different degrees or kinds of merorganization (which no one can, I think, for a moment doubt), is, I presume, sufficiently obvious; but the Doctor's observations on this passage are really a

curiosity, and as they place in a striking point of view, and in few words, his general mode of reasoning, I shall quote them, for the amusement of the reader. "*If the stomach merorganizes the food, it must have a disorganizing power, because the food is wholly organized when it enters it.* It will be admitted that no organization can be more perfect than that of the muscular fibre."!

The points to be considered with respect to merorganization, and to which I particularly wish to direct the attention of the physiologist, are, can or cannot, or rather does or does not, organization take place without the presence of these foreign bodies; and if so, under what circumstances? How is merorganization, in its simplest state, connected with *isomerism*? Had Dr. P. instead of quibbling about the use of the term, directed his attention to these points, he would have done physiology some service. For my own part, I can only say, after many years attention to the subject, and the investigation of innumerable facts, I have little doubt upon the subject; but I must leave the matter now to be confirmed or refuted by others.

6. The next point considered by Dr. Philip is, "how far chemistry is applicable to explain the phenomena of the living animal." And here, instead of keeping to the point in question, he gets as usual quite beside it, and brings forward such a mass of common-place and irrelevant matter that it is quite sickening to wade through it. The clue to the whole, as far as I can understand it, is, that what I do not happen to mention in my lectures, I either am quite ignorant of, or call in question, or actually deny; and by the aid of this clue, and a few illustrations, the reader will be enabled to get a little insight into the affair.

a. "Physiology and chemistry will always be regarded as distinct sciences, and the circumstance of the same person studying both sciences, will not form these sciences into one." I am perfectly aware of this, and have nowhere asserted the contrary. I have, indeed, spoken of the study of the anatomy, of the chemistry, and of the living functions of animals, as coalescing or constituting one great whole—viz. general physiology—and also of the physiologist turning chemist; but these are quite different things.

b. "To be thoroughly acquainted with the vast science of modern chemistry, and keep pace with the rapid discoveries which every day add to its utility and importance, he must make it his chief business, to the neglect of what more particularly belongs to his own department." A simple and obvious truism, but quite misplaced here, as it had never been called in question. The physiologist has no more to do with the vast science of modern chemistry in all its details, than he has to do with building steam-engines, or any other engines. Inorganic and organic chemistry are quite distinct branches of the science, and an individual may excel in one and know little or nothing of the other. In short, I should say that there is no one more liable to fall into errors than the mere inorganic chemist, when he comes to operate for the first time upon organic substances. I found this, from experience, fifteen or sixteen years ago, and was induced to relinquish general chemistry very much in consequence, since which time I have had little to do with minerals except those of the commonest kind, or those employed in pharmacy, with which every body is supposed to be acquainted. I will venture, however, to assert, whatever the doctor may think of the contrary, that, in the present state of physiology and pathology, a knowledge of organic chemistry, and its rational and cautious association with the phenomena of life, is absolutely necessary to him who wishes to become thoroughly acquainted with his profession; and that he who neglects it, neglects one of the most powerful, and in many cases, the only means of acquiring accurate knowledge.

c. "In speaking of the phenomena of animal life, Dr. Prout keeps in view only the chemical department."—"Now chemistry is powerless in investigating the laws of the vital functions, and ever will remain so." My object was professedly to speak of the application of chemistry to physiology and pathology: I had, therefore, nothing whatever to do with the other departments of the science, or, at most, occasion only to mention them incidentally. Besides, I was addressing an audience supposed to be already well acquainted with these matters. Had I been writing a professed work on physiology, of

course every part of the subject would have been equally treated of. If I am to be blamed, however, for this omission, is not Dr. Philip much more deserving of blame, who has written a book professedly on the vital functions, and yet has omitted entirely the chemical department of his subject? To the second part of the above objection, I reply by quoting my own words, "The only way in which a physiologist can hope to influence the operations of nature, is *through the indirect agency of those circumstances which naturally possess the power of influencing them*, and the management and control of which are, to a certain extent, within his power;"—and "*the laws of the animal economy will never be explained by chemistry, or by any other means.*"

These observations apply equally to the next four paragraphs, the whole of the objections in which are founded on the erroneous assumption that I am ignorant of, or neglect the other departments of physiology, because from the nature of my subject I am not led to mention them.

d. Dr. Philip next attacks what I have said on the powers of chemistry, when sufficiently understood and rightly applied, of "directing the physician generally what to do and what to shun;" and after stating a variety of objections quite beside the point in question, ends by observing, "Dr. Prout may say, that chemistry will assist in directing his choice of the means by which these effects are produced: undoubtedly it will: here chemistry is in its proper place." But then, (just as if I had laid down the rule, "neglect the laws of the animal economy, and attend only to chemistry") he lapses into the old story, and exclaims, "but of what use would this chemical knowledge have been if the laws peculiar to the animal economy had not in the first place supplied the indication of cure." I observe in his last letter, the doctor recurs again to this point, and places in juxtaposition the above observation, and one in which I have elsewhere said that I do not know a single instance in which chemistry can be *directly* applied to the purposes of the animal economy, as if the two were in opposition to each other. But the fact is quite otherwise. The absurd idea, indeed, never entered into my head that chemistry at any

time will never be capable of being *directly* so applied ; but that it will be capable of doing all that I have anticipated in an *indirect* manner, I have not the least doubt.

e. The last point connected with this part of the subject which I think it worth while to notice is, "what opportunities has the chemist of ascertaining the numberless chemical changes which take place in disease : can he operate on the internal organs and their juices as in an animal or vegetable product separated from the organic body which produced it?" When Dr. Philip made these remarks he was evidently thinking of the pots and crucibles, and clumsy chemistry, of a former age. And, indeed, it is evident, not only from this, but from the whole tenor of his remarks, that he is totally unacquainted with the refinements and resources of modern chemistry, particularly as regards its application to organic matters.

7. I come now to the last part of my opponent's observations, "the application of chemistry to the treatment of diseases;" and here as before, he is radically wrong from beginning to end. The clue already given may be here applied pretty much in the same manner as before ; I shall, therefore, think it necessary to notice only a few of his remarks.

a. "Dr. Prout forgets that the chemical changes which take place in the animal body are but the subordinate parts of its various functions, &c." To which I answer that I *do not* forget this. Had the doctor taken the pains to read what he professes to criticise, he would have seen that I allude to chemistry as a means of enabling us "to associate the fleeting phenomena of disease with the more tangible and intelligible phenomena of matter," but not of directly suggesting remedies for them. The phenomena of disease I study like the doctor or any other individual, and the only difference between us is, that I associate these phenomena with chemistry, (in the same way that others associate them with the state of the pulse, tongue, &c.) and as far as I am able, ascertain what deviations from the healthy state of the solids or fluids accompany the symptoms. Now these deviations often present a means of diagnosis infinitely more certain and instructive than all the others put together, especially in dis-

eases of the assimilating functions and certain constitutional affections.

b. "One of the greatest errors of Dr. Prout's pathology is that his principles lead him chiefly to look at the state of the fluids;" and "the tendency of his doctrines is to bring us back to the humoral pathology," and "lead to its worst results." The old idle dispute about solids and fluids, as these terms are understood in speaking of living animal bodies, has always appeared to me to be little better than downright nonsense, and to be founded in ignorance of the real nature of the subjects of dispute. What sort of a *solid*, for example, can we conceive that to be which, like a piece of muscle, contains upwards of seventy-seven per cent. of water? And by how much can it differ from the *fluid albumen ovi*, which contains only three per cent. more? And besides, do we not see that all the phenomena of life can be equally associated with the *fluid albumen* as with the *solid muscle*? Solidity and fluidity, speaking with reference to living products, have reference chiefly to some mechanical object, (e. g. the *fluid blood* to circulate in tubes—the *solid muscle* to move an inflexible lever, &c.) and have little or nothing to do with the mere circumstance of life, which can be equally attached to both states. Indeed, both states are equally necessary to the existence of the animal economy ; and were we to judge from various circumstances, and particularly the mere quantity, we should be inclined to think the fluid state was the most important of the two. Lastly, in a chemical point of view, in examining an animal product, the mere fluid or water is put out of the question, our attention being directed solely to the solid matters contained in solution in it. As to the *tendency* of my doctrines, I shall only say, if they are *true*, there is no fear of their leading to *error*.

c. In speaking of diabetes, Dr. Philip says, "I have within the last twelve months had the satisfaction to see a case of diabetes mellitus cured, and as far as I know, permanently, by *minute mercurial doses*, combined with other means of strengthening the digestive organs, but none of these means had any relation to the state of the urine." Had Dr. Philip given us the specific gravity of his patient's urine *before* and *after*

the said cure, some judgment might have been formed on the point. As it is, he may be assured that no one who knows any thing of diabetes, will for a moment attach the least credit to his history, at least I do not. As to the insinuated *cui bono*, (of what use is a knowledge of the state of the urine, when I can cure this and half the diseases of humanity with a little blue pill?) this is just what might have been expected from my opponent.

d. Dr. Philip terminates his observations by saying, that "the tendency of chemistry is to narrow our views of the animal economy, and lead in pathological reasonings to a neglect of the laws peculiar to it." Putting out of view the paradox contained in this observation, that what increases our knowledge is calculated to narrow our understanding, the general argument, if it has any weight, applies with double force to the man who confines his attention principally to the metaphysical portion of the economy he has to deal with, to the neglect of the physical and chemical.

I have now done with this subject, which I must leave to its fate. That my opponent, like all men of words, will have the last word, I have little doubt, but without some extraordinary provocation, it is not my intention to notice what he says. The controversy has ended like most controversies do, by probably convincing neither party, at least, I am sorry to say, this is the case with me. Dr. Philip has, indeed, talked largely of errors, &c.; but I think I have shewn that in general they have all been of his own creation, or have had no existence but in his own imagination; and after due consideration, I cannot admit that a single fact, or even opinion, has been shaken by any thing he has advanced. With respect to the feelings under which my former notices of his observations were made, I can assure him that they were not those of pride or of anger, as he supposes, but of something very different. It has been a rule with me through life never to attack any human being; but when I am attacked, and it becomes my duty to defend myself, I do this as well as I can; and on such occasions never hesitate to speak my mind. If the doctor, therefore, has got himself offended in this matter, he has to thank himself; for when he so far for-

gets what he is about as to make an attack upon me a pretence for bringing forward his own discoveries; when he advances paradoxes, and quarrels with me for not understanding them; when, instead of fair criticism, he brings forward all sorts of irrelevant charges; I say, when he does all this, can he reasonably expect to be "spared" from certain expressions of ridicule or contempt which such a mode of proceeding is calculated to call forth, and which he so feelingly describes in his last letter, as being so "melancholy to witness?"

W. PROUT.

Sackville-Street, Oct. 7, 1831.

CHOLERA.

To the Editor of the London Medical Gazette.

Exeter, October 1, 1831.

SIR,

I PROPOSE, with your permission, to submit to your numerous professional readers some remarks which tend, I think, to illustrate the mode in which *cholera* may, with most probability, be supposed to be propagated. You have, with great propriety, made it an especial object in your late numbers to put the public in possession of the best information in relation to this painfully interesting subject, both in regard to the facts of the case and the opinions which are to be deduced from them; and this being your avowed design, will serve me as an apology for placing the following thoughts at your disposal.

In a general view, epidemic diseases may be supposed to be propagated or diffused in one of the three following ways. 1. By corruption of the air, breeding a *morbific aerial poison*, independently of the contagion of previously existing disease. 2. By *contagion*—that is, the reception into the persons of the healthy, whether by *contact* or *otherwise*, of morbid poison generated in the persons of the sick. 3. By the union of both these modes, when the original corruption of the air engenders in human beings a species of disease which, being thus engendered, is afterwards capable of propagating itself by

contagion. Some further variations of circumstances may be imagined, as, for instance, that of a reciprocal infection of the air itself from the persons of the diseased; but as I am not writing a professed treatise on the subject, the distinctions made above will be sufficient for my present purpose.

1. If the corruption of the air in any case be that of the *atmosphere at large*, it must be inferred that such corruption will inflict disease *almost simultaneously* on a number of individuals throughout a large tract of country. I am not aware that such a result accords with experience in the history of any epidemic.—The late influenza spread as rapidly and extensively as perhaps any disease on record. Yet in this the affection was often found prevailing in one town or village many days, or even some weeks, before it commenced its attacks in others not many miles distant. This fact seems clearly to indicate that the state of air which so affected individuals was *local*; and that it did not exist, at least not in such intensity, as to be capable of producing the disease throughout the *atmosphere at large*. As respects the cholera, it is evident that its progress is much slower than that of the influenza, and therefore can still less be supposed to be immediately produced by a *general* corruption of the atmosphere. As far, then, at least, as regards diseases of this character, if they are to be accounted for by the supposition of a poisonous air, we are driven to the hypothesis of such a state of the air being *local* or *partial*, and at the same time spreading by a certain progress from place to place. Now such local corruption of the air in epidemic diseases may be referred to one of two causes, according as we suppose it to be always originally generated in or near the spots where it exists, or to have infected those spots in consequence of a spreading contamination derived from other neighbouring places. In the first case, setting aside the idea of mineral exhalations, we must ascribe the unusual local production of aerial miasma to something peculiar in the general constitution of the seasons or of the atmosphere. This hypothesis seems well fitted to explain the extraordinary prevalence of intermittent and remittent fevers in certain seasons, in districts infected by marsh miasma. But it does not appear to meet the case of a disease which,

like the cholera, is found to spread by a gradual and regular progress, over a varied and extensive range of country, and through a succession of seasons.—Putting, for the present, the hypothesis of contagion aside, I am at a loss to conceive of any probable mode in which such a disease can be supposed to be propagated, except that of a progressive contamination of the air itself, creeping onward with varying measures of force and velocity, and in varying directions, according to local circumstances, from place to place. If asked on what principle I propose to explain this progressive contamination, I should answer, that it may be explained by supposing some process to take place in the air analagous to *fermentation*. I should not suppose such a process to exist among the proper elements of the air itself, but in the stratum of animal and vegetable effluvia, which, though invisible, unquestionably exists in a state of solution or suspension, in the portion of the atmosphere which lies next the earth. And as these matters, in kind and in concentration, must greatly differ in different spots, and therefore may be supposed to present the requisite materials for the peculiar fermentative process in one place more than in another, we see a reason why the disease thence arising may be found taking an irregular and seemingly capricious rout, and especially why it may manifest itself with deadly virulence in large and ill-conditioned towns, while it spares its neighbouring country. Moreover, the manner in which cholera develops itself, generally attacking great numbers, and with fatal violence, almost at its first onset, and then declining and finally disappearing in a given place after a few weeks, seems well explained on this hypothesis, as a fermentative process would naturally proceed in this manner. Nor can I conceive in what other way the sudden transition of a town from a state full of disease and death to one of health, while in the meantime a neighbouring town has experienced a change exactly the reverse, can be explained.

Supposing the foregoing hypothesis to agree with reality, will the cholera reach our own country? This interesting question I think we have not the means of deciding. A fermentative process in the air might extend itself to us in two ways. The engendered poison might be *wafted* over to us, and might

proceed to leaven congenial spots on this side of the water. For although experience seems to teach us that the air of the open sea never contains the choleric poison in such intensity as to induce the disease in a man previously uninfected, yet that is no proof but that there may be enough of it diffused to act as a leaven on the land air of an island to which it may be carried.— Nevertheless, the supposition of the seeds of this fermentation being so extensively diffused, seems to be negatived by the slow actual progress of the disease, which in such a case could hardly fail to have been diffused by the easterly winds over the whole of the continent long ago. Another supposition is, that the stratum of air across the channel may itself be capable of taking on the fermentation in a sufficient degree to propagate it to our island, although not infecting those on the sea with such a dose as to induce disease. The observation of facts alone seems capable of deciding this question; and we are not, I believe, at present in possession of any which are sufficient for this purpose.— The disease has not yet crossed the Baltic to Sweden, it is true; but the Baltic is much wider than the straits of Dover; and what was the fate of Ceylon in India? If I rightly remember, it was not exempted. Unless, however, the hypothesis of contagion were absolutely negatived, this fact would not be of much weight. On the whole it seems to be rather a forlorn hope, should the cholera reach the opposite shores of the channel, that that narrow sea will be sufficient to arrest the progress of an aerial contamination which has taken so extensive a range. Notwithstanding, it cannot be denied that so happy a circumstance is *very possible*, and that our efforts to avoid the importation of that disease in another manner, ought not, therefore, to be relaxed.

2. This leads to the consideration of the second mode in which I observed that an epidemic might be propagated: namely, contagion. On this point, however, it is not, at present, my intention to enlarge: it is evident that the question of the contagiousness or non-contagiousness of cholera, is one on which those who have had the best opportunities for observation, have found it difficult to arrive at a satisfactory conclusion. In the meantime, however, we may observe, that it is one thing to

maintain the contagiousness of cholera, and another to maintain that the disease spreads, principally or entirely, by virtue of that contagiousness. I am inclined to think that but few well-informed medical men incline to the latter opinion, however they may be disposed with respect to the former. In India, certainly, the contagiousness of cholera was much less thought of than it has been in Europe; but it seems to me not an improbable explanation of this fact, that the disorder may be contagious in the stage of febrile re-action, though not in its first stage of coldness and depression; and as the febrile stage appears to have been much more strikingly developed in the European epidemic than it was in India, such a difference in the experience of the two regions would be a necessary result. Add to this the well-known fact, that in India contagious typhous fever is a disorder which hardly occurs. If cholera is contagious, it is in all probability only *as a fever* that it is so; but on the other hand, as all other epidemic continued fevers have been found in one latitude to possess a contagious power, there appears a considerable presumption for the opinion that cholera, when assuming this form, may possess such a power likewise; and I think there is much in the statement of the facts which have been transmitted to us which would tend to confirm this opinion.

3. An epidemic, spreading both by corruption of air and by personal contagion, is probably a common form of disease, while it affords an occasion of interminable controversy to those systematists who cannot enlarge their views so as to embrace more than the one or the other of these hypotheses exclusively. I have already intimated an opinion that cholera is, in Europe at least, a disease of this description; for however disagreeable it may be to entertain a conviction that our dreaded enemy is thus provided “with two strings to his bow,” it appears to me that the history of its progress is best explained on that supposition. Admitting, however, the probability that the *fever* of cholera may be more or less contagious, I would by no means advocate an opinion that such contagiousness is the principal cause of its progress, inasmuch as the detailed history of its progress does not appear at all to coincide with such an hypothesis. So far from it, that the

cholera does not appear capable of supporting itself in a given place more than for a very short time ; so that, whether contagious or not, we are almost constrained to believe that a local corruption of the air must in all cases be necessary to its popular diffusion. Unless, therefore, that corruption of the air should previously have extended across the sea to this country, it seems very questionable whether, even if imported, its contagion would be able to diffuse itself among us ; unless, indeed, the miasma from the patient be in itself capable of acting as a leaven of corruption to the air.

Should you deem the foregoing remarks deserving insertion in your excellent journal, you will gratify

Your constant reader,

T. F. BARNHAM.

CHOLERA IN TURKEY.

To the Editor of the London Medical Gazette.

117, Crawford-Street, Portman-Square,
October 1, 1831.

SIR,

THE enclosed is extracted from a letter received a few days ago from Constantinople ; the writer being non-medical, a member of a mercantile firm.

Yours most respectfully,

THOMAS BUSHELL.

The cholera had spread very considerably soon after the 10th instant, varying, however, very much in its force and effect in different situations, and had proved very fatal in all the low and confined places, particularly at Galata and Annout Kani on the Bosphorus ; and, perhaps, nearly 200 were taken off per day for several days : the species of cholera is, however, of a much less fatal nature than that of Russia, and when taken in time by bleeding and warm stimulants, the attacks have in few cases proved fatal. At Therapia nearly 200 persons have been attacked by a species of cholera ; but, from the healthiness of the situation, not above 10 or 15 have died : in other places all the inhabitants have fled. Its ravages have chiefly been confined to the poor Armenians and Greeks, and but few respectable Franks have died : among the English, only Mr. Canning has been

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dangerously attacked, (the 13th instant,) and had not immediate medical assistance been procured, he would certainly have been taken off. The symptoms are—headache, vomiting, violent spasms in the stomach, and cold and *cramp* in the hands and feet, and stoppage of the circulation of blood. The attacks come on suddenly, and people fall apparently dead with them in the streets, and are only recovered by immediate bleeding, rubbing of the limbs violently with warm cloths dipped in brandy and vinegar, and drinking warm brandy and water, with about 20 drops of laudanum, and the application of *hot tiles or bricks* to the stomach and feet : a warm bath is likewise strongly recommended when procurable. But in most cases bleeding alone has been sufficient, when done immediately. I believe several people have died from excitement and distress, caused by the fire at Pera.

TREATMENT OF BURNS.

To the Editor of the London Medical Gazette.

North Shields, Sept. 30, 1831.

SIR,

HAVING recently seen Mr. Thomas's Essay on the Treatment of Burns and Scalds, I have been induced to send you an account of the plan I have pursued for several years, which I beg you will insert in your valuable Gazette ; and as it is not my intention to write an essay, but merely to promulgate what I have found to be a most successful mode of practice, I shall be as plain and concise as possible, briefly stating the advantages this plan possesses over other modes of treatment. In the first place, it sooner affords relief to the sufferer, who seldom fails to express the comfort afforded by the application ; secondly, in most cases it heals the burn, however deep, without inducing suppuration, and never gives rise to those contractions produced by the cicatrization of burns : and lastly, the simplicity of the plan itself is no small recommendation.

Having enumerated the advantages to be gained, I will at once proceed to the mode of treatment, which requires a little variation according to the situation of the part injured, as it is necessary, for the full attainment of the objects

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before enumerated, that the injured parts should be subjected to no pressure. I shall, therefore, begin with burns of the face, which are by far the most manageable of all. Being called, then, to a case where the face is burned either seriously or slightly, I immediately administer hot wine and water, so as to produce as speedy a re-action as possible; and if the burn is deep and extensive, I have the patient placed in a well warmed bed; and having put $\mathfrak{z}\text{i}$. of the ung. resin flavæ, and $\mathfrak{z}\text{ij}$. of the ol. terebinthinæ, in a tea-cup, it must be melted over the fire, and when reduced to a liquid state it is to be applied, like a varnish, with a soft brush or bunch of feathers, over the injured surface; and this is to be continued until the parts are thickly coated, so as completely to exclude the atmospheric air. Care must be taken to renew the coating as often as may be necessary; at first perhaps two or three times a day, but afterwards partially repairing it once a day is all that is requisite; and in this consists the whole of the local treatment. If the burn has been deep, of course a slough has to separate; this will be found to take place, or rather to commence to take place, at the end of from ten to fourteen days, according to the depth destroyed. As it separates at the edges, it curls up like a piece of shrivelled leather, which curled edge may be cut off with a pair of scissors, taking care to renew the coating wherever it is displaced by the edges of the separating slough; and so it will proceed until the whole of the slough has come away, when it will be found that the cure is complete, the parts having granulated and skinned under the slough, which has only separated as that process has been completed. The new skin will be found to be perfectly smooth, there being no contraction of the features; and in a few weeks the face will recover its natural colour, leaving no vestige to indicate that such an injury had ever been sustained.

I shall next proceed to burns of the throat, breast, abdomen, and interior parts of the legs and thighs, in all of which precisely the same plan is pursued, only placing a frame of basket-work over the patient, to prevent the bed-clothes coming in contact with the injured surface, which must have no immediate covering except the application

with which it is coated. In burns of the upper extremities the same sort of contrivance will perfectly answer to preserve the part from even the slightest pressure.

The greatest difficulty to be encountered in following up this plan of treatment is when the shoulders, back, loins, breech, &c. are the injured parts, when, of course, it is impossible to defend them from encountering considerable pressure, and in such case it is impossible to prevent suppuration taking place, to a certain extent at least. The plan I pursue in such a case is, after coating the surface in the usual way, to cover it over with a very thick layer of cotton wool, and so securing it by drawing a sufficiently large cloth pretty tightly around the body; and this should be allowed to remain without removing for three or four days, when the operation must be repeated, unless the oozing of discharge, or a very offensive smell, renders it advisable to open out the part sooner, when it will be found that the suppurating surface will be very considerably less than if dressed in the ordinary way, besides healing in a much shorter period of time.

With regard to the constitutional treatment, as I before mentioned, I always give hot wine and water, or hot brandy and water, so as to induce a speedy re-action, which being once established, the treatment should be strictly antiphlogistic, attending particularly to keep the bowels moderately open, on the one hand, and to avoid too active purging on the other. Castor oil I have found the best medicine in these cases; saline purges I have often found to produce cold shiverings, and a disposition in the injured parts to suppurate.

I remain, sir,

Your obedient servant,

EDWARD GREENHOW.

P.S.—Should the application be found too soft, it is easily remedied by the addition of a small portion of rosin and yellow wax.

ANALYSES & NOTICES OF BOOKS.

L'Auteur se tue à allonger ce que le lecteur se tue à abrégér."—D'ALEMBERT.

Practical Observations on Prolapsus of the Rectum. By FREDERICK SALMON, F.R.C.S. Author of a Practical Essay upon Contraction of the Rectum, Piles, &c.; Senior Surgeon of the General Dispensary, Aldersgate Street.

MR. SALMON finds fault with the usual designation of the disease of which he treats, viz. prolapsus ani, and substitutes for it that of prolapsus of the rectum;—we have no objection. Farther, he protests against the practice of applying ligatures, either here or in piles, and proposes a method of his own. This is the point described, and to this we shall confine ourselves; for we quite agree with Mr. Salmon, that the subject is scarcely fitted for "theoretical ideas and metaphysical reasoning"—metaphysical reasoning about the anus!

The morbid anatomy of prolapsus is first considered; and the reader is reminded that the peritoneum, after it has given an investment to part of the bladder, is continued from the lower and posterior aspect of this organ, to the anterior surface of the rectum, a sort of pouch being formed by the portion of the membrane which intervenes between them. For some short space the peritoneum is continued upon the anterior surface only of the rectum, while higher up it encircles the bowel, and then is reflected to the sacrum. The portion of the rectum which is not covered with peritoneum, is connected with the adjacent parts by cellular membrane only; and thus, while the upper part of the bowel is kept in its proper place by the investment of the peritoneum, and the connexions of this, the lower part is comparatively free, and sufficiently lax to admit of partial eversion. When the rectum is long in a state of irritation, the uniting cellular tissue between the external and internal coats is increased, and the mucous membrane becomes thickened and elongated; and both these changes are usually present when any considerable prolapsus of the bowel takes place. If no means be adopted for its relief, the eversion continues to increase until the entire lower portion of the gut may be protruded. The

causes of prolapsus of the rectum, according to our author, may be either constitutional or local. Among the most common of the former are habitual constipation and diarrhœa; of the latter the most common is a contraction of the gut itself, offering a permanent obstacle to the action of the intestine, and preventing the free return of blood.

The author argues against the employment of ligatures in the removal of prolapsus, and holds, that if the danger of bleeding could be got over, no one would hesitate in giving the preference to excision. The method adopted by Mr. Salmon to accomplish this desirable object, is as follows:—

"The patient having been prepared for its performance in the same manner as for the division of the sphincter, the prolapsus is to be brought in view by the efforts of the individual, promoted by the use of an enema. The sufferer being placed in a convenient position, an assistant separating the nates, one or more of the pins, as may be necessary, is to be passed from above downwards, transversely through the basis of the tumor; these penetrating the muscular coat of the bowel, will prevent the return of the intestine after the diseased part is removed. The pain produced by this part of the operation is insignificant.

"The prolapsus being thus secured, the operator, with the hook or the forceps, is to lay hold of one of the prominent portions of the tumor, and to draw it gently towards the opposite side; with one stroke of the scissors he is then to remove the part as deep as the line of division between the mucous and muscular coats of the rectum, the latter of which should be left entire, otherwise a permanent difficulty of relieving the bowels will follow the operation. In like manner all the protruding portions of the prolapsus are in succession to be taken off."

And again:

"If any material bleeding occurs it is to be checked by the means generally used for stemming hæmorrhage, such as cold or astringent washes. In most cases the flow of blood, which it is better to encourage to a certain degree, will cease spontaneously; but if we are compelled to apply any ligature, it may be done with facility, as, while the pins remain in their situation, we have a commanding view of the parts.

"It is my custom to leave the pins

in their place for an hour or more after the operation, or the cessation of hæmorrhage, to permit the blood to coagulate in the extremities of the divided vessels, by which we prevent any recurrence of bleeding after the bowel is restored to its natural situation. Having removed them, the surface of the divided part should be smeared with sweet oil, and the rectum returned within the sphincter in the gentlest possible manner.

"The after-treatment of the case is usually simple, requiring only that the patient be kept in a state of perfect quietude, and supported upon the most sparing diet, of a liquid description; so as, if possible, to avoid the necessity of passing any evacuation for the first two or three days. At the expiration of such time, the action of the alimentary canal is to be induced by the administration of a mild purgative, assisted during its operation by an enema."

If any serious constitutional mischief should follow, it is to be met by vigorous treatment; but the author states, that he never knew any formidable consequences to result, when due attention had been paid to the preliminary cautions enjoined.

A Manual of Midwifery, or Compendium of Gynæcology and Pædonosology; comprising a new Nomenclature of Obstetric Medicine, with a concise Account of the Symptoms and Treatment of the most important Diseases of Women and Children, and the Management of the various Forms of Parturition. Illustrated by Plates. By MICHAEL RYAN, M.D. Member of the Royal College of Physicians in London, &c. &c. Third Edition. Price 12s.

WHEN the first edition of this work appeared, now some three or four years ago, though we smiled at some parts of it which savoured of the ridiculous, we yet declared it "to contain a great deal of information." We observe that much of the absurdity is retained, as regards the attempt to introduce a set of Greek words—as *gynæcophysiology*, *geneosology*, *encyonatalogy*, *chiragotocia*, and fifty others which are not, and we trust never will be, in use. It is evident, from the *naïveté* with which the author speaks of a journalist who praises him as "the first and best of medical reviewers in exis-

tence," how highly he prizes commendation; and they who do so are, for the most part, equally galled by censure. Having, therefore, on the occasion above alluded to, been more severe than is our wont, we shall not add a word of reprehension now. Nay, to do Dr. Ryan justice, there is more information on the subject of midwifery and female complaints, in the Manual before us, than in any other work of three times its size to which we can refer. This is no mean recommendation, and we are satisfied that it is no more than is deserved.

The elementary nature of the work renders it wholly unfit for analysis. Towards the close of the volume there are a good many typographical errors*.

Reports of Medical Cases. By DR. BRIGHT. Price 9l. 9s.

[Continued from page 16.]

Inflammation of the Membranes of the Brain.

WE resume the instructive and agreeable task of analyzing Dr. Bright's recent work; and the next subject which presents itself to us is arachnitis, with effusion into the ventricles. Cases are given in which this combination ensued, both where the inflammation had come on suddenly, and been rapid in its progress, and where it had been slow and insidious: under both circumstances convulsions, paralysis, passing the evacuations in bed—in short more or fewer of the symptoms regarded as indicative of pressure, manifested themselves. In one case, where the patient was thirty-four years of age, and laboured under symptoms indicative of commencing effusion, of a very formidable kind too, a cure was effected under the use of repeated blistering on the nape of the neck, and a grain of calomel every night for five weeks.

From these the transition is easy to

Cases illustrative of Hydrocephalus.

The two first cases are examples of this disease resulting from blows. In some of those which follow, the evidence of inflammation, as adduced either from the symptoms or post-mortem examination, is of a very equivocal de-

* As this sheet was passing through the press, we received a note explanatory of the circumstance alluded to; the errors are to be corrected in the rest of the impression.

scription. Irritation, in the opinion of our author, is sufficient to cause long-continued irregularity in the muscular actions of the body, and may possibly even give rise to effusion—more especially in those predisposed to hydrocephalus. In several of the cases there would seem to have been effusion, though the patients ultimately recovered; and these favourable results followed very moderate local depletion, cold lotions to the head, and small doses of calomel, with chalk, soda, or ammonia, internally.

The twenty-fifth case is a curious one. A child, twenty months old, who had been observed to have what was thought a trick of knocking his head against persons or objects of any kind, suddenly awoke screaming dreadfully, and appearing to have pain in the head. Leeches, calomel, jalap, and so forth, were employed; but the case went on, and is detailed at considerable length. Strabismus, occasional convulsions, and so forth, manifested themselves, and the child died at the end of about a month. The appearances on dissection were curious, and may be regarded as indicative of a state of congestion and stagnation in the vessels, rather than of acute inflammation. It should be mentioned, that at the time the child was seized, it had laboured under cough for some days.

“On taking off the scalp, the bone had a transparent look as belongs to the age, and a rather dark colour. The dura mater attached pretty firmly to the skull, and when first seen, the bone being removed, looked distended, as if it had been forcibly pressed upwards. On raising the dura mater, all the larger veins on the surface of both hemispheres running into the longitudinal sinus were seen round and hard, quite filled with yellow-coloured coagulum, as if injected with wax, while the whole vertex was covered under the membranes with dark extravasated blood; this was of a deep purple at the summit, and became lighter and more broken further down the sides; so that an incision made all round, rather above the cavity of the ventricles, took off nearly the whole extravasated blood. The coagulum filled the veins through one or two subdivisions in some parts; and on carefully opening the longitudinal sinus, the whole was full of a coagulum, taking the exact form of the sinus, and becoming firmer the more it was traced backwards; this coagulum was almost entirely fibrin, with a little of the red blood in parts and cavities; it was closely attached

in some of the mouths of the veins where they entered the sinus; but this was on account of the fibrous structure of the internal surface of the sinus entangling the coagulum.

The general surface of the brain where the ecchymosis had not taken place, was so far from being vascular, that it was unusually pale; but those parts of the pia mater which ran between the convolutions near the parts where the veins were obstructed, were red, and apparently vascular. On making a clean horizontal cut through the two hemispheres, a little above the top of the ventricles, a number of small round dark red spots, more venous than arterial in colour, were seen thickly distributed in patches: these were chiefly collected in a close body round the inside of the cineritious substance at those parts where the pia mater was most vascular; and the cineritious matter itself was of a darker colour than usual. In one part on the outside of the anterior lobe of the right side, the brain was completely softened, so that on pressing the finger upon it, it broke down easily, and was immediately excavated, leaving the surrounding part, in which were no red spots, obviously much more firm, and indeed natural, in texture and appearance.

“On examining the brain with a high magnifying lens, it was evident that the round specks were small coagula, and were not contained in vessels; for, in whatever way divided, they always presented the same appearance; nor could any thing further be discovered with a microscope. The vessels from which these little clots of blood had escaped could not even be discovered. The ventricles were distended with several drachms of clear fluid, and the septum lucidum and commissura mollis were so soft as very easily to break down; the membrane lining the ventricles was perfectly healthy, and free from vascularity; the posterior cornua were much distended with watery fluid.

“On exposing the base of the brain, the only remarkable circumstance was, that it appeared unusually free from vascularity, and white; and this was particularly the case with regard to the cerebellum, the cineritious portion of which was soft as well as pallid. The vessels of the base, the basilar artery, and the circulus Willisii, were perfectly healthy; there was near the optic nerves a spot where we perceived about as much puriform matter as would form one or two drops, but we did not discover its origin. In the basis was above an ounce and a half of serum; but some of this had probably found its way during the examination, from the ventricles.”

The next observation which arrests our attention relates to coagulation of fibrin in the veins during life. Two

cases of this kind are given as connected with the preceding; they are curious, and illustrate what is probably a rare pathological condition. As they are shorter than most of the others, we shall quote them without curtailment; indeed, they scarcely admit of abbreviation.

"Elizabeth Larnier, aged 17, had been long labouring under anasarca and ascites, with a pulse constantly accelerated and jerking in its beat, ascribed to disease of the heart subsequent to rheumatism, and to disease of the liver. After long and varied suffering, in which she was excessively reduced, and was in her appearance pallid and exsanguine, she became the subject of pain and tenderness above the left clavicle, where slight swelling was soon perceived, and this in a day or two distinctly to be traced in the direction of the jugular and subclavian veins, which might themselves be felt hardened like cords. The whole left arm became œdematous; her weakness and irritability increased; her pulse became more rapid; and she sunk in about ten days from the first discovery of these symptoms.

" *Sectio Cadaveris.*—The sternum adhered very closely to the pericardium; the two surfaces of the pericardium adhered generally by firm and close adhesions; the heart was large, chiefly owing to the increase of the left ventricle. The edge of the tricuspid valve was thickened, but still the auriculo-ventricular opening was very large; the blood in the cavities was but imperfectly coagulated, but the clots extended into the principal vessels; the pulmonary artery and its valves healthy. The left auricle rather thickened; both curtains of the mitral valve rather thick, but not ossified; one of them was a little contracted; some minute and soft fibrinous vegetations were attached to the edges of both curtains; the opening of the valve was very large. Aortic semilunar valves thickened, and almost cartilaginous at their convex part, so that each valve formed a bulging cup. The left jugular and subclavian veins were plugged up by a firm coagulum terminating abruptly just as they entered the cava; the coagulum was of a yellow white, with isolated portions soft and grumous; the blood was partly coagulated and partly fluid in the axillary and brachial veins and their branches, distending many of the superficial veins; the jugular above the fibrinous mass was filled with dark blood not very firmly coagulated: the coats of the vessels were healthy and smooth. The cellular membrane around the veins firm and semi-cartilaginous, and some of the axillary enlarged. The pleura adhered to the lungs, but not so generally as to prevent considerable effusion of serum; there was some recent inflammation on the right side and lower

part of the lung, and a small pulmonary apoplexy. In the abdomen were found about four pints of straw-coloured serum; and numerous old adhesions of the diaphragm to surrounding viscera. Liver contracted and firm, with an irregularly-indented surface. On the uterus was a small attached cyst. Kidneys large and healthy.

"Elizabeth Mucklow, aged 22, was admitted into Guy's Hospital in the month of December, under the care of Dr. Addison, eleven weeks after parturition, during which she had suffered from excessive hæmorrhage. She was pale and bloated, with a sharp jerking pulse, sometimes rising to 140 beats in the minute; she complained of some pain and swelling above the left clavicle, where the hardened veins were distinctly to be traced, as in the last case: the arm became excessively œdematous; and she had not been in the hospital above a week when she died almost suddenly; for though she had been from the beginning much oppressed, she had appeared relieved, and had expressed herself very cheerfully within an hour or two of her death.

" *Sectio Cadaveris.*—On examination, a firm white fibrinous coagulum, almost as hard as cartilage in some parts, was found plugging up the subclavian vein, and this extended two or three inches up the jugulars; it became thin, and more slightly attached to the inside of the vein quite at its entrance into the cava, where it ceased. All the valves of the heart had upon their edges small fleshy deposits of fibrin from the blood, forming fringes of vegetation attached with considerable firmness, so as to allow of being handled pretty roughly without being detached.

"The right auricle of the heart was distended with blood; some fluid in the pericardium; a considerable quantity of serum in the cavities of the chest."

Dr. Bright, after these cases and remarks—which may be looked upon as a kind of digression—proceeds to make some general observations on the treatment of hydrocephalus. He states, in conformity with general experience, that some degree of inflammation will usually be found to have existed, and that it is upon the earliness of the period at which this is discovered, and the readiness with which it yields to depletion, that the chance of recovery depends. But our author urges strongly, that the practitioner should not confine himself to this very limited view of the disease; nor wantonly repeat bleedings, though small, till the powers of life be exhausted. When the little patient begins to be worn out, and sometimes even in the

earliest stages, the depression and irritability are such as to require nourishment and sedatives; of the latter, Dr. Bright prefers tincture of opium, or the liq. opii sedativus, in doses, of course, proportioned to the age of the patient. He has seen four drops of laudanum given three times a day to a child two years of age, "with the best effect." The circumstances, however, were urgent, and the dose is not adduced as an example of what is generally proper. As to the treatment of those cases in which congestion is present—provided this fact could be satisfactorily ascertained during life—rather rapid, but limited, local depletion (as by cupping glasses), and the occasional sudden, but interrupted, application of cold, are suggested, with repeated blistering on the neck. The manner in which mercurials have been used in hydrocephalus is spoken of with disapprobation, as too indiscriminate, and as frequently productive of great irritation of the bowels, and sympathetic disturbance of the head. He recommends their combination with opium.

The author next directs our attention to the effects upon the brain of an inflammatory character, produced by inflammation of other parts, or by general febrile disturbance. Several cases are detailed in which cerebral disease of an inflammatory character appeared to be the consequence of diseased intestines in fever. Several cases are also given where this followed peritonitis; one where it followed erysipelas of the lower extremities; and another where it came on in scarlatina. Another source of inflammation of the brain, in a distant organ, is found in the liver; and various instances are detailed in which hepatic disease, existing with an imperfect discharge of its functions, gave rise to inflammation of the membranes of the brain.

Erysipelas of the Head and Face.

The last set of causes of inflammation about the brain pointed out by Dr. Bright, is disease of the external parts of the head. Erysipelas of the face and scalp constitutes a familiar example of such combination; and the doctor takes occasion to favour us, *en passant*, with his opinions on this disease. He strongly, and we think very justly, protests against the early use of stimulants, or, in fact, any unbending routine practice. He dislikes cold ap-

plications, and favours blisters to the neck; but what he most commends is Dr. Hobson's plan, of making a great number of minute punctures, which our author regards as one of the greatest improvements in modern medicine. The plan is thus described:—

"This consists in making fine punctures, in number amounting to several hundreds, or even thousands, with the point of a lancet over the whole inflamed part; then fomenting with warm water in a sponge, to encourage the bleeding; and repeating this operation two or three times in the twenty-four hours, if the parts look red or tense. If done early, it shortens the disease; but at all events it relieves the vessels in a manner which nothing else in my experience has effected. This remedy was unfortunately adopted too late in the fatal case I have just related: but I will insert the notes of a few cases, which are only a small part of those where I have either tried it myself or seen it tried by others."

Ten cases are given in illustration; we must limit ourselves to two.

Erysipelas of the Head during convalescence from Pneumonia.

"Juliana Pate, aged 26, was admitted under my care on the 30th of June, 1829, with pneumonia, from exposure to cold and wet, which was completely removed by bleeding, leeches to the chest, and the solution of tartrate of antimony. When convalescent, on the 19th of July she was attacked with erysipelas of the face, beginning on the nose and spreading over the whole face and scalp. On the 23d the affection was extensive; and from its attacking the lining membrane of the nose and fauces, and coming on in a patient much reduced, it bore a formidable aspect. I ordered the whole of the inflamed parts of the scalp and forehead and face to be punctured twice in the day with the point of the lancet, and to be fomented. The relief given was very marked: the delirium, which had already come on, was checked, and the inflammation subsided; the punctures were repeated the following day, with the same good effect. A day or two after, it was requisite to apply a blister to the nape of the neck, on account of a return of delirium, when the external inflammation was much diminished. From this time all the symptoms were moderated, and I was able to venture on the use of the lightest tonics."

Erysipelas of the Head in a case of Chronic Rheumatism.

"John Hurst, aged about 38, was admitted under my care on the 19th December,

1829, with rheumatism, which, after passing from joint to joint for some time, assumed a chronic character; but he was receiving essential benefit from small doses of blue pill and the syrup of sarsaparilla, so that he was able to walk without assistance, when, on the 7th of March, he began to complain of a febrile attack, attended with considerable headache and some confusion, and a slight erysipelatous appearance on the face.

Applicentur Cucurb. cruentæ Nuchæ, et detrahatur sanguis ad ʒviij.

Pilul. Colocynth. cum Calomel. gr. xv. statim.

Julep. Ammon. Acetat. cum Vin. Ipec.

March 10th.—The disease has been going on extending over the face, but in a mild form.

Habeat Haust. Sennæ. Repetantur medicamenta.

12th.—Little essential alteration.

Mist. Magnesiæ ad alvi solutionem.

14th.—He was delirious during the night; face greatly swollen and red.

Applicentur Empl. Lyttæ ampl. inter scapulas.

Habeat Hydr. cum Cret. gr. v. statim, et Ol. Ricini, ʒss. post horas quatuor.

Mistura Salina.

Let punctures be made over the whole inflamed surface with the point of a lancet.

17th.—The punctures were performed three times with great relief; the inflammation of the face is subsiding rapidly, but he still wanders a little in his mind.

Hydr. cum Cret. gr. v. statim. Ol. Ricini, ʒss. post horas quatuor.

21st.—Sumat Infusam Rosæ.

24th.—Walking about; his face perfectly restored, without any abscess or almost a trace of the disease. The rheumatic affection is subsiding rapidly, and bandages are now applied to support his ankles."

These cases will sufficiently illustrate the mode of using those means concerning which Dr. Bright expressly says that he never has seen erysipelas prove fatal, when they were used early and perseveringly; but he protests against being supposed to adduce any remedy as infallible. He represents them, and he is no enthusiast, as checking in a remarkable manner alike the local, general, and cerebral symptoms. Another observation worthy of remark is, that if the punctures be very minute, and not "lengthened into small incisions," he has never known them leave any permanent marks even on the forehead. There is generally some, and occasionally insuperable,

objection on the part of the patient to the operation, simple as it appears.

Inflammation from Purulent Discharges from the Ear, &c.

The last class of cases brought under our notice, as productive of inflammatory mischief within the cranium, are those in which there is purulent discharge from the nose or ear, generally, with disease of the neighbouring bones. Numerous examples of this kind are given, but we must content ourselves with a general analysis of them. They shew clearly that the local circumstances above-mentioned not very rarely produce the most formidable, or even fatal, inflammatory irritation of the encephalon. The ethmoid bone and frontal sinuses are certainly less frequently the seat of this mischief than is the temporal bone: one or two such cases, however, are alluded to.

The complication of discharge from the ear, with subsequent cerebral inflammation, is so frequent, that almost every practitioner must have met with it. For the most part, the discharge is first serous, then purulent, and sometimes is very fetid, even where the bone is not diseased. Occasionally the discharge goes off in a few days, and the patient gets well; in others there are headache, troublesome deep sounds, with deafness and vertigo; and if the case terminates badly, we have delirium, convulsions, and coma. The bones are generally found carious; there is ulceration of the membranes, and destruction of the substance of the brain. The dura matter is often extensively detached, and a large collection of pus is found under the pericranium behind the ear. At the commencement, local depletion and fomentations are of service; but when the disease has fairly implicated the bones, it is little under the control of remedies; nor can it ever be free from danger, existing, as it does, close upon the brain and investing membranes. In some cases, as where the mastoid cells are affected, the trephine might possibly be of use. In the case adduced by Dr. Bright (Case 66), inflammation existed in the sinuses of the brain, there having been discharge from the ear, and there being also extensive disease of the heart and lungs; this case is analogous to some of those in Mr. Arnott's paper, published in the *Medico-Chirurgical Transactions*, in which he so convincingly shews the

connexion between inflammation of the veins and certain secondary consequences.

In our next article we shall proceed to consider inflammation of the brain itself.

MEDICAL GAZETTE.

Saturday, October 15, 1831.

“Licet omnibus, licet etiam mihi, dignitatem *Ar-tis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso.”—CICERO.

THE ALLEGED DECLINE OF SCIENCE IN ENGLAND.

It cannot be denied but that those who adopted the affirmative side of the question were enabled to make out rather a strong case. They were more successful, however, in proving that science *ought* to be in a declining state, than that it really *was* so. The deficiency of royal and ministerial patronage was clearly exposed; the absence of place, pension, and title, from the list of enjoyments that fell to the lot of British philosophers, was distinctively set forth; and, in short, nothing would seem to have been wanting to a full and forcible conclusion, except the actual conclusion itself, which many unhesitatingly drew; whilst a small number of more judicious reasoners saw that there was an assumption in the case—either in the premises, or in the *res probanda*—amounting almost to a fallacy, yet far from being simple, or easy of detection. Those who were fully conscious of the error were, for the most part, as we have already stated, the very persons who most strongly felt the seal of modesty upon their lips. It only remained, then, for the learned “foreigner,” full of enthusiastic admiration for British genius, and well acquainted with the productions, as well of our school as of those

of the continent, to come forward in the ardour of his generosity, and take up the gauntlet of Mr. Babbage. With what success he has combated in the cause we need not repeat, after what we have said of it in our last number; but we may revert once more to the subject of the contest, just to make a few remarks concerning certain of its medical bearings.

It will not fail to have been observed, that the principal question at issue between the contending parties is, whether theoretical and abstract science is on the decline in England or not; or to particularize more distinctly, whether the mathematical and physical sciences are pursued with as much ardour and activity in this country as on the continent. The Messrs. Babbage, Brewster, Herschel, and South, think not; whilst the learned “foreigner” shews himself right willing and competent to support the opposite opinion. But neither party touches upon the condition of medical science in Great Britain—or barely touches on it. It is quite clear that it was not deemed advisable by the former party to attempt to corroborate the position of the decline of our science by a reference to the state of our medicine; whilst our foreign champion, we must say, lost an excellent opportunity of establishing his view of the case by not adverting to the same felicitous topic.

Let science decline amongst us as it may in other branches (and we shall not now enter into the merits of conflicting opinions on the head), in medicine it does not decline. With such names as we could enumerate—were it not invidious to single out individuals where there are so many of the very first order—in anatomy, physiology, chemistry, practical medicine, and surgery, it would ill behove us to despair of our medical commonwealth, or to rank ourselves second to any other in Europe. A most striking difference, it is true, is observable be-

tween the profession as it exists here and as it exists in France and other countries of the continent, but the balance, in point of practical utility, is with one consent admitted to be in our favour; with much less parade and affectation of profundity, we are perhaps the most of any people disposed to turn our stores of knowledge to account—and no sooner, it is observed, have we caught hold of even the simplest elementary principles, than we turn our attention to what may be the best practical application of them. This is the remark of the author of the pamphlet before us, in speaking of the comparative progress of the French and English when proceeding with the cultivation of pure science—but it is equally applicable to the same parties engaged in the cultivation of medicine. The French, moreover, may have more “pride, pomp, and circumstance,” in the externals of the profession; for instance, in the arrangements of their *Ecole de Médecine* and *Académie de Médecine* (both of them subject in every thing to the higher powers of the Minister)—in the co-operation of greater numbers in the compiling of their *Dictionnaires* and *Annales*—and generally in the mode of conducting all their scientific proceedings; yet our own countrymen, singularly enough, without those aids and appliances, have been so fortunate as to secure the palm of all Europe in the matter of unequivocal usefulness, which, after all, is the sum and essence of all that scientific men can pretend to aim at attaining.

One opinion of the author before us, when he attempts to account for the want of diffusion in England of those theoretical views which engage so much of the attention of our foreign brethren, we are not sufficiently prepared to adopt. The English, he says, are not well provided with the knowledge of foreign languages. Neither are the French; and whatever fault may be

found with us for our limited acquaintance with those “whole branches of continental discoveries,” of which Mr. Herschel speaks, it is some consolation that the same is equally applicable to our more theoretical neighbours. But our author is inclined to be more favourable to the pretensions of the German *savans* in this respect: he may be right—though we are not satisfied that he has stated sufficient grounds for such an opinion. What he says, however, of our deficiencies with regard to foreign literature, may be worth extracting. “Since the Latin has ceased, very happily in many respects, to be the common medium of intercourse of the learned in every country, the scientific intercourse between different nations has become cramped by the necessity of learning many foreign languages—at least three or four are indispensably necessary. In this respect the natives of England labour under great difficulties. The difference of pronunciation of the English language from all those spoken on the Continent, renders the task of learning a foreign tongue particularly troublesome to an Englishman; nor does he generally find, in the public schools and academies of his own country, many opportunities of receiving, in this respect, regular and solid instruction. The consequence is, that few Englishmen learn enough of a foreign language to enable them to converse freely with the natives, and to read, without great exertion, the writings of continental authors. There are countries of Europe where no young man could think of studying medicine, mathematics, or natural science, with the help of Latin only, and without being prepared, before entering the university, with a sufficient knowledge of German, English, and French. Many, indeed, are masters enough of Italian to read with ease and pleasure any scientific book in that language, whilst I have known others attain

a tolerable degree of proficiency in the Danish, Swedish, or Spanish; but in England, the number of those who acquire a smattering of French is very small, and still smaller is the number of those who know enough of German to read a book in that language without considerable trouble." Here, we suspect, our foreign friend rather underestimates the attainments of English students.

But, however this may be, we believe much of the complaint might be removed by a more unrestricted trade in foreign books: the market, in this respect, requires a thorough opening: the penalty of being obliged to pay five and twenty per cent. at the very least, to an English dealer for French books, and we believe much more to those who deal in Italian and German, cannot but constitute a serious restriction to the more general diffusion of foreign literature. Among the taxes on knowledge, so much complained of, we know not one which can compare with this for its real practical disadvantages and severity; and whenever we shall have the ear of the Chancellor of the Exchequer, we will use our earliest and best influence in that quarter for abolishing all taxes on the importation of books in foreign languages.

There is much plausibility, though perhaps something, too, that is objectionable, in that part of our foreigner's remarks where he treats of the futility of rewarding scientific men with badges of an order of merit. "In almost every country," says he, "where such distinctions exist, one-half of the ribbons are given to jobbing and patronage; and, upon the whole, we believe that the institution itself has an injurious tendency. Such crosses and badges are but too often the price for which honour and conscience are bought. These gaudy baubles are the hooks and baits by which a prey may be allured, which

could not be taken in any other way. The distribution of these distinctions must of needs belong to government: how are its members to judge of the different degrees of scientific merit? Mr. Babbage scarcely allows to government the capacity of choosing its own scientific advisers—how will it be able to discern those who deserve the 'order of merit?'" Our author must surely admit that there is a difference between choosing a scientific adviser and a suitable person on whom to bestow an order. Nor can we agree with him in supposing any difficulty to exist, granting that "the government" knows any thing about what is passing in the world around them, in fixing on the proper objects for their distinctions. At all events, considering the fewness of honours that await the members of our profession, we believe it must be admitted that the author's opinions, if applied to them, will appear to be somewhat fastidious.

An "order of merit" we should be inclined to consider as a very cheap and simple, and not an unsatisfactory mode of rewarding, or at least cheering the toils of those for whom so few good things are in reversion. They have been, however, long inured to contentment in this respect, and should there be no disposition in the government to comply with Mr. Babbage's suggestions, there will be the less damage done—they will not sink, we are sure, under the disappointment. As to the objection that the patronage of the bestowal of these honours would be liable to abuse—why, so is every other desirable thing. A baronetcy, a knighthood—nay, a peerage, may be frequently conferred on the unworthy; but is this a reason why they never should be bestowed upon men of science as a suitable mark of royal respect? Let but those who palpably merit distinction be even raised above the level of the crowd;

and it will at least secure to them the respect of the vulgar, if there be any thing in that worth having ; and the intelligent may not find it the more difficult afterwards to adjudicate them to their proper places among the learned.

COUNTY INFIRMARIES IN IRELAND.

ONE fact is worth a dozen arguments. We might be preaching for half a year against the propriety of maintaining a particular system ; yet the existence of an “exquisite reason” for its support, in the shape of a profitable monopoly, were stronger than all that we could urge. What is the use of arguing abstractedly on the grossly unfair and unprincipled system of insisting on apprenticeships in the education of the surgical profession in Ireland, so long as the monopoly of the valuable county infirmaries in this country subsists ? The *lucri odor* is an argument too powerful for any rhetoric ; and in this case it is likely to continue so till all question about the matter is set at rest. But all is not quite lost that is in danger. There is still hope of the evil being remedied by a *not-too-late* interference ; and after what we are about to say, it will rest entirely with the parties most interested to take the proper steps for asserting the cause of justice and their rights.

So all-absorbing has been the interest excited throughout the country by the recent proceedings of parliament on *one* question, that the public have been altogether lulled to the progress of certain minor measures, which at any other time would scarcely fail to create a sensation. Among these, we are informed, was a bill for the regulation of the county infirmaries in Ireland, to which a clause had been added for the throwing them open unrestrictedly for the competition of all regularly-edu-

cated surgeons of the united kingdom ; but this clause, it is provoking to add, was thrown overboard, for the gratification and by the intriguing of certain persons attached to the obnoxious system of monopoly. The bill, as it stands, would restrict the enjoyment of those fair objects of competition (that should be), to individuals who have served a five-years’ apprenticeship to members of the Irish College of Surgeons ; and this at a time when that very college has in its possession a recently-bestowed charter legitimating and professedly equalizing the rights of persons educated on the truly liberal system, which makes it unnecessary to enter into bond servitude for the purpose.

We hope this bill has not yet passed into a law ; but if it has, we trust the matter will be properly looked to in another legislative provision still before parliament, and which has reference to the subject in question—the Grand Juries (Ireland) bill, in which a similar fair and conservative clause is inserted ; but to what purpose, if measures be not taken to prevent the recurrence of similar intriguing ? We call upon the surgeons of England and Scotland, as well as the liberally-educated part of the profession in Ireland, to bestir themselves in this matter, and not to suffer a gross and egregious bargain to be struck before their eyes, without entering their indignant protest against it, and at the same time putting forth the assertion of their just claims and equal rights. A short breathing-time has occurred in the progress of parliamentary proceedings : the suspension is not inopportune, nor can be better employed than in investigating the secret plot, by which, if not timely exposed and properly prevented, the profession at large will find itself, when too late, curtailed of a serious share of its fair proportions. We shall take care to return to this subject.

KING'S COLLEGE.

THE opening of King's College took place last Saturday, on which occasion a very luminous discourse on the advantages of science was pronounced by the Bishop of London. This was followed by a very long and not particularly interesting essay on the same subject by the Principal. The Medical Classes commenced on Monday, Mr. Mayo delivering the first lecture.

In our humble opinion the proprietors would have acted more prudently not to have begun at all this season, unless they had been able to do so at the same time as the other schools: as it is, the building is unfinished, and the voice of the lecturer occasionally drowned by the hammer of the carpenter.

REPORTS OF CASES OCCURRING
AT PUBLIC INSTITUTIONS.

WESTMINSTER HOSPITAL.

Femoral Aneurism opened by mistake.

WILLIAM OAKLEY, ætat. 27, admitted Sept. 10th, 1831, under Mr. Guthrie, with a pulsating tumor, nearly circular, about six inches in diameter, and raised about three inches above the level of the surrounding integuments; the apex is about three inches below the middle of Poupart's ligament, on the left side. The pulsation is evident both to the eye and hand; and the sound is heard by the stethoscope very plainly over the whole surface of the tumor, and for about two inches above, and one inch and a half below, in the line of the artery. This is not heard on compressing the artery above. From the account given by the surgeon attending previous to admission, there is an opening at the apex, which is at present concealed by strapping, and from which about one drachm of blood has oozed since admission. On the other side is a blister, of about one inch in diameter, containing bloody serum. The limb is of the natural temperature, and highly œdematous; the artery cannot be felt either in the ham or at the ankle. Face much blanched, but calm; tongue covered with a coat of white, tinged with brown, and

moist; no thirst; bowels open freely; urine scanty, passed with difficulty; this has only existed two days; skin hot, but moist; pulse 110, but feeble; heart beats loudly, without impulse; respiration 30, natural in sound; percussion sonorous; emaciation considerable. He is a book-binder, which occasions constant standing, but has been out of employment for nearly one year: has led a tolerably temperate life, and has never suffered from any disease since infancy. The account he gives is, that between two and three months ago he first observed a beating in the groin, particularly on any exertion. This has increased ever since, and about six weeks ago he first perceived a slight tumor, which at first was not at all painful, but latterly became very much so. He has been attending at a dispensary about six weeks, and at first thought he obtained relief; but the tumor afterwards increased in size. Six days ago an opening was made into it with a lancet, through which a small quantity of coagulated blood escaped; after this a poultice was applied; nothing further happened till the 8th, when a sudden rush of blood took place, and, according to the statement of the man, he lost more than four pints. The surgeon was immediately sent for, who compressed the tumor by the aid of strapping, bandages, &c. and no further hæmorrhage has taken place. By the advice of a second surgeon, who was called in, he was brought to the hospital.

Mr. Guthrie was immediately sent for, and on his arrival, determined to defer the operation to the following morning at ten o'clock, directing the patient to be closely watched, and a cold lotion to be kept constantly applied.

Rx Sul. Opii Sedativ. ℥. xxx.

Aque fontan. f3 x.

℥ft. Haust. hora somni sumend.

Sept. 11th, seven A.M.—Has passed a good night; the tumor is as yesterday; pulse 104, rather fuller; countenance quite calm, still much blanched; skin hot and rather dry; no thirst.

Ten A.M.—Pulse 120, soft; countenance tolerably tranquil; temperature of the left ham $97\frac{1}{2}$ of Fahrenheit, of the right 101.

Operation.—He was placed on the table, the legs semi-flexed, and separated; the body being supported by the surgery-man. Mr. Guthrie then commenced the operation by making an incision through the integuments of three inches in length, beginning a little above and to the outer side of the internal abdominal ring, and carrying it perpendicularly downwards to Poupart's ligament. He then divided the tendon of the external oblique, and afterwards the internal oblique and the transversalis; raising the two latter muscles on the director, the fingers were

then carried down to the artery, pushing the peritoneum upwards: the vessel was now found pulsating, but confined by very dense cellular membrane, which strongly resisted the nail. The depth was fully three inches and a half; this rendered a slight enlargement of the external incision necessary, which was practised towards the outer side. After separating the artery, the common aneurismal needle was carried round, though not without considerable resistance, and the artery finally tied; the greater part of the time spent in this operation being occupied in scratching through the cellular membrane. A single ligature was put in at the external wound, and the man put to bed in pretty good spirits.

He gradually sunk, and died on the evening of the 13th.

Autopsy, eighteen hours after death.—The body presented an appearance of great general emaciation; features considerably shrunk; the limb presented the mottled appearance as far up as the lower part of the aneurismal tumor; phlyctenæ scattered here and there on the inside of the leg and foot; the abdomen felt very tense, owing to a tympanitic state of the intestines. The aneurism contained eighteen ounces of coagulated, grumous blood, besides the coagula adhering to the sac, which extended upwards under Poupart's ligament, in a line with the inside of the ilium. The artery was ruptured exactly where the profunda was given off. The iliac was sound, where the ligature was applied, and the internal coat was fairly divided, with slight redness on each side. The lower end of the artery was filled with coagulated blood, but Mr. G. thought that was from the extension of the mortification. About four ounces of serous fluid, mixed with flocculi of lymph, were found in the abdomen; slight inflammation of the peritoneum, extending about four inches round the incision; the intestines were glued together by lymph; there were old adhesions between the pleuræ, but the lungs were healthy; the heart, aorta, and common iliacs, were healthy.

Abstract of a Clinical Lecture on the above case, by Mr. Guthrie.

The case to which I am desirous of drawing your attention has terminated fatally, as I expected. It was that of a young man, Thomas Cabley, twenty-seven years of age, by trade a book-binder: it appears that he had some sort of swelling at the upper part of the thigh, seven years back, for which he applied to a practitioner for advice; but as it gave him little trouble, he did not

attend to it until within the last three months, when it enlarged, became painful and pulsating, and the limb proportionally weak. The surgeon he applied to poulticed it for three weeks, and, on Monday the 5th, he opened it with a lancet; but, as only blood followed, the opening was closed by compress and bandage. On Thursday the 8th, it burst out bleeding, and the man says he lost four pints of blood. This is probably an exaggeration; but, from his pale and depressed appearance, it is likely that he lost a considerable quantity. On Saturday evening he was sent to the hospital, and on Sunday morning I proceeded to place a ligature on the external iliac artery. Mortification of the extremity followed, and he died on Tuesday night, at eight o'clock.

This is the second case I have had of femoral aneurism sent into the hospital, after it had been opened. I do not believe that in either case it occurred through ignorance, but from inattention; from the practitioner having made up his mind that they were abscesses, and then proceeding in their treatment as such without further consideration.

These cases will shew you the necessity that exists for a patient investigation of disease, wherever it may take place, and more particularly when situated among parts of importance. It would have been impossible to mistake this last case, if the slightest examination had been made to ascertain whether it was or was not an aneurism; for the pulsation was distinct, and the peculiar whizzing noise was clearly perceptible to the ear. In the first case, the aneurism was in the situation of the artery, just as it passes through the tendon of the triceps; and, as the man was old, I preferred doing one operation only, and amputate the limb, but he did not recover. In the present case, the tumor extended up to and beneath Poupart's ligament, near the ilium. It was as large as an infant's head, and was found to contain, after death, eighteen ounces of coagulated and grumous blood, besides about eight ounces of adherent coagula. The artery had given way exactly opposite to the organ of the profunda. Amputation could only have been done at the hip joint, and I preferred the operation of tying the external iliac artery, which I found a difficult undertaking; so much so, that one of

the students, who has seen me do the operation several times on the dead body at lecture, expressed his great surprise at the different appearance the parts assumed. The limb could not be bent in the slightest degree, without giving the man intolerable pain, and the tumor pushed up Poupart's ligament like a bank, so that the artery, after the external incisions were made, was at a considerable depth, and perfectly tense. Bending the body only brought down the peritoneum, with its contents, so as to completely obscure the aneurismal needle, which was, however, passed under the artery, but with much greater difficulty than is usually experienced. This difficulty occurred from the fascia covering it being very strong, and from the vessel lying upon the stretch. It was secured with a single ligature, when the pulsation in the tumor ceased. The temperature of the limb before the operation was $97\frac{1}{2}^{\circ}$ in the ham, and in the opposite, or right ham, 101° . The operation was done at ten in the morning; at three o'clock p.m. the temperature of the left foot was $86\frac{1}{2}^{\circ}$; at seven in the evening it was 96° ; that of the other foot, 102° . The patient complained of pain in the thigh, and in the calf of the leg and heel, which I considered very unfavourable, and expressed my fear that mortification was about to take place. The pulse was 120; the temperature was maintained by the aid of bottles filled with hot water, and by flannel, and an opiate was administered. The next morning the mottled appearance of the leg was decisive of gangrene having supervened; and on the subsequent evening he died, the gangrene having extended up to the tumor. The external iliac, where the ligature was applied, was sound; the aorta was unaffected.

I have elsewhere given it as my opinion, that the operation on an old aneurism is more likely to be successful than on a recent one, of two or three weeks formation; but a small aneurism is always more favourable for the operation than a large one; and here it was too large—it pressed on the collateral branches; it pushed aside and pressed on the great vein, diminishing, I believe, in consequence, the temperature of the limb before the operation, and being the principal cause of the mortification which ensued. If the circulation

through the limb had been preserved, the patient had still many dangers to encounter, all depending on the opening which had been made into the sac. The first would arise from the reflux blood finding its way into the sac, and flowing through the wound: now this would not be, in all probability, of an arterial colour, it would be black, a fact of great importance, and which should never be forgotten, as it bears, in an especial manner, on wounded arteries, in which it is always so. I have seen the artery tied higher up when such an occurrence took place, but it did not, of course, succeed; and subsequently the lower end of the divided vessel, which was discharging black blood, had to be sought for and secured.

In aneurism this is not always the case, but it is frequently so. We had an instance to the contrary in this hospital some time ago, and I will take an opportunity of shewing you the preparation. A man came in with an aneurism in the leg, which had burst and distended the limb in every direction. It was thought advisable by his surgeon to make an incision into the swelling, to ascertain its nature; coagulated blood rolled out, and presently arterial blood in a full jet. This I could prevent by compression; but not a steady flow of it, without any impetus, which came from the lower end of the artery; and amputation was resorted to, but it was not successful. In this case the collateral branches were large, and the communication nearly direct, the artery having burst in the upper part of the calf. If the coagulation in the blood in the sac should have prevented this accident, still from the state of the parts, suppuration must have taken place: it had, indeed, actually commenced on the outside of the fascia forming the sac, and the whole contents would have been gradually discharged. An operation in the lower extremity of the artery would have become necessary, if the inflammation had not extended to it, so as to fill it with coagulum; and, lastly, the drain from this great hole would, in all probability, have destroyed the patient, in his reduced state. All these evils would have been caused by the error of making a small opening into the sac, and shews you the importance of a careful diagnosis.

A patient does not always, however,

die under these circumstances. A man was brought into this hospital some five years ago, with a large, deep, sloughing hole on the anterior and upper part of the thigh, precisely in the situation of this aneurism. From the history of the case, I had no doubt of its having been an aneurism, which had sloughed, and was undergoing a spontaneous cure. It was not, however, effected, for the capsular ligament of the hip-joint ulcerated, and he died from disease of the joint. On dissection, the artery was found to be deficient for several inches.

Mr. Lynn has mentioned to me, that, many years ago, a man was brought into the hospital, with an aneurism in each thigh, which burst at once, and so much blood was lost that he was left in an apparently dying state. The hæmorrhage did not return; the man gradually recovered, and a spontaneous cure of the aneurism followed.

Independent of the history of the case, and many collateral circumstances, there are two great distinguishing marks of an aneurism, the pulsation and the sound. An aneurism always gives a feeling of pulsation, unless it be filled by coagulum and be impervious to the passage of blood through it, which are rare occurrences; or when it bursts, and a quantity of blood is extravasated between the muscles, in the cellular structure of the limb, and under the fascia. In this latter case, the sound of the blood passing through the ruptured vessel may be heard, and it is peculiar.

I drew your attention several times last spring to a remarkable case among yourselves. One student thought it right to bleed another, and taking it for granted that an elastic vessel, which he felt under the skin after he had applied a bandage, was a vein, he opened it, and, on loosing the bandage, abstracted the desired quantity of arterial blood: this alarmed him, and, on becoming aware of his error, he placed a solid compress on the part, and brought his friend, two days after, to me. It was the ulnar artery, running superficially, that he had opened: the external wound was closed, and a small aneurism, the size of the tip of the little finger, had formed. On applying the ear to this, the sound emitted was similar, and just as great, as that which would fall on the ear from a bellows in a smith's forge: and I judged of the

progress towards cure through the obliteration of the artery at that part, by the gradual diminution of the sound. The application of the ear to a swelling in the situation of an aneurism is indispensable; and it will be found, I do not hesitate to say, decisive in most instances of even a very doubtful nature.

Mr. Guthrie then called in a woman labouring under aneurism of the arch of the aorta, and directed the attention of the students to the extent of pulsation under both clavicles, and the single sound, like the stroke of a hammer, which it communicated to the ear, when compared to the double sound of the heart. This woman, he said, came to the hospital about five years ago, with a small aneurism presenting at the right sterno-clavicular articulation; it was called a subclavian aneurism, and the operations on the carotid and subclavian were recommended for its cure. Mr. Guthrie believed he had been blamed for not operating, but he was even then satisfied that the disease was of the aorta. This woman has since had a child, and may yet live to have more; and, upon the whole, Mr. Guthrie believed forbearance to have been the safest and most judicious course of proceeding*.

* The case is taken from the Medical and Surgical, Mr. Guthrie's remarks upon it from the Medical and Physical Journal; from which were also taken Larrey's mode of treating fractures, and the comparison between bleeding as practised in France and England, in our last number.

BOOKS RECEIVED FOR REVIEW.

Observations on Cholera, as it appeared at Port-Glasgow, during the months of July and August, 1831. Illustrated by numerous Cases. By John Marshall, M.D.

Pharmacopœia Medico-Chirurgica in usum Medicinam Facientium, a Roberto G. Holland, Chirurgo, Concinnata.

NOTICE.

We regret much that we cannot insert any of the Introductory Lectures which have been sent to us: they are only of local interest.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, OCTOBER 22, 1831.

LECTURES
ON
THE THEORY AND PRACTICE OF
MEDICINE;

Delivered at the London University,

By DR. ELLIOTSON.

LECTURE III.—Oct. 7, 1831.

General Observations—Explanation of Terms.

I MENTIONED, gentlemen, in the last lecture, that the occurrence of death—even of premature death, and the occurrence of disease, whether leading to death or terminating favourably, were nothing more than circumstances in perfect unison with the operations of nature throughout the animal and vegetable kingdom; and, indeed, throughout inanimate nature also.

I stated that there was not only a definite period for every animal and vegetable to exist, but that the greater part were cut off before that period arrived; and not only so, but that those which were not cut off suffered more or less derangement from time to time during their existence;—that we were so dependent upon all the circumstances around us for our existence, and that these circumstances were so liable to change, to be in a state of deficiency or depravation, that it was scarcely possible to escape disease.

I mentioned that disease, perhaps, might be defined to be an aberration of function, or structure, or size, in any one or more organs, or an aberration in quality or quantity of any of the fluids, from the sound state; but that it was necessary that a certain degree of these unhealthy circumstances should exist for us to say that a person was labouring under disease; and therefore there is generally added to such a definition as I have just given, “rendering assistance desirable or necessary.” I mentioned this as a defini-

tion of disease in general; and I stated that with respect to particular diseases we usually defined them, not from their ultimate nature, but merely from their appearance—from the symptoms—that is, from every thing that was observed in an unnatural condition in a sick person: the most evident, the most constant, and the chief of these are grouped together, and said to constitute a disease. We do not generally look at the nature of the affection, but merely at the concatenation of the most important, and as far as we can judge, indispensable symptoms. Yet, if the proximate cause is evident, we may put it into the definition, or name the disease from it. For instance, certain symptoms arise so clearly from inflammation of the bowels that we do not hesitate to name the disease inflammation of the bowels, or enteritis. But we seldom know the proximate cause, and thus are content with calling certain convulsions, the falling sickness, and a certain change of colour, jaundice or yellowness.

With respect to the cause of all these symptoms, that, I mentioned, is called the proximate cause, which, when present, makes the disease, which, when changed, causes a change in the disease, and the removal of which causes a removal of the disease. According to the words of writers, *præsens morbum facit, mutata mutat, sublata tollit*. I stated, that besides this cause of the disease, which is called the proximate, those causes which produce this indispensable state, whatever it may be, and which gives rise to the symptoms, are styled remote causes; and I mentioned that these are divided into two—the predisposing, those which give a liability to disease; and the exciting, those which immediately produce it.

I mentioned a few varieties in regard to diseases; that sometimes they affect merely function, sometimes they affect structure, sometimes they affect the solids, sometimes the fluids, sometimes both, sometimes they are general, sometimes local; that they are sometimes acute, at others chronic; that they

may be continued, remittent, or intermittent; that they differ likewise in their fundamental nature; that they, of course, differ according to the cause which produces them, according to the texture which they affect, and likewise according to the organ to which that texture belongs.

I must now enter a little more minutely into the explanation of certain expressions which will be used throughout the course, because, unless they are understood, it will be impossible for me to speak intelligibly. The whole doctrine of disease together is called *general pathology*. Pathology is the science of disease—the *agroti hominis scientia*—while physiology is the science of man in health. I of course mean human physiology. With regard to pathology, I stated in the last lecture that sometimes an attempt has been made to limit the term to signify diseased structure, but certainly pathology implies the symptoms of a disease, and every thing relating to disease, as well as the particular circumstance of change of structure.

General pathology is commonly said to embrace four particulars: First, *general nosology*—that is to say, an account of diseases in general, and an account of their chief differences. Secondly, an account of the symptoms of disease, which is called *semiology*, or *symptomatology*. Thirdly, the causes of disease, which is called *etiology*. And, fourthly, the treatment of disease, which is called *therapeia*.

With respect, however, to pathology, there is not only a general but a special pathology, which respects particular diseases; and if the epithet *methodical* is attached to *nosology*, an arrangement of diseases is meant—a classification of the various affections to which we are liable. I shall first speak of what is called *general nosology*, or the consideration of diseases in general. These may be local or general: when they are local, however, they seldom continue so without producing more or less general effects. I stated in the last lecture that some contend that all diseases are local—that they are seated in some particular spot or organ; but I mentioned my dissent from that doctrine, because in scurvy, for example, all the blood seems vitiated, and all the solids, without our being able to fix upon any particular spot. Diseases vary in their symptoms, and vary in their course, according to the texture which they affect. You know that the body consists of a certain number of textures—that the various parts of the frame may be resolved into a certain number of particular tissues, or textures, as they are called. Now some diseases affect certain textures, and others affect other textures; and the symptoms of diseases, and their course, are influenced very much by the texture which is thus affected. To take inflammation, for example, we shall find that

the symptoms are very different when it affects a serous membrane, from what they are when it affects a mucous membrane. Again, the symptoms are very different when it affects the skin, or when it affects a bone. The symptoms of disease, too, and the course of diseases, vary exceedingly according to the organ. Diseases may not only attack a particular organ, but they may attack a particular texture in a particular organ, or they may attack all the textures of an organ. Now when they do attack any particular organ, the importance of that organ to the economy, the function which it performs in health, the whole set of relations of that organ, greatly influence the symptoms. For example, inflammation which may be of no consequence in the finger, may, though only to the same extent, be fatal when seated in the stomach; a little ulceration of not the slightest importance on the hand may produce death if it occur in the stomach; an affection in the brain, or in the heart, may be productive of very different results—a very different degree of danger from what it would be if seated in many other parts.

Diseases likewise vary exceedingly, accordingly as they are attended by excessive or diminished action. Some diseases are characterized by an inordinate violent action of the part; the natural state of the part seems augmented. Whatever is done in health is done ten times more in the disease, at least for a time, till the strength is exhausted, or the state gradually subsides into health. Other diseases are characterized from the beginning, or nearly so, by a great diminution of action; the natural functions of the part decline exceedingly, so that the depression is the character of the disease. If the part affected be one characterized by feeling, as the surface of the body, the feeling may become very intense; if it be one whose function is motion, motion may become very intense, and you may have convulsions as the consequence. On the other hand, you may have a diminution of feeling, perfect torpor, insensibility; and such an absence of all power of motion that the part is useless to the individual. Again, if the part be one which secretes, the secretion may be in excess; and if it cannot escape we have dropsy. The secretion, on the other hand, may be exceedingly diminished, so that in the case of the kidneys no urine can be formed, and in the case of the skin the surface shall be perfectly dry.

Some diseases, among structural affections, are attended by a great dilatation of the part—the part becomes much dilated, beyond its natural extent;—on the other hand, in some diseases, the parts become contracted and diminished. In some diseases the parts become exceedingly hardened, and, from being naturally soft, they become

as firm as cartilage, and are sometimes converted into absolute bone. Again, very firm parts will occasionally become perfectly soft, and bone will become as pliable as muscle. I myself have seen the liver, to all appearance, become softened down to a pulp in the course of three days.

Again, diseased parts will become exceedingly full, distended with fluid; and this may be general or local—it may be local congestion or universal plethora of the body. In other instances, the parts become devoid of fluid and shrink, and this is observed also generally or locally. Alterations of texture likewise take place, and alterations in the quality of the fluids. The texture—to say nothing of induration, softening, hypertrophy, or atrophy—the texture of the parts sometimes becomes perfectly different from what it is observed in the state of health. So with respect to the fluids, they will frequently be formed of a character totally dissimilar from what they should be. The urine is sometimes any thing but genuine urine, and the bile is sometimes so pale that you can scarcely recognize it as bile; and so with respect to the blood itself—it is sometimes very little more than a reddish watery fluid.

These are important and essential differences in disease; there are, however, many other varieties, which are considered more or less accidental—that is to say, which do not affect the nature and course of the disease. For example, some diseases are hereditary, and this will not at all influence the nature of their course or duration; but some diseases may be hereditary in the particular individual, and other diseases are not hereditary, not transmitted from parent or ancestor to offspring or posterity, but still are born in the individual—seem to be implanted in him at his first formation; and then they are called only *connate* or *congenital*. A disease which is connate, or congenital, may be hereditary; but it is not necessarily so. All diseases which are not hereditary, nor connate or congenital, are said to be *acquired*.

Then diseases—to speak still of accidental varieties—are sometimes primary and sometimes secondary. Sometimes the disease which occurs is the first; but sometimes after one disease has taken place it is joined by another—that is to say, exists with another, or gives such a predisposition that another is produced; but it is of no consequence to the nature of a disease, whether it is primary or secondary.

Again, diseases are sometimes said to be *sporadic*, sometimes *endemic*, sometimes *epidemic*. A disease is said to be sporadic when it occurs from an accidental circumstance occurring to any particular individual—when it is a solitary accidental case. Hence, when a person, by exposure to cold, suffers inflammation of the lungs or intestines, he is

said to labour under a sporadic disease. These are insulated cases, having no connexion with each other, not having any general prevailing cause. But if diseases occur from some general and temporary cause affecting a whole population, then they are said to be epidemic. Sporadic and epidemic, therefore, stand contrasted with each other; the former class of diseases being those which occur occasionally and accidentally, from some peculiar circumstance happening to the individual; epidemic are those which affect a large mass of persons from some general prevailing cause. Again, diseases are sometimes said to be endemic—that is to say, when they do not arise from any general influence at a particular time, pervading a population, but when they spring from certain causes fixed and inherent to a certain place—when certain places in which people reside send forth causes of disease which all persons who come to that spot are liable to, but which persons do not suffer unless they go there. This is the distinction between endemic and epidemic diseases—the former being confined to the inhabitants of a particular spot, and the latter being those which arise from a general cause, taking place only from time to time, and pervading, not a particular spot, but a considerable population.

Another difference in diseases, not at all affecting their nature, their course, or their treatment, is, that they may arise from a common cause or from a specific contagion. Some diseases are contagious and some are not, but the symptoms do not vary in their essential nature; so that we are continually very much in doubt as to whether a certain disease is contagious or not. There is nothing in the disease itself to make us say that it must necessarily be contagious, and the greatest disputes are carried on as to whether a particular affection is or is not contagious.

Diseases have likewise been distinguished according to the time of year at which they occur, so that we have vernal, summer, autumnal, and winter affections—*vernales*, *æstivi*, *autumnales*, *hyemales*. Other accidental differences of disease arise from their seat, whether they are internal or external—and then, again, as to whether they are fixed, or whether they wander. If they cease in one particular part, and appear in another, they are said to be *metastatic*, or *retrograde*, or *retrocedent*—if disease suddenly cease in one part, and another become affected, a metastasis, or change of place, is said to have occurred.

Then diseases differ according to their severity. Some are exceedingly severe, others are light—some are said to be mild, and others are said to be malignant. Those which are said to be of a malignant character, however mild they may be in appear-

nance, are sure to baffle all the resources of our art, unless we can eradicate the affected part by surgical operation, which sometimes is, but sometimes is not, possible. However, the word malignant is sometimes employed simply to denote a certain degree of severity; it is a term of rather indefinite meaning. Small-pox is said sometimes to be mild, and sometimes malignant;—and, again, a sore which is of a cancerous nature, however trifling the pain it may be attended with at first, but which is sure to go on to ulceration, violent pain, and, at last, destruction of the part, is also said to be malignant.

Diseases vary likewise as to their duration. If they be exceedingly short, so as to terminate within four days, they are called by Latin authors *acutissimi*; but if they terminate within seven days, then they are called *peracuti*. We have no English words to signify the difference between these two durations. If they terminate within 14 days, they are called *acute*; if they run on to 20 days, then you will find authors calling them *sub acute*; and some are so nice in their terms, that if they last forty days they call them *acuti decidui*; and all that run on above that period are called *chronic*. It is sufficient, perhaps, to remember, that an acute disease is considered one which terminates within 14 days—a sub-acute one which terminates within 20; for though some authors make a still more nice distinction, and call a disease sub-acute which terminates within 20 days, and apply the term *decidui acuti* to those which extend to 40 days; yet if the disease extend to or above 40 days, we may consider it chronic, at least I fancy a patient would think we were justified in so denominating it.

Diseases, again, are considered as they are continued, or intermittent, or remittent. Continued diseases are generally understood to embrace two varieties as to course; namely, diseases which pursue one uniform course to the end, and are called *continuent*—and those which relax from time to time, and are called *remittent*. You find many persons speak of continued disease as distinct from remittent, and it is so common that we are justified in using the distinction; but some make a nicer distinction, and use the word continued to include *continuent*, that is to say, one which preserves the same tenor to the end, and *remittent*, one which, although it never ceases, yet relaxes from time to time. Diseases, however, are sometimes *intermittent*; they absolutely cease altogether at intervals, and then recur again. If intermittent diseases recur and disappear at regular intervals, then they are called *periodical*. A periodical disease is necessarily intermittent; but an intermittent disease is not necessarily periodical. Intermittent embraces the circumstances of simple

intermission, no matter when or how; but it embraces also regular intermission—periodical disease. The presence of a disease which is intermittent, is called the *paroxysm* or *exacerbation*, these being considered synonymous terms, and the interval which occurs between them, is called the *intermission*, or, in case of febrile disease, the period of *apyrexia*—the period of the absence of pyrexia.

I fear being troublesome in mentioning the meaning of so many terms, but it is absolutely necessary that they should be understood before we proceed to speak of particular diseases; and I will now make a few more minute observations in regard to what I said of the causes of disease.

The remote causes I mentioned have been divided into two—the predisposing and exciting; those which are exciting, you will find are by some authors called *occasional* or *procatartie*. Generally, in England, we say predisposing or exciting, but in foreign books you will find a variety of other names employed.

In regard to the predisposing causes of disease, they depend upon a very great number of circumstances. Each particular age, for example, is predisposed to particular affections: the child is predisposed to one kind of disease, the youth to another, the adult to another, and the old man to a very different set. Climate, also, causes a great difference in the disposition to disease; the season of the year, likewise, has a great effect; the habit of the individual, as to temperance and sobriety, as to a natural or unnatural and artificial course of life, has a peculiar influence; all these circumstances cause a predisposition to different affections. In regard to some diseases there is not so much a predisposition required as a want of indisposition. For example, almost every one will take the small-pox, the measles, and the hooping-cough; so that we can hardly say that there is a predisposition to these diseases; the cases in which they do not occur are rare exceptions; and we are more justified in saying, when these diseases will not occur in spite of the presence of contagion, that the person is indisposed to them, than that those who catch them are predisposed to them—that is to say, by nature we are all more or less liable to them; and it would be almost an abuse of terms to say that we were predisposed to them; all that appears to be requisite is, that there should be no indisposition to them.

In regard to the exciting causes of disease, they have been generally classed in three orders—those which excite or stimulate, and those which depress or debilitate, and those which exert a physical or physico-chemical action. Heat, and excess in wine, are stimulating causes of disease; the depressing passions, and an excessive loss of blood, may be instanced as

debilitating causes of disease; and these debilitating causes you will find, when you read some Italian authors, spoken of as *contra-stimulating* causes. Many causes, however, cannot be said to depress or stimulate solely; they exert a peculiar action, which is not to be thus explained—it is something we do not understand. For example, malaria, which produces ague, is a certain agent perfectly unknown to us, except so far as regards the situation in which it is produced. Malaria produces a set of symptoms of so peculiar a nature, that we cannot say it is a mere depressive or contra-stimulating agent—that is to say, though the first symptoms of ague produced by it are those of extreme debility, yet if we weaken the body to the same extent in various ways, by bleeding and other means, we do not produce that train of symptoms which are known to us as intermittent fever. Therefore, we have stimulating, depressing, and physical or chemical causes, which are sometimes called *stimulantes*, *contra-stimulantes*, and *irritantes*. We must not, in medicine, be very nice about the etymology of our words—that is to say, we must employ words of a certain meaning, so as to be clearly understood; but we shall find words signifying something very different from what their etymology would lead us to suppose. Nor is this remark confined to physic; it applies equally to anatomy; and the only object should be to become understood among ourselves—to know what ideas are meant to be conveyed when certain expressions are used.

An exciting cause may become a predisposing, and a predisposing an exciting. For instance, malaria excites ague. But a person who has been exposed to malaria may not have ague till he accidentally gets wet through, and then this excites the disease to which the malaria had only predisposed him. Again, if a predisposition continues to increase, disease will happen without any exciting cause. Plethora of the head may increase till a fit of apoplexy occurs, without stooping, overloading the stomach, or any common exciting cause of a fit taking place.

Among the most common causes of disease are aberration with regard to six particular circumstances, which have been very strangely called *non-naturals*. The air we breathe, the food and drink which we take, the retentions and excretions, motion and rest, sleep and watching, and the various passions of the mind, which are all natural enough, are called by old writers *non-naturals*; and others, seeing the strangeness of this expression, have called them *things necessary to life*—that is to say, things which are necessary in order to the continuance of our existence. The great sources of disease are an aberration from that state of all these which conduces to health. For example,

with respect to air; vitiation, I need not say, produces a variety of diseases—fevers, and many others. I need not say how much our health must depend upon the wholesomeness of our food and drink, and upon a proper quantity of them. I need not say that the body cannot bear more than a certain degree of exertion—that many diseases arise from fatigue; and that, on the other hand, excessive rest and indolence produce a collection of fluids in various parts of the body—stagnation, plethora, fatal dropsy, and various organic diseases; so with respect to sleep and rest, alternations are absolutely necessary; and we every day see persons who, for want of sleep, become the victims of fever, and gradually of the most destructive diseases. So with respect to the passions of the mind; there is scarcely a disease, with the exception of those that depend upon contagion, to which they do not give rise; nor is there any one part of the body, in fact, exempted from disease through the passions of the mind. No one can imagine what a vast number of diseases, not only functional but organic, arise simply from unpleasant passions of the mind—that is to say, from grief and deep-rooted sorrow, from violent anger, from regret and chagrin, and all those feelings which are of an unpleasant character. On the other hand it is also to be remembered, that passions of the most agreeable kind, excessive joy, for instance, may have almost as injurious an effect as those which are of an opposite character.

DR. PHILIP'S REPLY

TO

DR. PROUT'S PAPER IN THE LAST
NUMBER OF THE MEDICAL
GAZETTE, ADDRESSED TO THE
EDITOR.

*To the Editor of the London Medical
Gazette.*

Cavendish Square, Oct. 17, 1831.

SIR,

I HAVE already said that I cannot stoop to Dr. Prout's language; to his epithets therefore I make no reply. I shall confine myself to those faults of the style, and substance of his paper, which interfere with the questions between us.

I expected that after so long an interval, his reply would have been in a different tone; but it is the same in its expressions, its inaccuracies, and its shifting grounds without acknowledgment.

As I send with this reply an essay, being an attempt to ascertain how far chemistry is applicable to the explana-

tion of the phenomena of the living animal, and the cure of its diseases, I shall here only make some general observations in support of the foregoing statements, and adduce a few specimens of Dr. Prout's mode of replying, and a few of the facts to which he makes no reply. It is quite evident, that from discussion with an opponent who only shifts his ground on every new exposure of inaccuracy, nothing profitable can arise. The course I am about to pursue will, I hope, place the subject of our dispute in a clear point of view, which the observations of my antagonist serve only to perplex.

His language, indeed, is generally obscure, and sometimes such as makes it impossible to arrive at his meaning. The peculiarities of his style, as every reader must remark, whether he speaks of himself or others, whether in his lectures or his replies, every where, in short, except in his account of his chemical results, where there is no room for them, are the substitution of positive assertion for all proof and even illustration, and promised for present performance. It is remarkable that in his replies he constantly accuses me of a fondness for verbal disputes, while I have been obliged in almost all of mine to complain of his dwelling almost solely on this part of the subject, and not replying to the facts I had adduced, a complaint I am now again obliged to repeat, and shall presently illustrate by instances.

The truth is, that on the one hand the discussion is constantly encumbered by the extreme inaccuracy of Dr. Prout's expressions; and on the other, as he finds it more easy to quibble about words, instances of which I am just about to adduce, than facts, it is very difficult to obtain from him any reply respecting the latter, proofs of which I shall also adduce, for I have throughout the discussion made no unsupported assertions; while on the part of Dr. Prout all the assertions are of this kind, which consequently may be, and in fact almost always are, the mere expressions of an angry man.

I am now to adduce specimens of Dr. Prout's mode of replying. A few, I believe, the reader will think sufficient, and I have no wish unnecessarily to prolong this reply.

In the 40th page, at number 4, he observes, "the next point which I deem

it necessary to notice is Dr. Philip's opinion on the nature of the nervous principle." "Does Dr. Prout (Dr. Prout here quotes my words) then consider the terms nervous and living principle as synonymous? principles that have not one quality in common?" Dr. Prout proceeds, "To which I answer that I *do* consider the nervous principle to be a living principle, and beg to ask who besides Dr. Philip does not?" I might, I believe, with great truth, reply, those who are better acquainted with the subject than Dr. Prout: but of this hereafter. Our present object is Dr. Prout's mode of replying to my question.

I accense Dr. Prout of using the terms nervous and living principle as synonymous. To which he replies, that he *does* consider the nervous principle to be a living principle. It is hardly possible to suppose that he does not see that, whether the nervous principle be a living principle or not, it cannot be synonymous with the living principle usually so called, which pervades every part of the living body; whereas it is notorious that the nervous principle is confined to a certain set of organs. For what purpose, then, does he make this reply? Is it with the hope of persuading the careless reader that he has made a correct reply?

I must beg the reader's patience while I adduce one more instance of the inaccuracy, to say the least of it, of which I complain.

I have repeatedly called Dr. Prout's attention to his new term *merorganization*, and pointed out the inconsistencies attending its composition and employment. He at length gives his reply. It will at once elucidate the nature of this reply, to ask Dr. Prout why he labours in it to confound the terms different *degrees* and different *kinds* of organization,—for, according to his present *corrected version* of what he formerly said, these terms are used indifferently. He says, "Does the organization of the zoophyte not differ, *in kind or degree*, from that of the human being?" And in another part of the same page, "admitting the existence of different *degrees or kinds* of merorganization." In the original definition, we see nothing of difference of kind. The term expresses only difference of degree. He there says, "I have provisionally adopted the term *merorganized* (*μερος, pars*

vel partim), meaning to imply by it that bodies, on passing into this state, become *partly, or to a certain extent, organized*."—Phil. Trans. for 1827, p. 375.

I objected to a word which implies different degrees of organization, and it is changed to different kinds of organization, to which the objection, of course, does not apply. He affects to think *kind* of the same import here as *degree*; for it is difficult to suppose that Dr. Prout really does not perceive that these terms are, as far as applies to our argument, of the most opposite significations, the one exposing him to the whole force of my censure, while the other as perfectly relieves him from it. I accused him of introducing a term to designate different degrees of organization, without being in possession of a single fact to prove that such degrees exist. He attempts to relieve himself from the censure, by pretending that by degree he means kind! That there are different kinds of organization, of course, no one can question. It would be difficult, I believe, in a grave discussion, to find a parallel instance of defence. Besides, Dr. Prout, in his hurry to escape the censure, forgets that this new meaning suits not the composition of his term. He forgets also, that such means could blind no man whose opinion was worth a moment's consideration. It was Dr. Prout's part either fairly to shew on what principle he spoke of different degrees of organization, or to confess his error.

So much for the meaning of his term; his attempt to vindicate his use of it replies to itself. I repeat, that if the animal merorganizes the food, it must, according to his original definition of the term, possess a disorganizing power, because all admit that the food is wholly organized when it enters the stomach; and Dr. Prout *says*, it is there rendered merorganized, and therefore, according to that definition, only "*partly, or to a certain extent, organized*."

* Dr. Prout says I am unacquainted with the refinements of modern chemistry, but, as is his custom, adduces no instance of my ignorance. The degree of my knowledge has nothing to do with the question between us. However great may be the improvement in chemical knowledge, no improvement can change the nature of the science. With regard to Dr. Prout's desultory observations on galvanism, the accompanying essay will show that, however applicable they may be to his chemical experiments, they are wholly inapplicable to the functions of the living

I shall conclude with a few of the instances in which he finds it convenient to make no reply; for it is not so easy to perplex the reader respecting facts as terms.

In the 4th page of my observations on Dr. Prout's Gulstonian lectures, I have stated, that in his summary of digestion he confines himself to the mere chemical part, overlooking all the other equally essential parts of that process. My words are, "although the gastric juice be healthy, and consequently capable of effecting the due chemical changes on the food, the functions of the stomach may, notwithstanding, be wholly deranged, and no proper chyme transmitted to the duodenum. If that portion of the food which has undergone the action of the gastric juice be not duly separated, and kept apart, from that which has not, and in this separated state duly carried forward towards the intestine—if an error in any one of these functions take place, the office of the stomach is just as certainly deranged as if the chemical part of the process had failed; yet, in Dr. Prout's observations on the office of the stomach, the chemical part of it alone is referred to, as if it were in this alone it consisted."

The present is the third reply I have written to Dr. Prout since the publication of the foregoing sentences, but I have been able to obtain no reply from him respecting the facts stated in it, and others which I have pressed on his attention.

I shall trouble the reader with but one other instance of this kind.

In the same page of my observations on Dr. Prout's Gulstonian lectures, the reader will find the following paragraph:—

"Prepared as the reader must now be for Dr. Prout's partiality to his favourite science, he can hardly be prepared for the following prediction in the 9th page of his lectures:—'I will venture to predict, that what the knowledge of anatomy at present is to the surgeon in conducting his operations, so will chemistry be to the physician, in directing him generally what to do, and what to shun.' Does not the surgeon guide his knife solely by a knowledge of anatomy—a knowledge of the parts and their relative position? Will

animal; and independently of this consideration, it is ludicrous to attempt to disprove a simple fact by any train of reasoning. See page 76.

it be asserted that, let our knowledge of chemistry be as perfect as we can suppose it, the physician could regulate his practice on chemical principles alone? Could chemistry give him any knowledge of even the simplest principles of medical treatment? Could it tell him, for example, that a copious discharge from the bowels will relieve the turgid vessels of the brain; or that that by the skin will allay the symptoms of fever? But Dr. Prout may say, it will assist in his choice of the means by which these effects are produced. Undoubtedly it will. Here chemistry is in its proper place; but of what use would the chemical knowledge here be, if the laws peculiar to the animal body had not, in the first instance, supplied the indication of cure?"

To this paragraph Dr. Prout, after having been repeatedly urged to reply to it, replies by the following passage, in the 44th page of the last number of the Medical Gazette—I say to the paragraph, for to the fact there is no reply:—

"Dr. Philip next attacks what I have said on the powers of chemistry, when sufficiently understood and rightly applied, of 'directing the physician generally what to do, and what to shun;' and after stating a variety of objections quite beside the point in question," (what are they!) "ends by observing, 'Dr. Prout may say that chemistry will assist in directing his choice of the means by which these effects are produced. Undoubtedly it will. Here chemistry is in its proper place.' But then (just as if I had laid down the rule, neglect the laws of the animal economy, and attend only to chemistry) he lapses into the old story, and exclaims, of what use would this chemical knowledge have been, if the laws peculiar to the animal economy had not, in the first place, supplied the indication of cure?"

The substance of this pretended reply may be stated in a few words. Dr. Prout's original position is, that what anatomy is to the operating surgeon, chemistry will be to the physician, in directing him generally what to do, and what to shun. When he is told that chemistry can never, in the nature of things, be to the physician what anatomy is to the operating surgeon, because it cannot direct him even in the simplest case of medical treatment; while by anatomy

alone the operating surgeon is directed in all cases without exception; he neither attempts to prove his own position, nor to disprove his antagonist's, but greatly modifies the former, and simply *asserts* that the latter is quite beside the point.

Dr. Prout continues, "I observe in his last letter the doctor recurs again to this point, and places in juxta position the above observation, and one in which I have elsewhere said, that I do not know a single instance in which chemistry can be directly applied to the purposes of the animal economy, as if the two were in opposition to each other. But the fact is quite otherwise. The absurd idea, indeed, never entered into my head, that chemistry at any time will ever be capable of being directly so applied; but that it will be capable of doing all I have anticipated, in an indirect manner, I have not the least doubt." This is one of Dr. Prout's promises; but he does not say, nor can any human being conceive, how it is to be accomplished; and as to reconciling the two passages contrasted in my last reply, and to which he here alludes, this is an attempt he very prudently declines; and thus adds another to the many instances in which I in vain look for a reply.

Dr. Prout still accuses me of not having given his favourite science a place in my Inquiry into the Laws of the Vital Functions. I explained, in a former reply, that it is only in a certain department of physiology that chemistry has any place. Now my Inquiry does not include this department; because, as I have explained in that Treatise, it is an attempt to trace the general laws of the animal economy, not to descend into the particulars of its various processes; and when Dr. Prout says, that if I had availed myself of the aid of chemistry it would have rendered my positions more perfect, of which assertion, as usual, he gives neither proof nor illustration, he only shews himself unacquainted with the nature of the Inquiry; and, indeed, has, in several of the instances which have been adduced, betrayed a very imperfect knowledge of its subject. My object was to obtain and communicate a correct and comprehensive view of the great laws of the animal economy, respecting which there had been, among physiologists, so much discussion and so much contrariety of

opinion—not to trace minutely the steps of any particular class of its phenomena, but to determine the great outline to be filled up by the more partial and minute labours of those who were to follow me. Accordingly, in one of the last papers which the Royal Society did me the honour to publish, I observed, “I am fully sensible of the vast extent of the subject, and that it is only the great outline which I have attempted to trace. If this has been accurately laid down, my object has been accomplished.”—Phil. Trans. for 1829, p. 262.

I cannot conclude this correspondence without saying that I have reason to complain of Dr. Prout, in which, I think, every candid reader will agree with me. If I was in error, why not calmly point out the error? To substitute assertion for proof, and abuse for argument, might hurt his opponent’s feelings, but could not in any other way promote his views.

Dr. Prout again refers to the origin of our discussion. I therefore again say, that it was with great reluctance, and not till after much hesitation, that I entered into it. Having devoted so much time and labour with a view to the improvement of physiology, was it not painful to be publicly told, by a person supposed to be well versed in the subject, that no advance had been made in it for 20 years, as nearly as possible the period of my labours? He says, indeed, this observation did not apply to me, because he includes physiology in metaphysics; but this he did not explain; and it was impossible either for myself or any one else to know it. Besides, I have shewn that, now that he has given his explanation of metaphysics, it will not apply to the expressions used in his lectures. I venture to maintain, that he cannot find one man who will say that he could, from his lectures, infer the explanation he has now given.

As to the note in my first communication, to which he refers, there is no body, except himself, who will not at once see, that what is there said is merely adduced as a proof of his inaccurate use of the term, and can admit of no other explanation. Two things, at all events, must be admitted, that if he did use the term in the new sense he now explains, he should have defined that sense; and it should have corresponded with the expressions used in his

lectures, which I have proved it does not.

As I have reason to hope that this may be the last reply that I shall feel myself called on to make to Dr. Prout, it only remains, sir, that I should thank you for the attention with which you have honoured my communications. I hope the essay which accompanies this letter, and which presents a summary view of the functions of the more perfect living animal, in several respects different from any other with which I am acquainted, will, if it prove to be correct, in some degree compensate for the tediousness of this reply. Nothing but a desire to maintain the truth could have induced me thus long to persevere in such a discussion.

I am, sir,

Your obedient servant,

A. P. W. PHILIP.

A REVIEW

OF

THE FUNCTIONS OF THE LIVING ANIMAL BODY;

Being an attempt to ascertain how far the Science of Chemistry is applicable to the Explanation of its Phenomena, and the Cure of its Diseases.

BY A. P. W. PHILIP, M.D. F.R.S. L. & E.

FROM the following view of the functions of the living animal body, founded on the statements in my first communication to the Medical Gazette, it will not be difficult to see how far chemistry is applicable to explain its phenomena, and assist in the cure of its diseases.

The functions of the living animal body may be divided, according to their objects, into two great classes; those which maintain its existence, and those which connect it with the external world. Corresponding to this division we have seen them divided, according to their nature, into the sensorial, on the one hand, and the nervous and muscular on the other. The sensorial alone are the source of those functions which connect the more perfect animal with the external world. The nervous and muscular alone are the direct means of maintaining its existence.

The difference in the nature of these sets of functions, is essential. There is

a chasm between them which no labour can enable us to fill up; and the attempts which have been made with this view have only convinced us of their futility.

Thus different in their nature and their objects, in their working, if I may use the expression, they are intimately associated. As the life of the animal directly depends on the nervous and muscular functions, as far as we see, the sensorial functions cannot exist independently of them; and we certainly know that, in the living animal, the sensorial can neither operate, nor be operated on, except through the nervous and muscular functions; nor are the nervous and muscular independent of the sensorial functions, because the animal has not the means of existence within itself, and the nervous and muscular powers afford no means, I speak of the more perfect animals, of obtaining the necessary aids from without, except under the direction of the sensorial powers.

As the animal becomes imperfect, and approaches the nature of the vegetable, the sensorial power dwindles; and the lowest animals can extract their nutriment from air and water, which being generally diffused, are at hand, and consequently obtained without any sensible effort on the part of the animal. His life, therefore, although not independent of the external world, is, like that of the vegetable, independent of any act of volition.

As we rise in the scale of animals, the sensorial power becomes more perfect, and in the same proportion more essential to existence. From those animals which obtain food without any act of volition, we come to those who can only obtain it by such an act; but who still, without any act of this kind, obtain the influence of the air, yet more immediately necessary to their existence.

We arrive at length at the most perfect class, which can neither obtain food nor air, except by an act of the sensorium; for in them the sensorial power is as necessary for the inhalation of the air, as the ingestion of the food*. When sensation ceases, the more perfect animal as certainly ceases to breathe as he ceases to eat.

Such appears to be the constitution of our bodies as far as our existence de-

pends on the external world, and therefore on the sensorial power. But with this world the sensorial power connects us in a manner far more extensive and infinitely more varied. It is the source of enjoyment, the end of our being.

Here, as in maintaining life, it is still through the nervous and muscular powers that the sensorium operates. Through the former it receives all its impressions, and transmits all its dictates. But the nervous, like the sensorial organs, are incapable of communicating any impression beyond the limits of the animal body itself. For this purpose the muscular system is necessary.

By its means our powers are rendered active with respect to the external world. As through the nervous system we are influenced by that world, through the muscular system we can influence it. By giving motion to our limbs, it enables us to impress the inanimate objects which surround us, as by giving language to our thoughts, to influence the sensorial powers of other beings like ourselves.

It is thus by the impressions we receive and communicate that the wide field of enjoyment is opened, and not to the individual alone, but through him to other beings, who resemble him, and who repay him in kind. Thus happiness accumulates, and with it knowledge, for the experience of each becomes the experience of all, and the savage is changed into the industrious mechanic, the learned academician, and the contemplative philosopher.

BUT our present object is not the relation which the individual bears to the world which surrounds him, but the powers on which his own existence and well-being depend; and I think it will be found on an inquiry into the nature of these powers, that we have sufficient data to determine how far they are peculiar to the living animal, or partake of the nature of those which operate in the inanimate world, and consequently how far we may indulge in a rational expectation of deriving assistance from other sciences, in tracing the laws of the science most interesting to us, that of our own bodies.

I had occasion, in my first communication, to point out the general nature of the powers of the living animal, and the relation they bear to each other,

* My paper on the Functions of the Nervous System, in the Philosophical Transactions for 1829.

deduced not from the fanciful theories which have filled so many volumes, and added so little to our knowledge, but from an extensive and very laborious set of experiments, whose subjects are not such as bear a certain analogy to the functions of the living animal, but those functions themselves.

From these experiments it appeared, that in the more perfect animal there are three distinct powers, each in their nature independent, though in their organs more or less directly dependent on the others, and more or less capable of being influenced by them.

Two of these powers, the sensorial and muscular, are evidently peculiar to the living animal. No other power bears such an analogy to them as can suggest a doubt on this subject; but the case is different with respect to the nervous power. Its effects bear too striking an analogy to many of the processes of inanimate nature not to suggest the idea, that the power which operates in both may be the same. Hence the best-informed physiologists have long looked to this power as that which probably forms the link which connects, if I may so speak, the animate and inanimate world.

In entering on the inquiry respecting the nature of the nervous power, I endeavoured, in the first place, by the aid of many experiments, to ascertain the line of distinction between the nervous and sensorial functions, that it might clearly appear what belonged to each power; for it is the sensorial power alone with the functions of which those of the nervous power can be confounded, the function of the muscular power being simple and well defined. In order to render the results the more certain, I endeavoured to ascertain this line by two different sets of experiments, conducted on different principles, the object of the one being to ascertain what functions remain after the sensorial power is withdrawn, and of the other, what functions fail on withdrawing the nervous power.

I was thus enabled accurately to ascertain what functions belong to the latter power*, and was, therefore, prepared for ascertaining experimentally, whether there be any power which operates in inanimate nature capable of all its functions; and in galvanism I found such a power.

It passes along the nerves in both directions. It impresses the sensorium; it excites the muscles; it effects the formation of the secreted fluids from the circulating blood; it preserves the structure of the various organs during the performance of their functions; and it causes an evolution of caloric from arterial blood: for these I had found to be the functions of the nervous power*, and as far as I myself and others well versed in such subjects could see, in the experiments referred to, it performed all these functions as perfectly as the nervous influence for which it had been substituted.

This seemed sufficiently to determine the point at issue, the nature of the nervous power. But it was a necessary inference, that if the nervous power be galvanism, it is not a power peculiar to the particular structure of the nerves, and ought therefore to be capable of existing elsewhere. After many vain attempts made with a view to cause the nervous power to leave the nerves, and give evidence of its presence elsewhere, this was at length effected by experiments, the accuracy of which has been ascertained by repetitions both in this country and on the continent.

This fact, in combination with the results of those experiments in which galvanism was found capable of all the functions of the nervous power, the most essential of which have also been repeated both at home and abroad, removes, as far as I am capable of judging, all doubt respecting the identity of the two powers. Thus the living animal body possesses the sensorial and muscular powers peculiar to itself, and the nervous power of which it partakes in common with inanimate nature.

In attempting to trace the laws of the two first, therefore, it is in vain to expect assistance from any other source. But if the view here taken of the nervous power be correct, it necessarily follows, that many analogies must exist between the effects of that power and the phenomena of inanimate nature, and that many of the inferences from the latter will be found more or less applicable to the former. The operations of the same power, however different the circumstances under which it

* See the statements in my first communication to the Editor of the Medical Gazette, the paper just referred to in the Philosophical Transactions for 1829, and the third edition of my Inquiry into the Laws of the Vital Functions.

* My Inquiry into the Laws of the Vital Functions.

operates, must always have much in common; and, in fact, we find that all the analogies which can be traced between the phenomena of the living animal and those of inanimate nature, belong to the phenomena of the nervous power.

Where shall we find in inanimate nature any thing analogous to the circulation of the blood, or any of the other effects of the muscular power, and still less can we look for any thing analogous to sensation, volition, or any of the other functions of the sensorial power. But when we turn to the phenomena of the nervous power, many analogies with certain phenomena of inanimate nature, force themselves on our attention; nay, there are none of its functions in which such analogy may not be traced; a circumstance from which alone the similarity of their origin might have been suggested.

The nervous power, in its impressions on the sensorium, in its excitement of the muscles, in all the chemical changes it effects in the formation of the secreted fluids, the preservation of the healthy structure of all parts of the body, and the maintenance of animal temperature, nay, in its very generation and mode of conveyance, bears a striking affinity to that world with which the living animal is in so many ways associated.

But such is the difference of the circumstances in which galvanism operates in the living animal and elsewhere, that, however striking the analogy, the results cannot be identical. We have seen it proved, that galvanism operating in the living animal under the same circumstances as the nervous power operates, produces the same effects; but it is only under such circumstances that its effects are, or, on any principle, can be expected to be the same; and any inference respecting the functions of the living animal from the phenomena of inanimate nature without regard to these circumstances, must necessarily be erroneous.

Of all the chemical powers with which we are acquainted, galvanism is the most energetic, and for this reason doubtless is employed by nature in certain functions of the living animal, where the chemical changes are at once the most rapid, the most varied, and the most complicated; but here, as in inanimate nature, its effects, as appears from what has just been said, are not confined to chemical changes; and

whether chemical or not, they are influenced by, and some of them wholly dependent on, its association with the more strictly vital powers.

Thus its chemical operations are such as no unassisted chemical agent can produce, and many of its other effects differ essentially from, and bear but a faint analogy to, any of those which belong to inanimate nature. It is the medium of the sympathy which exists in the different parts of the living animal body. It is the bond which connects them together, and thus forms them into a whole. It is through the nerves, for example, that an offensive body introduced into the trachea or the stomach excites the motions which expel it; that the obstructed skin excites the heart and bloodvessels by their increased action to remove the obstruction; and so on in a thousand other instances.

On carefully reviewing what has been said, we may form a pretty just estimate of the extent to which, and the modifications under which, chemistry is applicable to the functions of the living animal.

In the functions of the sensorial and muscular powers it has no place. Can it move a limb, or propel the blood, or perform the slightest act of the sensorium?

In part, and but part, of the nervous functions it has place. These, it will readily be admitted, are an important and extensive part of those functions; but even here its effects are essentially modified. The chemistry of the nervous power is not the chemistry of inanimate nature. We know that the changes are effected by the same chemical agent, because we have seen them all effected by that agent; but in the animal economy its effects are modified by the vital powers with which it is there associated.

The chemical effects of the nervous power are not only, in a manner analogous to its other effects, modified by the vital powers, but also in a way peculiar to themselves; which, even were the chemical laws of animate and inanimate nature the same, would greatly increase the difficulty of applying them to the functions of the living animal. In the processes of secretion and assimilation it is not only impossible for us to trace the steps by which the changes are effected, which compels us to admit the operation of certain causes peculiar to the living animal, the effects of

which, consequently, we can never even hope to see exemplified in inanimate nature; but the results of the chemical processes of the living animal, unless the resulting substances be immediately removed from the sphere of action of the vital powers, are so constantly influenced by them, that, in a large proportion of instances, even were we acquainted with every chemical change which takes place, it would be impossible to say what change is effected at any particular moment of its complicated and ever varying processes; and this difficulty is still farther increased by our very attempts deranging the natural process, by necessarily introducing a set of causes which are foreign to it. If, for example, we wish to ascertain the changes effected on the blood by respiration, the very circumstance of removing it from the vessels, as I have found in repeated trials, unless our experiments be made while it is flowing from them, causes the most essential change in its chemical properties. Yet this is a favourable instance—we can obtain the blood in the state and at the moment in which we wish to examine it. In most of the assimilating and secreting processes, it is impossible for us to determine the moment at which any particular change takes place; and if we could determine it, by what means shall we, at that moment, submit the subjects of these processes to our tests?

When, to the whole of these circumstances, we add, as I pointed out at length in a late communication, that the chemical changes take place chiefly in the fluids of the body, whereas it is chiefly on the state of the solids that the great changes of the animal system depend, that of the fluids constantly varying as their state varies, we readily see how limited must be the application of mere chemical principles either to elucidate the functions of the animal body in health or restore them when they have deviated from it.

Such is the view of the subject which our knowledge of the animal economy, deduced from observations and experiments made on the healthy animal, affords; and this view is amply confirmed by all our experience of its diseases. Let us take our illustrations from those with which we are most familiar.

Has not the chemical composition of the bile been long known to us? Has this knowledge assisted us in correcting

its derangements? If we could exactly ascertain the nature of its derangement in each particular case, and could find a chemical agent capable of correcting it, would it afford any permanent relief while the diseased action of the liver continues? On the other hand, if we look to the means which experience has taught us are most effectual in correcting diseased states of the bile, do we find any particular relation between these means and the morbid states of the bile corrected by them? They do not act by correcting the morbid states of this fluid, but by improving the function of the organ which secretes it.

The vitiated state of the bile already formed, no doubt occasions certain symptoms which aggravate the evil. In its passage, it irritates the surface of the alimentary canal, and as far as its morbid properties can be corrected, this irritation will be allayed; but being once discharged from the liver, it has no more to do with the morbid action on which its vitiated state depends, than any other equally irritating substance introduced into the same passages would have; and while it is being formed, as its vitiated state is the consequence, not the cause, of the disease which has deranged it, its correction, even at the moment of its secretion, were this possible, could not correct that disease.

Let us take another familiar instance, which is the more in point because the imperfect knowledge of our predecessors respecting the laws of the animal economy actually led them to practise on the mere principle of the chemical properties of the remedy. Putrid fever takes its name from the putrid state of the various secreted fluids; and such was their confined views of the animal economy, that they thought, if the putrescent state of the fluids could be corrected, nothing more would be required for the cure of the disease; thus mistaking a consequence of the disease, the necessary effect of the failing powers of life, for the disease itself.

The trial was fairly made, for at one time no physician would have been considered as doing his duty if he had, in such a case, omitted the use of antiseptics; and what has been the result? that any physician who should now place reliance on them, although the labours of the chemist have supplied us with such as are far more efficacious than any possessed by our predecessors,

would only expose himself to the animadversions of his better-informed associates. It would be superfluous to pursue the subject farther, as I have already had occasion to observe the tendency of such views is to bring us back to the humoral pathology—a pathology equally narrow and mistaken, which could not for a moment have existed except in the very infancy of our science.

Are we, then, it may be said, to regard chemistry as wholly inefficient in tracing the laws of the living animal body, and enabling us to cure its diseases? Such a question could only have been suggested by the exaggerated claims which have been made in its favour. Its legitimate claims in these departments of knowledge, notwithstanding all the deductions which have been stated, are great. They are sufficient to satisfy the most ardent admirer of a science which has left so few branches of knowledge indebted to it. Let us take a rapid view of these claims, and first as respects physiology.

Although chemistry cannot, and, for the reasons which have been mentioned, never will, enable us to trace all the changes of composition which take place in the living animal body, physiology is in many respects indebted to it. It can tell us many of the changes of composition which take place in the living animal, and all those which the living animal effects in the external bodies influenced by it. It can explain, for example, certain changes in the blood effected by respiration, although it cannot explain all these changes, and of all the changes effected by respiration on the air, it can give us a perfect knowledge. Thus we owe to chemistry all the knowledge we possess respecting one class of the effects of this important function.

Similar observations apply to the function of perspiration, which, like respiration, both influences the composition of, and, under different circumstances, variously impregnates, the air.

In like manner chemistry, although incapable alone of explaining any stage of the process of digestion, can determine many of the changes which the food undergoes in that process. Although we have reason to believe that the chyle, when taken from its vessels, is not in all respects the same as that which moves in them, chemistry can, by enabling us to compare its compo-

sition with that of the food, determine many of the changes effected in the latter.

To chemistry we also owe our knowledge of many of the secreted fluids. These are separated from the living powers by being discharged externally, or into the various cavities of the body, and are therefore no longer subject to their influence; and their composition throws much light on the nature of the general, if not on the detail of the particular, processes by which they are formed. It tends also to throw no small degree of light on many of the functions of the animal body, that chemistry has unfolded to us the changes which would take place in the substances of which it is composed, were they not counteracted by the vital powers of that body. Many of the changes which take place in disease are of this nature, for as these powers fail, the laws of inanimate nature begin to prevail over them, and, before the patient expires, the latter have often in a great degree established their dominion, producing symptoms which cannot be explained without a recurrence to those laws. This leads us to the second part of the subject.

If the science of the vital functions in a state of health has been indebted to chemistry, that of their diseased states has been infinitely more so; not only as a means of occasionally explaining a certain class of the phenomena which attend and modify these states, and thus occasionally suggesting means of relief; but in giving the most efficacious and convenient form to a large proportion of these means. Respecting the first part of the subject, we cannot have a better illustration than Dr. Prout has afforded in his investigations respecting urinary diseases. These diseases, I have already had occasion to observe, are particularly favourable to the labours of the chemist, because many of their most distressing symptoms depend on the state of the urine which is so long retained after its secretion, forming no longer any part of the living animal, and acting in the same way and being subject to the same changes as would be the case with respect to any similar fluid introduced from without.

In cases of other deranged secretions, and those of morbid extravasations and growths, we have derived much information from the labours of the chemist; and those acquainted with the

progress of knowledge will not undervalue such information, because, in many instances, it has not yet led to practical results.

When we turn to the improvement of the medicinal part of our means of cure, we owe every thing to chemistry. On this subject it would be superfluous to make many observations: it is the peculiar province of the chemist. In the various pharmacopœias, and the works published with a view to illustrate their processes, we see how much we are indebted to him. To the improvement of this important branch of knowledge chemists of the highest character have lent their aid. To the French chemists we are particularly indebted, and not less to some of our own countrymen, who have devoted to it a large proportion of their time and labour. We have been supplied with many new means of relief, and many of our former means have been rendered both more congenial and more efficacious.

ASTHMA, &c.

The Case of Mr. George Hobson, of Great Marylebone-Street, drawn out by himself.

MR. HOBSON, aged 60 years, a native of the North Riding of Yorkshire, where he resided till he was 21, was subject, from early youth, to violent attacks of asthma, which were almost regularly periodical, occurring once in six or eight weeks. They varied as little in their frequency as in their violence, and each fit continued six or seven days, during which he could never lie down nor breathe unless his legs were in a depending position, nor sleep except with his head on a table placed before him; after this, easy and copious expectoration came on, which, continuing for three or four days, completely relieved him from every embarrassment. He continued thus dreadfully to suffer from the age of seven to twenty-one, when he came to London, and remained upwards of eight months, during which period he never experienced even difficulty of breathing; and though he had, from frequent attacks of cold, violent coughing and its other usual attendants, his asthma did not return. He revisited the north, but be-

fore he reached home was attacked with his old enemy, which continued for four months, without any complete interval of ease, and the paroxysms were more violent than formerly. He then determined upon returning to town, where, being of the medical profession, he settled, and has since remained, without ever being deprived of a night's rest from asthma, though he has had, in severe colds, more than ordinary difficulty in respiration. In three or four years after the asthma left him, he became subject to irregular action of the heart, and palpitation, attended with great flatulence and acidity of stomach, and he fancied himself relieved by alkaline medicines, and particularly by the carbonate of ammonia, from its cordial effects in large doses. The attacks were at first of short duration, but soon became more serious and frequent, and have continued without intermission for eleven or twelve hours, attended with a violent and indescribable pain along the whole course of the spinal column and under each scapula, and difficult and painful respiration; the whole of which deprived him, at the time, of the power of walking without support. He can only compare his feelings to what would be the result of an immense weight placed upon each shoulder, dragging them and the scapulæ down. These symptoms were attended and followed, for some time, with a sighing laborious respiration, much increased upon the slightest exertion, and a sense of numbness and pain extending down both arms, particularly the left. The digestive functions were at times a good deal disturbed; the secretions of the liver and the bowels much depraved; bowels regular; motions dark and offensive; appetite very good; sometimes, but not often, vomiting after meals; his general rest at night but short, and disturbed. The period during which these attacks were absent was very irregular; sometimes as many as four or five times in one week, at other times he had not one in two months. Whenever dyspepsia attended, the usual remedies have always failed to cure it. The affection of the heart, however, came on in the intervals, and just as much when the nutritive functions were well as when they were imperfect; and he was at all times pale, debilitated, and bloated, with œdema of the feet and legs, extending to the knees, and moved

about with difficulty; and such was his state of health, that, for four years past, he has never left home without having his name and address in his hat or a card in his pocket, being doubtful if he would ever return alive. It was soon after his arrival in town, about thirty-four years ago, he became the subject of irregular action of the heart; this, at first, was but slightly noticed, but soon became of a more pressing and alarming nature; the seizure would come on without any sign or note of its approach, when either sitting, lying in bed, or in active exercise; four or five of the heart's pulsations could not be felt; the fifth would be an irregular bound against the parietes of the chest; there was no distinct or regular interval observable in the pulse — its rhythm would be nearly continuous against the finger; there was an oppressive constriction across the chest, coldness of the extremities, numbness and pain in the arms, as before noticed; the face expressive of intense anxiety; nose and lips cold, and of a blue colour; great restlessness, and inability to remain in one situation; frequent windy evacuations, without any apparent relief. The only thing which appeared to afford ease was the employment of strong friction along the course of the spine. These phenomena lasted indifferently for two, six, or twelve hours (never so violent, however, when so long as the latter period), when, with a sudden and very forcible and painful impetus against the ribs, the heart would be righted, and its action, with that of the pulse, would at once become regular. From the long continuance of these attacks, and frequent recurrence of irregular action of the heart, he was led to believe the existence of some structural disease in or about the heart; in which he was confirmed by the opinion of medical friends, many of whom he consulted, and by his father having suffered in the same way and died very suddenly.

Dr. Wilson Philip was at this time (about a year ago) consulted; who afforded him great consolation by assuring him that there were good grounds to hope that, in a great measure, his fears were unfounded, and, as many of these appalling symptoms were evidently sympathetic, there was reason to hope that the others might be of the same nature. Dr. P. ordered him five grains of the blue pill for three alternate nights, with

an aperient draught on the following morning. The alimentary canal was thus cleared, and Mr. H. by his advice, commenced taking half a grain of blue pill with three of Extr. Hyoscyamus, three times a-day, with stomachic draughts, and other occasional treatment; which he continued for many months, and which, he believes, has restored him. At first the relief was less perfect, and only occasional; by degrees the intervals of relief became more perfect, and of longer duration, and it is now four months since he has had any attack; while his looks, feelings, and general health, are in every way so much improved as to surprise those who have known him for the greater part of his life. His œdematous swelling and bloating have disappeared, and he has regained both the appearance and habits of health.

October 10, 1831.

OVARIAN DISEASE.

To the Editor of the London Medical Gazette.

SIR,

MRS. BOTTRELL, æt. 24, of short stature and light complexion, married four years. Has had no children, nor abortions; until a year and a half ago enjoyed good health. At that time was attacked with inflammation in the left iliac region. In a few days it was so far subdued that nothing but a slight pain and uneasiness remained. In a few weeks the abdomen was observed to be enlarged, and continued to enlarge for four months, when fluctuation was discovered. The operation of tapping was resorted to, and was repeated four times previously to her being under my care. What remedial means had been employed, I am unacquainted with. After the second operation the abdomen was noticed to be still large, and on examination a tumor was detected in the pelvic region. I was called upon to perform tapping on February 24th, 1831. About four gallons of limpid fluid were removed. After the operation the lower part of the abdomen remained prominent, and the parietes were distended; it had an even surface, and conveyed to the touch a doughy feel; no tenderness or pain on pressure; the body was much emaciated; tongue furred; appetite bad;

bowels torpid, and secretions dark-coloured; urine pale for a few days after tapping, but became thick and albuminous; pulse 120, small and thready; skin dry and hot; sleep disturbed; shooting pains along the course of the large nerves of the thigh.

Purgatives, mercury, and the various diuretic medicines, were exhibited. The Tr. Iodinæ was given for a few weeks, commencing with five drops every four hours, and increased to five and twenty. The abdomen was rubbed twice a-day with an ointment composed of Ung. Hydrarg. and Camphor. Digitalis was given to diminish the frequency of the pulse, which it did effectually. The general health became improved, but without any diminution of the tumor, and only slight retardation of the dropsical accumulation. The Ung. Hydriod. Potassæ was rubbed into the abdomen for the space of half an hour twice a-day, regularly. The circumference of the abdomen after tapping was found to have diminished three inches and a half in eight weeks from the commencement of the Iodine friction. Paracentesis abdominis was repeated every three weeks or a month. The fluid varied in colour each time; sometimes it was limpid and watery, at others thick and opaque, and containing in it flakes of lymph and pus. At one time about a quart of uncoagulated blood followed the withdrawal of the trocar. On the 5th of September the last operation was performed. Seven pints of fluid more than previously, were removed. The day following she was up, and would, injudiciously, be assisted into the yard; she sat there for half an hour, but on account of faintness was obliged to return. From that time complained of soreness in the abdomen, not tenderness on pressure, nor positive pain; she said it was an indescribable uneasiness. Peritonitis being suspected, the various antiphlogistic measures were promptly adopted, but without affording any relief. On the 19th she died*.

Ovarian disease is universally allowed to be one of those direful maladies which have hitherto baffled every attempt at their removal by medicine. Happily this disease is not of very frequent occurrence; and to this may be

fairly attributed our ignorance of any true diagnostic marks by which it can be recognized in its early stage. On reviewing the preceding case, many circumstances present themselves importantly interesting. We observe that this disease occurred in a *young* woman, who, though married, had never borne children; that it commenced with inflammation in the left iliac region; that in *four months* the abdomen was filled with fluid; and that the tumor was at this time so diminutive, that its existence was not even suspected. But one of the most remarkable features was the rapidity of the growth of the tumor, it not being cognizable immediately after the first tapping, yet in six weeks being as large as a child's head.

Had the tumor been discovered when the size of an orange, or even *twice* that size, would extirpation of it have been practicable?

The question is answered by that philosophic physician, Dr. Blundell, in his *Physiological Researches*; where he has proved very satisfactorily the groundlessness of the fears entertained with respect to wounding the peritoneum, and has also shewn the facility with which diseased ovary, in its *early stage*, may be removed. In this case it is obvious that extirpation could have been successful only in the very earliest period, in consequence of the early formation of the dropsical sac; yet I believe no one would have apprehended that such a structure did at the time exist. Even at the period when I first saw the patient, and also during the whole of my attendance, I did not doubt that the dropsical fluid was contained in the peritoneal bag. Dissection revealed that it was not; but that it was contained in a new structure, the 'sac,' which sac must have been formed either prior to the enlargement of the substance of the ovary, or have increased in a much greater ratio, so as to have lined nearly the whole of the abdominal cavity—to have become filled with fluid, and to have existed so for some weeks before the tumor was discovered.

We observe the inefficacy of the most potent medicines in this disease; and it affords further corroboration of the veracity of the conclusion, that on such morbid structures medicine has no influence. We therefore arrive at the inference, that extirpation of diseased ovary, in the *early period of its growth*,

* We omit the postmortem examination, as the appearances were not unusual, and as we are much pressed for space.

is the only curative measure we are yet acquainted with.

Ought paracentesis abdominis to be performed in ovarian dropsy, when the fluid does not amount to more than *thirty-two* pints? What advantage can accrue from such early tapping? Will it prevent a future accumulation?

Should any urgent symptom arise, such as violent dyspnœa, then the operation of tapping might be demanded; but it does not appear that in this case any unpleasant symptom was manifested;—she was tapped because the abdomen was filled. It is clear that no permanent benefit can be obtained by early tapping; for, while the cyst remains, fluid will be again secreted, unless the sides of the cyst could be retained in coaptation, and inflammation induced, so as to obliterate the cavity; but it would be difficult to effect this, owing to a certain quantity of fluid which must necessarily remain and form a nidus.

With respect to the tumor there can be no doubt that it was a disease of the ovary; but under what genus it can be classified, it would be difficult to say. It is neither a scirrhus, nor an hydatid, nor yet a common encysted tumor; but a combination of the three structures. The cells could not all be the enlarged vesicles of De Graaf, because these are not more than sixteen or twenty in number; whereas some hundred cysts were discovered, many of which had all the characters of hydatids; it is probable that the 'vesicles' were implicated. The envelope of the tumor appears to have been the *tunica propria* of the ovary; and the 'sac' an extension of the upper border of that tunic, which had become separated from the contained parts.

These brief remarks may enable the reader to comprehend more clearly the nature of the case.

I am, sir,

Your obedient servant,

W. DOBSON, Surgeon.

2, Belgrave-Street South, Pimlico,
Oct. 5, 1831.

ANALYSES & NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Observations on Cholera, as it appeared at Port-Glasgow, during the months of July and August 1831. Illustrated by numerous Cases. By JOHN MARSHALL, M.D.

DR. MARSHALL, of Port-Glasgow, is the gentleman who created a somewhat unnecessary alarm some months ago by reporting to government the occurrence of cholera in the town where he resides. It soon appeared, however, to use a vulgar expression, that he had "found a mare's nest," and some degree of ridicule, naturally enough, followed this discovery; in fact, we imagine from the contents of the brochure before us, that the Doctor's cases had become a standing joke in the north. Instead of letting the matter drop quietly and be forgotten, Dr. Marshall resolved to print the cases, "and shame the rogues." This proceeding, we think, was injudicious; for, in our opinion, he has thus condemned himself out of his mouth. Twenty-four cases of vomiting, purging, with cramps, &c. are related, such as every man occasionally meets with in the hot months; of these only one died, and he had been drinking some days before at an Irish wedding, and at the onset of his attack, before Dr. Marshall was called in, had had a mixture, containing six grains of tartar emetic. The Doctor does not admit that he had taken much of this, but adds, "Let us, for the sake of argument, allow that the whole was swallowed at one draught, could these six grains of emetic tartar have produced the morbid symptoms (namely, excessive depression, vomiting, purging, &c.) which I witnessed when called to him nineteen hours after?" The practitioner who can gravely ask such a question, must be wonderfully ignorant, or have his ideas strangely perverted by prejudice.

We shall quote one of Dr. Marshall's cases, selecting one which was severe, but the account of which is conveniently brief:—

"July 11, 1831.—James M'Lachlan, ætat. 25, labourer in a sugar-house, very stout made, enjoys excellent health, was seized during the course of

last night with deadly sickness, vomiting, and purging of dark matters, which speedily changed to colourless fluids, excruciating pain in the bowels, spasms, by which the abdominal muscles seemed drawn in till they rested on the spine; excessive debility; surface of body cold; profuse cold perspirations. Reaction took place in eight hours after first attack; was completely established in twelve. The patient convalesced rapidly, and returned to his work on the 15th.

"Had sat with John Murray during the night of the 3d July; knows of no other cause for his illness; has not partaken of fruit, vegetables, or 'new potatoes,' during the present season."—*Ex uno disce omnes.*

Illustrations of Surgical Anatomy, with Explanatory References; founded on the work of M. Blandin. By JOHN G. M. BURT, Surgeon to the City Dispensary, Extraordinary Member of the Royal Medical Society, &c. 4to. Edinburgh.

These illustrations of surgical anatomy are founded on the justly-esteemed work of M. Blandin, on whose plan, however, the editor has improved. In the fasciculus which lies before us there are two plates, the first representing the superficial and the second the deep-seated anatomy of the neck. They are beautifully executed, and if those which follow equal them in this respect, the work will prove highly creditable to Mr. Burt, and extremely useful to the practical surgeon as well as to the student of anatomy.

Pharmacopœia Medico-Chirurgica in usum Medicinam Facientium, à ROBERTO G. HOLLAND, Chirurgo, Concinnata.

This volume contains a certain number of lists of medicines, tables of synonyms, formulæ, &c. perhaps as good, certainly no better, than many others previously published.

Reports of Medical Cases. By DR. BRIGHT. Price 9l. 9s.

[Continued from page 57.]

Inflammation of the Substance of the Brain.

The next subject which attracts our attention, in the regular order of the volume, is inflammation of the substance of the brain.

A healthy man, nearly sixty years of age, fell, and slightly scraped the side of the left temple. He found it very painful, used common remedies, and in a day or two had forgotten the accident, and returned to his usual avocations. Three weeks after he went upon a shooting excursion, took much exercise, got wet, eat heartily, and slept into a coach to return to town. During the journey pain in the head came on, and by the time he arrived at home he was labouring under a smart attack of fever, with pain spreading from the eyebrow towards the vertex, and occasionally down the back. He was bled generally and locally, and had appropriate remedies to allay the febrile disturbance. Next day was much the same, perspired freely, and became chilly on the least exposure; his manner was hurried; in fact, he was slightly delirious. There was puffiness above the eyebrow, and a tumor in the socket below the eye, from which last some discharge of pus took place at night, with great relief. On the following day, the symptoms continuing, he was seen by Mr. Brodie, who cut down upon the bone just above the superciliary ridge; it was rough; about "three drops" of pus came away, and the patient was "in heaven." He went on for some time without any very decided change, afterwards became rather childish, being, however, at intervals, more rational. For several days he got up regularly for a few hours, though his pulse was extremely weak. He was now seized with an erysipelatous attack of the face and head, which carried him off in a week.

"*Sectio Cadaveris.*—Some serous effusion in the cellular membrane of the scalp. The bone was denuded only to a small extent, about the size of a sixpenny piece, where the second opening was made; but the surrounding bone, more particularly between this opening and that which had been made upon the superciliary ridge, was unusually brittle, splintering in various directions under the saw and the chisel. The pericranium just round the denuded bone was thickened. The dura mater was not detached, but a deposit of organized membrane, nearly as thick as the dura mater, but more flocculent, had taken place between it and the bone in that part, over an extent of the size of a crown piece; and a mark remained upon the bone where the deposit had chiefly been, of an irregular form, with white specks as if from adventitious matter deposited in the pores of the bone. When the dura mater was raised,

its internal surface shewed also a flocculent deposit, and the whole surface of the brain exhibited the arachnoid raised by serous effusion, so as to conceal the convolutions; the arachnoid was slightly opaque; but on the edge of the anterior lobe of the left hemisphere, close to the falx, was an ulcer of the size of half a hazel-nut, oval, depressed, and containing a small quantity of turbid white fluid, of puriform character; and when this was removed with a sponge, a clear depression became visible, as if a piece of the brain had been cut out. The brain itself did not appear unhealthy, but the blood was rather fluid in the vessels divided in the cerebrum, giving bloody points. In the ventricles was at least double the ordinary quantity of serum, and that rather turbid. The choroid plexus was natural, nor was any other obvious deviation from health to be discovered."

Although the above be adduced as a case of ulceration on the surface of the brain, Dr. Bright is not disposed to argue that the chief symptoms depended upon this.

A man of colour, between fifty and sixty, was admitted at Guy's with severe dyspnœa, and dropsy. He had been in the hospital before, for the above symptoms, but had been sent away "on account of the noise and disturbance he made during the night." He was now in a drowsy state, capable of being roused, but not so as to give any satisfactory account of himself. He was not considered as being in his sound mind, and at night became noisy. About four days before his death he was observed to be sinking, but his pulse always had a peculiar sharpness. There were some appearances of slight paralysis.

"*Sectio Cadaveris.*—On removing the dura mater, a thin layer of effused blood of several inches in extent—in some parts as thin as paper, in others in flat masses as thick as a shilling—was discovered on the posterior part of the left hemisphere, and on the anterior part of the right; this was not effused so as to be smeared over, or to discolour the arachnoid, but remained attached to the dura mater, and could be peeled off as if the thin clots of which it was composed were contained within a membrane; but whether this was only a fibrinous envelope of the clot, or an actual membrane, was doubtful. I inclined to the latter opinion. On examining the base of the brain, both of the anterior lobes were found to be eaten by superficial ulceration in three or four irregular patches of the size of a shilling, which were yellow and excavated to the depth of the eighth of an inch, though the yellow soft matter could scarcely be considered as true pus. The vessels of the base were not

perfectly healthy, but in different parts contained cartilaginous deposits. On the surface of the medulla oblongata was a curious black carbonaceous deposit, following somewhat in the course of vessels, which, judging from some other cases, I believe to be the remains of a former effusion of blood under the arachnoid, or upon its surface.

"The left lung was partially adherent to the pleura, containing much grey matter, natural in structure, but loaded with serous effusion. The bronchial tubes of the deepest red colour, and much dilated towards their extremities. The right lung much more generally adherent, and extensively hardened, from old and chronic inflammation; so that scarcely any part performed its functions: the upper lobe, in particular, was a hard, tough, grey mass, and the bronchi were still more diseased than in the left lung."

In this case there were sufficient signs of some derangement of the brain, but this was conceived to depend chiefly on the difficulty of transmitting the blood through the lungs.

Cases follow in which encysted abscesses were found in various parts of the brain; in some of them, from external injury, in others from spontaneous causes: but as these cases do not appear to us to present so much interest, either in a pathological or practical point of view, as most of the others, we shall pass on to

Cases illustrative of softening of the Brain;

that peculiar change of structure which has been of late years described by some of the most distinguished among the French pathologists, as well as by some among ourselves. Dr. Bright is of opinion that "softening" of the brain may exist in various forms, may originate in various causes, and may be marked by different symptoms. The symptoms, however, which generally first attract attention, are those of pressure: for the most part they come on suddenly, and they vary in their progress and duration. The pressure, however, seems to be but a secondary part of the diseased condition—a consequence of some change which has gone before it; while it throws but little light on the nature of the pathological condition.

The first case detailed (LXXXI.) is one in which there was sudden hemiplegia, all power of voluntary motion and all sensation being lost both in the upper and lower extremity. The face was only slightly affected, but the speech was exceedingly imperfect. Pulse 68,

weak. This man had married a woman much younger than himself a few years before. He had also been subject to an open ulcer on the leg for several years, but which had healed up about six or eight months before. He then became subject to attacks of diarrhœa. He had never complained of his head. He was admitted August 5, and died on the 21st of the same month. Post-mortem examination discovered softening of the middle and posterior lobes of the left hemisphere of the cerebrum, with limpid effusion beneath the arachnoid and between the convolutions.

A man, aged 30, was admitted at Guy's Hospital, April 7th, with imperfect hemiplegia and coma. It appeared that he had complained of the left side of his head for three or four months, and just four weeks before admission he had been seized with very severe headache, which had continued ever since. The loss of power came on gradually. On the night between the 5th and 6th he became worse, and passed his fœces involuntarily, losing his speech at the same time. He lingered till the 21st. Softening was found of the anterior lobe of the left hemisphere. The arteries in the vicinity of the fissura Sylvii were indurated and cartilaginous, and the circle of Willis was studded with bony deposits.

A young man, aged 28, was admitted October 4, 1827, with partial paralysis, marked by slight deformity of the face, and feeble, unsteady action of the arms and legs. Had from infancy been afflicted with headaches, generally referred to the forehead, with occasional giddiness. The symptoms increased about three years ago, and about eighteen months since had a fit, in which he lost his senses for a few minutes. There was a very gradual, but decided change, from this time, indicative of chronic mischief in the brain. He died on the 12th of November. Great vascularity of the pia mater was found, with softening of the left hemisphere in two places.

A gentleman, aged 73, was suddenly reduced from comparative affluence to penury by the knavery of a partner, who absconded. On receiving the information, he fell down in an apoplectic seizure. After a time he recovered from this attack, and was able to write for one of the weekly journals, although his memory was defective. In ten

months he had a second fit nearly like the first; after this he never was able to engage in any occupation, and about three weeks before the date of the report, he had a third fit, for which he was copiously bled, and had some aperient medicine. After this he was at times tolerably sensible, but in general lay in a comatose state, and appeared to have lost all power in the right hand as well as in both legs. He continued much the same from the 24th of June to the 3d of February following, when he became more conscious, and had more power over his limbs. This improvement lasted three or four days, when he sunk into a comatose state, and died on the 9th. On examination after death, considerable cerebral congestion was found, with partial softening, especially of the fornix and septum lucidum. There was a good deal of fluid about the brain. Dr. Bright observes of this case, that it was probably one of those in which no considerable vessel is ruptured, the apoplectic fit rather depending upon a general state of congestion, producing slight laceration of the fine vessels, chiefly at the union of the cineritious with the medullary matter.

A tall, athletic man, 40 years of age, was admitted at Guy's, under the care of the author, January 21, 1829. He had been long in the army—had received many wounds, and had "scars to shew." He laboured under great depression of body and mind; his left arm and leg were weakened; his gait unsteady; and he squinted. "His manner was peculiar, and his conversation quite incoherent; he stood for some minutes together in one position, as if lost in abstraction; when spoken to, he answered a question with some consideration apparently correctly, but then at once ran on to something else, without however saying much, but making some statement perfectly without foundation; as, for instance, saying that he was bled this morning, that the blood flowed well, and did him a great deal of good;—all which was most distant from the fact." His illness had been coming on about a year, and the first indication of it was his falling down in the street, and losing his recollection for a moment. This happened repeatedly, but was equally transient as at first. He had, however, constant pain in the forehead, and for the last six weeks had passed his urine,

and sometimes even his faeces, in bed. He now had leeches applied, a seton was introduced into the neck, and he was purged. Afterwards he took a grain of sulphate of zinc three times a day, and the quantity gradually increased to gr. v. ter die. Carbonate of iron, sulphate of quina, and various other means, were tried without avail. July 10th he is thus described:—"His general manner and aspect quite unchanged; he stands about musing; and if you ask him how he finds himself, answers, after a few seconds of consideration, 'I am not quite dead;' says he has hurt his thigh, and that there are wounds in two places; but on examination there are only two old scars from wounds he had received in service; he complains of weakness of the whole left side, but then says he feels rather better."

He died on the 23d. The skull shewed a slight irregularity, with depression of half the size of a sixpence, with a minute point of bone running towards the brain: this was at the site of a wound. The chief morbid appearance was in the corpus striatum of the right side, which was quite flattened, and of a yellowish colour: it felt quite soft, and on making a section was found to be completely broken down, having a "filamentous and watery brown appearance."

The author, in commenting on the cases of which we have above given a brief outline, observes,—

"Thus we perceive that partial softening of the brain probably owes its origin to a variety of causes, and to a considerable range of morbid action; and we may therefore conclude, that however much the symptoms of the confirmed disorganization may approach in all cases, and may generally evince proof of pressure, and of interrupted nervous susceptibility and action, yet that the symptoms which mark its progress and approach will vary greatly; and this conclusion will be borne out by a comparison of the symptoms in the cases to which I have had occasion to refer. Although it is possible that inflammation may exist without being attended by very striking symptoms, yet I am inclined to make a threefold division of cases in which a softened condition of portions of the brain exists. First, where from obstructed circulation the part undergoes a change analogous to gangrene; and this is the more ge-

nuine form of the disease (Case LXXXI. LXXXII. LXXXIII.) Secondly, where from congestion the substance of the brain suffers a more or less complete laceration (Case XXIV. LXXXIV.); and under this division might be classed all those cases in which the disorganization is secondary, and to a certain degree mechanically dependent on the presence of tumors, or effused serous fluid, or apoplectic clots (Case LXV.) And thirdly, where softening is produced by inflammatory action (Case LXXVIII. LXXIX. LXXX.)

"It must however be remarked, that in these observations I speak only of partial softening with disorganization of the brain; not including those cases in which the whole substance of the brain is found unusually soft, a condition which often occurs when the body has been greatly debilitated and worn out by disease, but seems not to interfere with the structural integrity of the organ.

"Dr. Baillie, in his incomparable manual of morbid anatomy, by which the attention of modern physicians was chiefly drawn to the study of pathology, speaks of the softening of the brain as it is described more particularly by Dr. John Hunter, in the *Gulstonian Lectures* read before the College of Physicians in 1796; and he refers to its occurrence under two forms—in advanced age, and combined with apoplectic effusion; though, by a remark he afterwards makes when speaking of the effusion of blood into the brain, it is doubtful whether he considered it in the latter case a disease existing previously to the rupture of the vessels, or a consequence of the mischief. The fact appears to be, that it occurs in apoplexy under both of these circumstances, and cases illustrative of both will be found recorded even in the present volume.

"With regard to the treatment of cases in which we are induced to suspect that the softened disorganization is taking place, it will of course vary according to the cause; as long as we are obliged to infer excessive action we must deplete, and use counter irritation; but looking to the general condition of those who are the subjects of such disorganization, we should be inclined to prohibit active depleting remedies, as likely to diminish the powers of the system. Nor can we suppose that the mercurial action would produce any

good effect; on the contrary, whatever is calculated to disturb the more healthy and natural actions, may be expected to do harm, and probably the careful avoidance of every thing which can over-excite either the body or the mind, with the employment of gentle tonic remedies, both as medicine and as diet, and even as occupation and amusement, will be most effectual in delaying the mischief, or in supporting the frame under its gradual decline. To what period life may be prolonged, when a considerable extent of the substance of the brain is diseased, we have not the means of asserting; but that large portions of the brain may be lost with apparent impunity has been proved in many instances; and in some of the foregoing cases there is reason to believe that the mischief had existed several months, attended, however, with great functional derangement. Whether healthy brain be ever regenerated is very doubtful; but as to the power of nature of repairing in some degree the injuries of the brain, there can be no doubt; and there is reason to believe, that parts of the substance which have been lacerated by blows, or by apoplexy, and thus rendered useless, are frequently absorbed; but how far the powers of the system are sufficient for such an operation, where, as in the cases of Kennedy or Murrledge, spontaneous change has taken place, remains a matter of speculative opinion; at all events, one of the best objects we can propose in the treatment, is to give force and vigor to those natural actions by which such reparations are effected."

PRESSURE.

The division of this part of the subject adopted by our author, is, first, into those cases which illustrate the effects of mere vascular turgescence; 2dly, those in which the symptoms appear to have been in some degree, at least, the result of effused serum; 3dly, cases in which there has been rupture of vessel, with extravasated blood; and 4thly, those in which the pressure has resulted from some organic change.

Cases illustrative of the effects of Cerebral Pressure from Vascular Turgescence.

When this state comes on suddenly, it is productive of vertigo, loss of consciousness and voluntary power, and

not unfrequently of convulsions; apoplexy from congestion, and certain forms of epilepsy passing imperceptibly into each other. When the congestion takes place more gradually as from the effect of narcotics, or habitual indulgence in fermented liquors, or arising in the progress of disease, it is indicated by increasing listlessness, drowsiness, and finally, by coma; temporary numbness, or loss of sensation, or convulsions, being experienced towards the termination of the disease.

The following case is a good illustration of the mingled attack of apoplexy and epilepsy:—

"James Eames, aged 60, was admitted into Guy's Hospital under my care, September 10th, 1829, in a state of great feebleness, with occasional vertigo, from an attack of a mixed apoplectic and epileptic character, which he had experienced a few days previously, and for which he had been cupped; he was a stout-made man, with a bald head and short neck, exsanguine in his appearance, I believe from the frequent necessity of losing blood. I had seen him before, and was no stranger to his history. Six years previously, while he was walking, he was first seized with giddiness, and on the following day experienced a more severe attack, with loss of sight and of consciousness. He was twice bled from the arm, was freely cupped, had cold applications to the head, and gradually recovered, not having suffered during the whole attack any paralysis. Three years and a half afterwards he had a similar seizure, and again about a year after that; subsequently to which he was in the hospital, under the care of Dr. Cholmeley; and while still in the house about a year ago, he had a more severe attack, greatly resembling apoplexy, in which he remained perfectly senseless for several hours, but this was attended with spasmodic action of all the extremities. He was cupped, and recovered pretty speedily, but remained feeble for several weeks; and about six months afterwards had another similar attack. As he was free from the immediate severity of the fit at the time of his being received under my care, I had recourse to gentle purging, and afterwards to tonics, and on one occasion was obliged to cup him at the neck; under this plan of treatment he gained strength rapidly, and left the house in about a month."

In cases of this nature, which are by no means uncommon, the chief point to be decided is, the extent to which depletion ought to be carried, and Dr. Bright observes, that in most instances, in proportion as the epileptic character

preponderates, the propriety of large depletions will become more questionable. Indeed, the abstraction of blood can only be regarded as a temporary means, calculated to remove present danger, which having been effected, keeping up regular action of the bowels, with tonic regimen, is best calculated to guard against relapse.

Next in succession we find a set of instructive cases, shewing the tendency of narcotic poisons to produce congestion in the brain. In the following case, which proved fatal, the state of the cerebral vessels was matter of demonstration:—

Poison by Opium—Great Vascular Congestion of the Brain.

“—Williams, a young woman, was brought to Guy's Hospital January 21st, 1825, in a state of profound coma, from the effects of opium, taken about two hours before her admission, with a view to self-destruction. The quantity she had swallowed was not exactly known. The stomach-pump was freely used, both to wash out the stomach and to administer stimulants; but sensibility never returned, nor did she ever so far recover as to be able to swallow, and she died about fifteen hours after admission.

“ *Sectio Cadaveris.*—Vessels of the head unusually turgid throughout, and on the surface of the anterior part of the left hemisphere of the cerebrum, an ecchymosis was observed of a very light colour, about the size of a crown-piece—such as a very few drops of effused blood might have produced. The bloody points were very numerous in the cut surface of the brain: there was no serum collected in the ventricles. The stomach was quite healthy, except two or three spots of ecchymosis, which were ascribed to the tube of the stomach-pump; there was on the whole less vascularity than usual. In the large intestines were some appearances of inflammatory action. A small ovarian tumor, about the size of an orange, was found in the pelvis.”

But all cases come not to this unhappy end, even where large quantities of laudanum have been swallowed, and Dr. Bright relates three instances in which “an ounce and a half,” “something more than two drachms,” and what was believed “to exceed an ounce,” were taken; their baneful effects being warded off chiefly by the affusion of cold water upon the head, in the manner recommended by Mr. Wray some years ago, in a paper which he published in the Medical Repository.

MEDICAL GAZETTE.

Saturday, October 22, 1831.

“*Licet omnibus, licet etiam mihi, dignitatem Artis Medicæ tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso.*”—CICERO.

USE AND ABUSE OF CERTAIN COLLEGE REGULATIONS.

WE have been a good deal amused during the last week or two with witnessing the cool impudence and empty babble of a would-be director of study—an open instigator of cabals among the medical students of the metropolis. Amused we were, because we knew how impotent and harmless were all these unbidden efforts; nay, we should be quite satisfied with the amusement, did we not fear that some unlucky simpleton or other might be misled by the contrivances of the medico-political mountebank. As he “bides his time” with the instinct of an animal of mischief, he calculates about this period on the number of the inexperienced whom he may hope to delude; nor does he scruple to employ the most abject arts of insinuation to gain the ear of those who are about to enter upon their earliest course of medical study. To reason with him, or to reply to the sort of trash which he indites, were out of the question; it would first be necessary to know exactly what he aims at—what it is, in the name of every thing incomprehensible, that the man means to insist on? Is it the abolishing of all rule and government in the profession? or rather, is it not his notion that he may eventually be able to set up some fabric of his own upon the ruins of all that is respectable in our time-honoured establishments? The blockhead! the merest tyro could expose his folly and presumption.

Attending hospitals and lectures, and afterwards receiving certificates—

are not these the chief objects of the man's hostility? Has he not, in his very last effusion, held up the clinical instruction that could be afforded by a private country practitioner as equivalent to that which the student could obtain from our greatest hospitals? Let us have the language itself in which his notable scheme is described. His country-master is surely a most accomplished character:—

“He furnishes his apprentices with *repeated demonstrations in anatomy and botany*, and reads to them during an hour or two daily, upon subjects connected with *the theory and practice of medicine*. With a view of making them acquainted with *pharmaceutical chemistry*, he experimentalizes with his pupils in his *snug little laboratory*; and with a wish to teach them diagnosis and prognosis, he causes *one of his apprentices daily* to accompany him *to the bed-side of the sick*; while, in order to render familiar to them those changes which the various organs of the body undergo from the progress of disease, he neglects no favourable opportunity of instituting *post-mortem examinations* (!) Thus tutored, how can these apprentices fail to acquire a knowledge of their profession?”

The question is really too silly to merit a reply. The personage (of whom more anon) who doles out these copious stores of medical tutorship, is, a little higher up, described as one who had just “walked the hospitals” in this metropolis, who had reluctantly acquired the “requisite quantity of recognized certificates,” had happened to succeed in passing the “ordeal” of an examination for his diploma, and who had but lately “plunged into the active duties of his profession;” when, being “happy to avail himself of the assistance of one or two apprentices,” he succeeds in swindling the parents of the hopeful youths out of 200 or 250 guineas for

each, under the *false pretence* of completing their education for those sums. And this is the able gentleman whose tutorial care should supersede the acquisition of recognized certificates, and the attendance of London hospitals; this the *beau ideal* of the medical millenium which is to take place upon the abolition of the old system! Surely chaos must come again.

Hospitals! who is so weak as to endure for a moment the folly of him who would undervalue them? Hospitals! without which even the best appointed schools (if they can be said to be appointed at all without them) cannot be looked upon as half made up—can scarcely deserve the name of schools so long as the defect exists. We should be paying but a sorry compliment to the understanding of our readers were we to dwell upon the thrice-told tale of the great advantages of hospital attendance in the acquisition of a competent medical education; and we decline the task the more willingly as it has been so recently and so ably done in our pages by the accomplished professor of medicine in the University of London. But we cannot suppress our indignation at the bold effrontery of the man who is not ashamed to stand forth as a public adviser, and to advocate the possibility of their being dispensed with.

And is there a shadow more of feasibility about his notions regarding certificates? Questions may arise, as they have often arisen of late, touching certain differences as to the persons from whom certificates should be received; but we believe there never was a time when those documents were not demanded as a preliminary for an inaugural examination—as a rational, and in some sort a sufficient guarantee, that the individual presenting himself was not unworthy of being admitted to the test—a test which, without it would either have to be conducted with a painful minuteness, or be liable to frequent

frustration, from the circumstances of disreputable persons offering themselves as candidates, and from the growing prevalence of the cramming system.

Time was, undoubtedly, when restrictions had to be imposed upon those who, without sufficient reason, assumed to themselves the right of bestowing certificates; when, in consequence of the great increase of persons educated to the profession, many additional schools were set up—places in which, for temporary purposes, inefficient plans of instruction were adopted—and where persons became teachers who were totally unprovided with the suitable apparatus, preparations, drawings, clinical wards, or subjects for dissection. This grew, at length, into an evil which could no longer be endured.

Nice distinctions had to be adopted with regard to the bodies or individuals to whom this important privilege of giving certificates was to be entrusted; and, from time to time, alterations had to be made, all tending to check the intrusion of unworthy characters, and to maintain the respectability of the profession. But we conscientiously believe, that the privilege is at this time fully as freely and judiciously communicated as it can be, with due protection to the interests of the medical republic; nor shall we take the word of any disturber who would prompt us to demand its more unrestricted bestowal—much less the abolition of the whole system.

The absurdity of the proposal is not a little ingenious. Abolish hospital attendance, and dispense with certificates! Upon what ground? Who proposes it? Why, one who has no connexion whatever with hospitals, because hospitals will have no connexion with him—and who has had no interest in the “certificate system,” because *heretofore* he has never had a certificate to bestow; but one who has now a nostrum of his own to recommend, which

is to cure all the diseases incident to medical education. Oh! the disinterestedness of this noble-minded projector—who has no other object than the establishing of a poor “cave of Adullam,” a REFUGE FOR THE DESTITUTE, whether all who have nothing to lose, but every thing to gain, and are discontented with their present condition—particularly broken-down teachers of anatomy—may betake themselves, with the hope of comfort from the “eleemosynary fund!” Why should hospital tickets be purchased, or certificates of any sort obtained, when the price of the very cheapest of them were enough to buy a place in the “refuge?” And a place in the refuge makes a man a Doctor at once, and entitles him to the privilege of the fund. These must be allowed to be “advantages seldom offered to the public, and never equalled;” they are, moreover, seldom likely to be offered again—so now is the time to secure a bargain.

But what constitutes the most striking feature of the new establishment is, that it offers, like the cave of the Israelite, a most desirable haven to “every one that is in distress, and every one that is in debt;” and, we suppose, to every one that has got himself into the meshes of the law for the “good cause’s” sake. And this, by the way, reminds us of a letter which we have just received, bearing on the subject in hand: we shall make no apology, but give it to the reader at full length.

To the Editor of the London Medical Gazette.

Taunton, Oct. 18, 1831.

Sir,—I must appeal to your generosity for setting me right before the public about a little matter in which I am concerned here, and an account of which I sent the *Lancet* last week; but what was my astonishment at finding my private and confidential communication laid before the readers of that journal in a garbled and strangely altered form!

I applied for advice in a bit of a law difficulty to the editor of the *Lancet*—knowing how clever he is in scrapes of this kind—but I did not want to have myself exposed, nor my name given to the public: judge, then, of my surprise and annoyance, at finding myself betrayed—I say *betrayed*—by the awkward management of that gentleman. Sir, my name is Cureall, or more properly Cur'l, as it is still pronounced here, and I live in this town, having a decent practice of from two to three hundred a year. There is no reason now why I should conceal any thing from you: I am exposed and disgraced—myself and my business, by the way in which I have been introduced into the pages of your contemporary: I am regularly blown up by having any thing to do with that magazine of scandal and absurdity; and I have no hope of recovery but by throwing myself on the public generosity through your kind interference.*

You must know then, sir, that I am lineally descended from the celebrated Edmund Cur'l, of whom such honourable mention is made in Pope's works: this should be some guarantee for my claim to a character for learning. The name by some corruption, I suppose from the connexion of some of my later ancestors with the healing art, has descended to me as Cureall; and by that denomination many of your readers will, perhaps, recollect my father, who was sentenced to confinement in Ilchester jail for twelve months, for having been unsuccessful in an experiment with prussic acid, which he tried in the cause of science. From him I received the first rudiments of my general and medical education; and, in process of time, came to London, where, to do your contemporary justice, I found the hospitals the most useless places I ever stepped into. I learned nothing; I walked them—that was all. None of the surgeons seemed to care any more for me than if they never saw me, or had never received my money. It was the same way at lectures. I attended all the prescribed courses at one of the great hospital schools, and, except when paying for my tickets, never had the honour of enjoying the conversation of any of my preceptors, who in fact spoke only to those who were anxious for further information, and I did not need that. I would have been quite solitary, and perhaps have died of the va-

pours, had I not picked up with some despised pupils like myself, and with them contrived to spend the time pretty tolerably until the courses expired. Well, for all that, I was not deficient at the examination. I was always ready at gathering knowledge, and contrived to have enough to secure my diploma. What with the talk which I could always muster, on no matter what subject, and what with a little *cramming*, I passed. But what does it signify how I passed, when I am conscious that I know quite enough for any country practitioner? I was not going to be an hospital surgeon, or a metropolitan teacher; and so, in short, with that impression, I went home and began to practise.

It is not true that I bought a business; no, I got into notice by the credit I had of being all-accomplished, coming straight from the feet of the Gamaliels in London; not that I have got into any such mighty practice either, but my London education, and my easy account of it, stood my friend and got me a couple of apprentices, with whom I secured very respectable fees. Of course I said nothing, when taking the lads, about regulations of hospital attendance, and certificates from recognized schools. I always thought, with your contemporary, that they were odious and disgusting things; and, having equity on my side, I set my conscience at rest. The parents of my apprentices, I knew, would hear of these odious regulations some time or other, and would be readily convinced of their grossness; and, besides, they were very well able to bear the loss, or to go to law with the College, if they thought fit. I thought, too, that if they did, it would be no harm; and I should have the credit of being instrumental in effecting this most desirable consummation. I merely promised to make surgeons of them; and surgeons they are, I have no hesitation in saying, as good as myself. I taught them, it is true, more theory than practice—that was unavoidable; but I taught them the most useful practice: what is the use of capital operations I would be glad to know? I never had a capital operation to perform in my life, and perhaps never shall, but I have had a fair share of the useful minor surgery—much teeth extraction, phlebotomy, treatment of cut shins, sprains, and burns; and what more does a country practitioner

require? "My snug little laboratory"—oh come, that is too good a joke! I may have mixed sulphuric acid with infusion of roses, or lemon juice with solution of soda, but as to direct experiments for learning or teaching chemistry, that is all—nonsense. Clinical instruction I certainly gave when I could, and I remember we once had a fine fever case for a post-mortem; but I never could venture upon a *subject*; I should have been taken up for a resurrection-man, I have no doubt, if I attempted it, and I always had a wholesome fear of the law of misdemeanors before my eyes.

Well, now came the time for it. My pupils' apprenticeships expired, and they were to be examined in London without more ado. I gave them their indentures, and a general certificate for what I had taught them, and desired them to demand an early examination at the College. With what anxiety did I await the first post after their arrival: it all came out at once: a knock at my door; the fathers of my pupils enter to insist upon an explanation of their sons' being refused examination. I explained the gross injustice of such an occurrence, and proved how they *could* not be refused. "But, zounds! sir, they *have* been refused," cried one of my furious visitors. It was of no use: they swore they would not be trifled with; and, sure enough, it was not long till I had notice of an action for *swindling* served upon me, and that action now pends.

Now, sir, I appeal to you, is not this a scandalous result of the London Hospital and Lecture-certificate system? Am I not a martyr to the "horrible and disgusting," "cold, unfeeling, and relentless" system pursued by the College? But, as I said before, I throw myself on your generosity, and leave my case entirely in your hands; though, to tell you the truth, I should not have troubled you, had not Wakley made so base a use of my confidence.

Your most obedient, humble servant,
PETER CUREALL, M.R.C.S.

However much we may be obliged to Mr. Cureall for the honour of his transferred confidence, we fear he will not find us of much use to him: we would recommend him though, by all means, to become reconciled to his old

patron and adviser as soon as possible, and perhaps he may thus be enabled to exchange his "snug little laboratory" for a snug little professorship in the Refuge for the Destitute. In the meantime, the best thing we can do for him, we have done: *littera scripta manet*: what he has written, he has written; and his epistle carries its own commentary along with it.

FACTS REGARDING CHOLERA.

GREAT pains have been taken by Drs. Russel and Barry in tracing the history of cholera as it manifested itself at St. Petersburg and the adjacent places. Their inquiries have been chiefly directed to ascertaining its mode of propagation, with a view to determining the question of contagion; and although some of the facts are difficult to reconcile with any of our prevalent theories upon the subject, yet we must say that the great majority of them point to infection conveyed by persons, and not by goods or clothes, as the usual source of the contamination. The gentlemen alluded to seem to have exerted themselves to lay aside all preconceived opinions—marking the events passing under their own immediate observation, and in tracing the history of what they did not themselves witness—admitting only the testimony of those who had seen what they narrated; a caution which they were led to adopt in consequence of the colouring which they found to be often given to circumstances according to the views entertained by the narrator as to the nature of the disease itself. Of the many important facts which their last letters (received a few days ago) contain, we can only give a few, but we hope soon to see the whole laid before the public in a systematic shape. There is at St. Petersburg a city prison, under the medical charge of Dr. Bish, an intelligent physician, and

previously to the appearance of the epidemic, an anti-contagionist. He, as well as all the other officers of the establishment, resides within the walls of the prison, by which all communication with the neighbourhood is prevented. On cholera appearing at St. Petersburg, all intercourse between the gaol and the town was rigidly cut off, and no instance of the malady occurred for some time; but at length the wife of one of the prisoners, she being also a prisoner, was readmitted from one of the hospitals to which she had been sent to be cured of a syphilitic complaint, no one labouring under such being retained for treatment in the prison. On her return she passed through the apartment where her husband was: she spoke to, and saluted him, but proceeded in a moment to the part of the building appropriated to females. She had some diarrhœa when admitted, which on the following night proved to be cholera, and of which she died in twelve hours. Three women, who had been in the room with her, were next taken ill, and after them her husband, who also died. There were four hundred persons within the walls, and of these twenty-seven had cholera, which in fifteen proved fatal. No case occurred in the portion of the prison allotted to nobles, it being apart from the rest.

There is a German colony on the Neva, thirteen versts from St. Petersburg. The houses are detached with gardens, and the surrounding country highly cultivated, while the inhabitants are more cleanly, and fonder of the open air, than the Russians. Thither some persons fled on cholera appearing in the capital; one of these, a female, took the disease, and died of it; but it did not spread, no other instance of it having occurred, though her bed seems afterwards to have been used. Indeed, there are many instances in which the beds and clothes of those who have died, seem to have been made use of with im-

punity. Opposite this colony, on the other bank of the river, which is there about the breadth of the Thames at Blackfriars, are Russian villages; but they did not escape, though the locality was as salubrious as that of their German neighbours. It is a curious fact, that another German colony, on the road to Moscow, between Yshora and Colpina, escaped, although the disease raged at both the places just mentioned; but it was observed that no travellers ever stopped at the German village, because the others afforded much better accommodation.

At Cronstadt, as at Moscow, no case occurred among the military cadets (150 in number), all communication having been cut off between them and the rest of the fortress; but, on the other hand, several instances happened where persons had the disease, though no communication with any source of infection could be traced. Thus a man, confined by hæmoptisis on the third floor of a house, who had not been out of the room for many days, and who saw no one who had been exposed to the disease, took it notwithstanding. At the Foundling Hospital a good many children died of cholera, and several nurses had it; and it is a curious fact, that when any of these last, who were suckling, had the disease, so as to render it necessary for the infant to be given to another nurse, none of those who gave the breast in this way became affected with cholera, although, in many instances, the infant's clothes were not changed.

QUARANTINE.

WE understand that most vigorous arrangements have been made for the purpose of excluding cholera from our shores, if this is to be effected by quarantine. We hear that a considerable additional number of ships of war have, within the last week, been ordered upon this service.

DRS. RUSSEL AND BARRY.

THE Russian Government has conferred the decoration of St. Anne, of the second order, on Drs. Russel and Barry, for their zealous and scientific investigations on the subject of cholera at St. Petersburg. These gentlemen are supposed to be now at Hamburg, where the presence of the cholera will probably detain them for some time.

THE CHOLERA AT VIENNA.

THE mortality from the cholera in this capital, during the first month of its visitation—that is say, from the 13th of August to the 13th of September, amounted to no more than one hundred deaths. But about the latter period, a heavy rain and storm having prevailed for the better part of three days, the disease broke out with the most alarming violence on the night of the 13th, and, in the course of four-and-twenty hours, swept away eighty victims; and this principally in the *city*. In one little street, not containing above ten or a dozen houses, six persons died on that night. The higher classes seem to have been peculiarly singled out for the ravage; and the Doctors Sidprowitsch, Gassner, and Roehrig, were among the first who fell. Since this alarming burst, however, the cholera has not gone on with proportional fury: it is comparatively tranquil, though the deaths are still by no means inconsiderable.

If the local peculiarities of Vienna be taken into account, it is not difficult to explain why the higher orders have particularly felt the severity of the disease, and also why the *city* has been its principal hamlet. Vienna, the city, is small, compact, and surrounded by suburbs, from which it is separated by a wide glacis. The streets are very narrow, and the houses immoderately

high. Yet it is here that the higher classes reside, though their first-floors are generally destitute of both air and light. The lower classes, meantime, enjoy the upper stories, as well as the more spacious and airy suburbs.

It should not escape observation, too, that Vienna is almost completely hemmed in with a chain of mountains; which, however, do not protect it from violent gales during the best part of the year, and especially about the equinoxes.

Rheumatism, in a severe form, is a constant resident at Vienna; and, every autumn, a malignant dysentery prevails, of which the mortality is considerable. The people, too, it may be remarked, particularly the higher orders, are fond of *good living*.

On the whole, it would seem to augur well for the future progress of the complaint, that in Vienna it has not been more severe; for few cities in Europe, perhaps, possess more of those elements which constitute the *fomites* of this direful malady.

EXTRACTS FROM JOURNALS,

Foreign and Domestic.

REPLACEMENT OF NOSES.

DR. BARTHELEMY, in a paper in the *Journal Hebdomadaire*, on the re-union of parts entirely separated from the body, gives the following nose cases, which he says he has had upon the best authority. The first of them he obtained from a lady, who gave him *her honour* that it was true. A soldier, at Lyons, in 1815, had the end of his nose cut off in a duel by his adversary's sabre. He put the piece of nose in his pocket to keep it warm, and returned home to his lodgings. A surgeon was sent for, who put the disunited parts together again; and they *took* to admiration.

For the following case the veracity of Dr. Regnault, of the Military Hospital of Grand Caillon, is pledged. In the prison of Niort two men had a fight, in which one of them bit off a considerable portion of the other's nose. The unfortunate fellow who suffered the mutilation did nothing for four or five hours but deplore his loss: the piece of nose, however, he picked up, wrapped it in his handkerchief, and put it in his pocket. A fellow-prisoner suggested to him the propriety of applying to the surgeon of the jail, to see what he could do for him; he did so, and the medical officers having speedily warmed some alcohol, steeped the part in it, and then put the divided surfaces together. At the end of ten days they were solidly united.

ACTION OF VARIOUS MEDICINES ON THE HEALTHY BODY.

A set. of experiments has recently been performed at Leipsig on the action of several important medicines on the healthy body. They were made on twenty-two physicians and medical students, between the ages of twenty-one and forty-five; two boys, and three females, one twelve, another eighteen, and the third forty-five years of age.

Nitrate of Potass.—Eight members of the association above mentioned took twice a-day doses of nitre, gradually increased from fifteen grains to a drachm, and the effect was excitement of the kidneys, intestinal canal, and skin. The sensation of coolness felt in the mouth, throat, and sometimes the stomach, on taking a solution of nitre, is soon followed by a state of re-action, accompanied by increase of the pulse. It is, therefore, not an antiphlogistic, as some physicians imagine. It ought to be prohibited in all inflammatory affections of the alimentary canal or genito-urinary system; neither is it a proper remedy in cutaneous diseases, except to produce determination to the kidneys or intestines. As a purgative it ought to be altogether abandoned, since we possess many safer and more certain medicines of this class. As a diuretic it may be admissible in some acute dropsies, but in general it is an ineligible diuretic, for other remedies of the same class are more powerful and safe, and do not disorder digestion, as nitre does, when long

continued. The best effects are to be procured in inflammation of the brain or its membranes, or of the throat, or even of the chest. According to the experiments made with it on the females of the club, it is an emmenagogue.

Cherry-laurel water.—The article made use of was prepared by distilling three pounds of liquid from a mixture of six pounds of water, one ounce of rectified alcohol, and one pound of fresh laurel leaves chopped. Twelve members of the club took it in doses progressively increased from five to twenty-five, and finally to one hundred and twelve drops. Its whole action appeared to be concentrated on the brain, the symptoms being a sense of weight in the head, sleepiness, torpor of the intellectual functions, general lassitude and feebleness, retardation of the pulse, corresponding in degree to the headache; and this state was preceded by dull and pungent pain in the head, chiefly in the fore part, in the region of the optic nerves. The pathological cause of all these symptoms appeared to Professor Jörg to be turgescence or plethora of the blood-vessels of the head. It likewise produced unpleasant tickling in the larynx, cough, and an increased secretion of mucus, continuing sometimes for several days, and constituting a slight attack of bronchitis. Hence he infers, that cherry-laurel water is contra-indicated in inflammation of the brain and lungs, and in the first stage of scarlatina and puerperal fever. By its secondary action in lessening nervous sensibility and retarding the circulation, it may be useful in inflammation of the abdominal viscera, or organs of generation, or in excitement of these organs without inflammation. It is contra-indicated, he further conceives, in spasmodic affections where the cause is excitement of the brain, or turgescence of its vessels. M. Jörg has found that the cherry-laurel water, like the other forms of prussic acid, is rapidly decomposed, and likewise that its effects on different people are very different in degree. Bitter-almond water, though stronger in taste and odour than cherry-laurel water, is less active and more uncertain in its physiological effects. The effects of pure prussic acid are similar to those of the cherry-laurel water.

Valerian.—This was taken in infusion in the dose of from two to eight

drachms in four ounces of water, or in powder in the dose of half a drachm or a whole drachm, or in the form of tincture in various doses. Sometimes no effect was manifested; sometimes it acted on the brain; sometimes on the alimentary canal. When the brain was acted on, excitement was first remarked, being indicated by serenity or disposition to liveliness; then slight congestion followed, as was shewn by the sense of weight in the head; but there was never prostration nor torpor, as after other medicines which have the same kind of action. When the alimentary canal was acted on, eructations were produced, with a sense of fullness in the stomach, flatulence, colic, and tenesmus; sometimes nausea, with a bitter taste. Valerian likewise quickens the circulation, though moderately. It was farther remarked that the action of half a drachm continued four hours, that of two drachms twelve hours, that the action on the stomach is most easily induced by the powder, that on the brain by the infusion, and that the disagreeable taste of the infusion passed away quickly, while that of the powder remained several hours. M. Jörg concludes from his experiments, that valerian may be useful in supporting the strength whenever inflammation is not present; but that it is contra-indicated whenever there is congestion in the head or abdominal viscera.

Serpentary.—Ten members of the club tried the effects of serpentary in the dose of from two to five scruples infused in four or eight ounces of water, and likewise in the form of powder in the dose of fifteen grains or one scruple. The result was that it excited the intestinal canal, produced determination to all the abdominal viscera, and occasioned flatulence. The usual symptoms were eructations, nausea, vomiting, pain in the stomach, colic, discharge of wind downwards, tenesmus. Sometimes, too, it produced a sense of heat, weight, and pain in the head, and at other times excitement of the circulation, or increased urinary secretion. Its effects lasted from eight hours with small doses, to twenty hours with large doses. It is therefore inferred to be a remedy which can only be useful when the practitioner wishes to rouse the vitality of the alimentary canal; hence in particular it may be useful in colliquative diarrhoea,

not connected with inflammation, as it tends to diminish the secretion of mucus.

Arnica.—*The Flower*, in the dose of from two to thirty-six grains infused in from one drachm to six ounces of water, was taken by thirteen individuals. It produced much irritation in the whole alimentary canal from the mouth to the anus, particularly in the gullet, stomach, and small intestines, exciting rather increased contraction in these parts than increased secretion. It likewise excited the brain, probably through sympathy with the intestines; and a distinct acceleration of the pulse, increased perspiration, and various modifications of the urine were also observed among its secondary effects. Its action lasted from twenty-four to thirty-six hours. The conclusions are, that the flower of the arnica may be useful in general or local debility of the intestines, brain, or locomotive organs, but is contra-indicated when there exists irritation or inflammation of the intestines, or congestion in the brain. Externally, an infusion in eight parts of water caused heat and itchiness of the skin without redness; and the flowers themselves also produced redness in eight hours, without affecting the cuticle. Thus they appear to be useful rubefacients of the mildest kind. *The Root* affects the muscular coat of the alimentary canal and the brain, more powerfully than the flowers; and hence M. Jörg thinks it may be useful in chronic or colliquative diarrhoea, without inflammation, where the object is to strengthen the intestinal canal without augmenting its mucous secretion. Besides these effects, both the flower and root appeared to have a powerful effect in stimulating the absorbent vessels, and may be useful, as they have been conceived by some to be, in promoting the removal of indurations of various organs. In this respect they have some analogy to calomel in action; but they have a greater tendency to excite inflammation.

Camphor was sometimes taken in powder in the dose of half a grain, and from that to twelve grains—sometimes in the form of tincture in the dose of half a grain, and from that to four grains. The result was, that it stimulated directly the brain and alimentary canal, and indirectly the urinary organs, the skin, and organs of circulation. The effects on the brain were very different

in degree on the different individuals—some experiencing much excitement, and being then thrown into profound sleep, while others remained free from any cerebral disturbance. It increased the heat of the body in all, and often produced sweating; it accelerated and strengthened the pulse, modified the quantity and composition of the urine, and excited the organs of generation. Camphor would thus appear to be contra-indicated in acute inflammatory fevers, and in irritation of the urinary organs, however contrary this principle may be to the usual therapeutic creed of physicians, and in every state of the cerebral system where congestion either exists or is apprehended. It is indicated in diseases of debility, and as it does not produce visceral congestion in the abdomen like the two preceding, it should be used in preference to them in tympanitis and colliquative diarrhoea, not connected with inflammation. The dose should be half a grain to one grain or upwards.

Castor in the dose of ℥ss. produced in all who tried it disagreeable eructations, but no perceptible effect on any system of organs, and in small doses it had no effect of any kind. M. Jörg therefore concludes that it might be erased without detriment from the list of the *materia medica*.

Musk was taken by nine persons in doses varying from two to fifteen grains. It appeared by no means so diffusible a stimulant as is generally supposed. It powerfully excited the alimentary canal and the brain, producing eructations, sense of weight in the stomach, and dryness in the throat; also sense of weight in the head, and giddiness. The secondary effects on the brain were more prominent; the symptoms consisting of yawning, somnolency, depression, and finally deep, prolonged sleep. When the dose was large, trembling and even slight convulsions of the limbs occurred. It excited the circulation, rendering the pulse fuller and more frequent. It never produced its peculiar odour in the sweat, urine, or other secretions. His experiments led the writer to infer, that musk can only be used in debility of the brain and nervous system, when no congestion is present—but that it is not so appropriate a remedy in extreme nervous debility as is commonly supposed, since the stimulus it produces is followed by a cor-

responding collapse. Three or five grains are a sufficient dose; the effects cease in eight or twelve hours.

St. Ignatius Bean.—From half a grain to four grains of the powder, or from nine to ninety drops of a tincture made with eight ounces of alcohol and one ounce of the bruised seeds, produced salivation, nausea, weight and pain at the epigastrium, eructations, colic, constipation, or purging, and irritation of the anus. Its secondary effects were weight in the head, giddiness, pain and inflammation of the eyes, and finally much prostration and apathy, sometimes with increase of pulse, and a sense of creeping and pricking in the urethra. These effects disappeared and reappeared several times. Professor Jörg recommends it in weakness of the stomach and intestines, especially when attended with induration of the mesenteric glands; in diseases of a periodic character, as a powerful alterative; and in atony of the brain and weakness of sight. The dose should be half a grain of the powder once a-day; it may be gradually increased.

Assafoetida.—This, in doses varying from one to fifteen grains, stimulated the alimentary canal, particularly the gullet, stomach, and small intestines. Secondly it produced cerebral congestion, indicated by dull pain and excitement of the circulation and respiration. It also produced strong excitement of the genito-urinary organs. Hence its use in indolence of the digestive organs he considers well-founded; but he is inclined to doubt the propriety of its almost universal use in hysteria and hypochondriasis, when depending on irritation of the uterine or abdominal organs. He thinks it is usually given in too large doses, and too often; from half a grain to one grain he generally found sufficient, and the effects often lasted till the second or third day.

Digitalis, in doses varying from half a grain to three grains, excited directly and powerfully the brain, the alimentary canal, and the genito-urinary organs. Its secondary effects were directed on the circulation, which it depressed. Its operation on the brain was indicated by intoxication, giddiness, dull headache, warmth of the face and dimness of sight; its operation on the alimentary canal by a sense of heat in the pharynx and gullet, colicky pains, and sometimes increase, sometimes diminution, of appe-

tite and of the alvine discharges; its operation on the urinary organs was evidenced by a remarkable increase in the secretion of urine, observed in all but one, and attended sometimes by diminution, sometimes by increase of its colour; its operation on the genital organs by itching of the glans, and by erections, and in females by symptoms resembling those which precede menstruation. The consecutive effects were marked depression of the circulation, indicated by feebleness and smallness of the pulse; which, however, were always preceded by excitement.—From these results, the author concludes that digitalis is far from being an appropriate antiphlogistic remedy;—that it is contra-indicated in hooping-cough, acute hydrocephalus, and all dropsies connected with inflammation of the serous membranes—the very diseases, in fact, in which it is chiefly used; but that it is very useful whenever the object is to stimulate the urinary organs to increased secretion. He questions the propriety of its employment in diseases of the heart, as its depressing operation must prove hurtful in some and useless in others. The dose should be from a quarter of a grain to a grain of the powder, and when its action has commenced the dose should not be repeated oftener than once every twelve, twenty-four, or forty-eight hours, as its effects often continue thus long. In unnecessarily large doses he is inclined to think it may tend to induce an inflammatory state of the urinary organs, and consequently diminution, instead of increase of the secretion of urine.—*Archives Générales de Médecine*.

CAMPHORATED OIL IN CHOLERA.

In lieu of cajeput, when dear and scarce, M. Champonnier, of Paris, recommends the medicinal use of a compound of olive oil and camphor. The proportion is twelve grains of camphor to the ounce of oil. The dose is a teaspoonful, in a warm drink, taken every hour if the symptoms are urgent, every second hour if not so severe. Tea, he thinks, is the best menstruum. The natives of India employ for the same purpose, and with good success, a warm infusion of the leaves of the *laurus camphora*, in sweet oil. M. Champonnier's design is to imitate them, and he adds, that he has found his compound a most valuable remedy, and a complete substitute for cajeput.

DIAGNOSIS OF HYDROCELE.

A very simple and ingenious method of ascertaining the transparency of the parts in obscure cases of hydrocele, has been hit upon by M. Segalas, and submitted to the Académie de Médecine. He merely applies to the scrotum one end of the eye-tube which is employed for exploring the urethra, and puts his eye to the other, upon which the existence of fluid, if it be there, becomes manifest. The result is explained by the isolated condition of the eye, the perpendicularity of the rays of light transmitted through the tube, and the pressure of the instrument against the scrotum. M. Roux, however, doubts the utility of this method in certain cases; for instance, where there is a cartilaginous thickening of the tunica vaginalis; and upon the occasion of discussing M. Segalas' contrivance before the Académie, the professor mentioned the curious fact of the extreme transparency of the scrotum in some examples of sarcocele. He was lately called in to a case of this kind, where the surgeon in attendance mistook the complaint for a simple hydrocele, misled by the transparency of the parts, and neglecting to take into account the specific gravity.—*Gazette des Hôpitaux*.

[M. Roux's objection, in the meantime, proves nothing; and perhaps the only circumstance that would tend to depreciate the utility of the new method, is, that it enables us to see *too well*—to discover transparency where such a thing would be least expected. With the tube and a candle light may be perceived through the palm of the hand.]

CLINICAL LECTURES,

Delivered in the Dispensary of the London University,

BY

DR. ANTHONY TODD THOMSON.

LECTURE I.

October 12th, 1831.

General Observations on Clinical Instruction.

GENTLEMEN,—In commencing the series of discourses which I propose to deliver, on the nature and treatment of the diseases which are brought under your notice in this institution, a few remarks on their utility may not be unimportant. Such discourses

are generally called *clinical lectures*; but, in the strict signification of the term, no discourses can be termed clinical except those which are delivered at the bed-side of the patient, and which have a reference to the immediate state of the disease which is there under the eye of the student. Were it admissible, in any situation, or under any circumstances, to disregard the feelings and to overlook the safety of the patient, for the benefit of the student, such a plan of instruction would be highly advantageous; but this is a sacrifice which humanity and justice equally forbid; and, therefore, the discourses to which the term clinical is applied, refer merely to cases actually existing, which have been recently seen by the student. Their intention is to explain the practice that has been adopted, and to make evident to the pupil the principles by which the practitioner treating them has been guided: in plain language, to display to the student the mode of applying and rendering practically useful the instructions which he is supposed to have previously acquired, in the various elementary branches of medical science, from attendance on lectures. Such is the nature of the instruction which you are to receive from me in this place: the real object of it is to make you acquainted with the phenomena of disease, and the effects of medicinal agents in counteracting these, or in restoring the healthy state of the system.

It has been supposed that dispensaries are little calculated to afford much information respecting diseases, from the absence of proper opportunities for tracing correctly the influence of medicines; and that it is in hospitals only, where the patients are completely under the control of the physician or the surgeon, that these objects can be fully attained. Far be it from me, gentlemen, to depreciate the utility of these institutions, which shelter, whilst they administer to the relief of the poor and friendless, whom disease has overpowered, or whom accident has rendered incapable of pursuing their ordinary avocations: they are the noblest monuments which can be erected to the glory of a nation, to those godlike feelings of benevolence and charity, which are the boast of England; and, assuredly, in no part of the world are their duties more ably or more humanely fulfilled. But to assert that it is in these institutions only that practical medicine can be taught, is assuming too much; for, although we must admit that, when the patient is completely under the control of the physician, the progress of disease can be most correctly and closely observed, symptoms traced to their sources, and the real influence of remedies ascertained, yet, it must be borne in mind, that this is not the state in which the practitioner is to combat diseases in private practice.

Under such circumstances, therefore, the treatment of every case must be regarded simply as an experiment, successful or otherwise, which teaches the practice of medicine, when the patient is placed in a situation the most favourable for the fulfilment of the various indications contemplated by the physician. If you suppose, gentlemen, that the results which you witness in the hospital are to follow the administration of the same medicines prescribed for similar disease in private practice, you will soon find yourselves woefully mistaken. It is not disease only that you have then to combat: injurious localities, occupations, unwholesome food, extremes of temperature, habits, passions, prejudices, all array themselves against your judicious efforts, and the best directed skill, and too frequently render it difficult to decide, how much of the benefit following any line of treatment is to be conceded to nature, how much to be attributed to art. If these remarks be correct, it is not too presumptuous to assert, that whilst hospitals are essential in the education of a physician—under which term I comprehend every well-educated practitioner of medicine—in order to teach him the nature of diseases and the value of remedies, dispensaries are equally so, inasmuch as they display to the student the effects of the treatment of diseases, under those circumstances which are likely to influence his practice, and to modify powerfully the operation of medicines, in every step of his future career. In these institutions, he sees the patients in the situations which they occupy in society—surrounded by all the inconveniences which attend poverty—borne down by vexations and anxieties—and, because free from the restraints imposed in an hospital, perhaps still under the sway of obnoxious habits, from the indulgence of which their diseases may, in a great measure, have originated. To observe the influence of medicines on disease, under such circumstances, is of the utmost importance to the student: he acquires an additional art, invaluable in the practice of his profession,—that of overcoming the obstacles to the accomplishment of his object, which seldom or never present themselves in hospitals. The hospital physician resembles a pilot who steers his vessel into a safe harbour, amidst the shoals and hidden rocks which obstruct its entrance, with the position of each of which he is well acquainted; but who performs his duty in a calm, and under the most favourable aspect of the heavens; the dispensary physician, and the private practitioner, is the pilot performing the same duty amidst the violence of the hurricane; he has not only to look out for the breakers, but to baffle the conflict of the elements.

In making these remarks, gentlemen, it is far from my intention to lessen, in the

smallest degree, the high appraisement which is usually bestowed upon hospital practice: the advantages to be derived by the student who witnesses it with the attention which it merits, are great and manifold: in no other place so well as an hospital can he attain an accurate knowledge of the advantages which remedial agents are capable of conferring; under no other circumstances, I would almost venture to aver, can he witness diseases unmasked and in their true characters; and, undeniably, in no other situation has he so favourable an opportunity of tracing their effects upon organization, and the changes produced by them on the tissues of the body. He not only enjoys the advantage of seeing much, but of investigating deeply; not only the benefit of having pointed out to him the complexities of disease, but also the probability of beholding them unravelled. To deny the utility of such establishments to the pupil would be folly; but, because hospitals hold forth such advantages, are we to refuse to Dispensaries those which they also are capable of affording? I am most anxious, indeed, to clear from your minds, gentlemen, the prejudices which have been so assiduously attempted to be impressed upon the student against the utility of Dispensary practice, by some hospital physicians and surgeons. Believe me, that, although it cannot be regarded as equivalent to that of an hospital, yet, to those who are disposed to observe attentively and to reflect on what they see—to reason, with the aid of their teachers, upon the phenomena of diseases, and to labour unremittingly in attempting to trace them to their causes; it holds out many advantages, not the least of which is the opportunity that it affords of studying exceptions to general rules, and, as I have already said, of anticipating those obstacles to success in the treatment of diseases which will invariably be encountered by them in every stage of their professional progress. You must perceive, therefore, gentlemen, that if the desire of acquiring a practical acquaintance with the phenomena of diseases, and of studying attentively the varied resources of medicine, be sincere, a Dispensary affords many opportunities of doing so; and of throwing, if not a strong, at least an indubitable light, upon the path of the student. Let me now endeavour to point out to you the manner in which these advantages are likely to be secured.

In the first place, be assured, that whatever may be your genius for observation, it will avail you little in the investigation of diseases if it be not based upon the foundation of an intimate knowledge with the elements of medicine—with anatomy, physiology, chemistry, materia medica, and those other branches of science which are justly regarded as valuable accessories in a medi-

cal education. Without the aid of these, indeed, it is possible that an individual of great natural acuteness, and possessing a talent for observation, may attain to considerable expertness in discovering and managing diseases, within the beaten track of practice; but the moment any thing extraordinary occurs, the uneducated practitioner, like the fair-weather pilot, loses his control, and is at once confounded with the vulgar. I must therefore presume, gentlemen, that you are at least somewhat advanced in these studies before you enter as pupils of this Institution.

The diseases which present themselves to the attention of the student in a Dispensary, consequently those which you are likely to see in this place, are seldom of that acute description which requires the daily, I may almost say the hourly, attendance of the physician: but, if they are not those of the most formidable kind, they are those of most ordinary occurrence, and therefore they require to be well understood and readily recognized. The talent for observation in medicine, as in general matters, is strengthened by habit; but it must first be acquired; and the acquisition is not to be accomplished without the closest and most attentive examination, not only of those symptoms which obviously present themselves to the senses, but of those which lie deeper. "He who is incapable," says Zimmerman, "of observing the moral man, will never be able to know diseases." To accomplish this, however, the observations must not be made in a cursory and partial manner; it is not sufficient for the pupil merely to listen to the examination of the physician; he must employ his own eyes and exercise his own reasoning faculties; if he do not fully comprehend the aim of any query in the examination of a patient, he must not rest satisfied until it be explained; and in affording this satisfaction, I trust, gentlemen, you will never find me backward. In the same manner, I am desirous of seeing you inquisitive with respect to the object and the intention of remedies; and believe me, that the more closely you examine their nature and effects, the more confidence you will repose in those that you employ hereafter, and the more you will have reason to hope from their administration.

One of the most important qualifications of a good practitioner, and, consequently, one of those which ought to be most assiduously cultivated by the student, is such a love of sincerity as will lead to the observation of symptoms such as they present themselves, not such as we would suppose them to be. It is by accuracy in observing effects that we gradually acquire the knowledge of causes; and it is by reasoning upon these that we are led to the indications and the means of cure. In recognizing any disease,

therefore, the description of which you may have previously read in books, or to the history of which you have listened in lectures, note down every exception presented by this individual case to the general appearance and progress of the symptoms, as detailed in the general history of the disease; for, as you will discover that all cases offer some exceptions to the general rule, you will find these particular histories of the greatest utility in your future practice.

Students are apt to fall into the common error, that the greatest sum of information is to be obtained where the greatest number of patients congregate; but crowded hospitals, or very well frequented dispensaries, rather embarrass the student than contribute to his instruction. He sees too much, and reflects too little; the rapid succession of objects does not permit him to fix his attention on any of them; in his endeavour to embrace the whole, he receives only an obscure impression of each; and, consequently, can neither understand nor retain the remembrance of those minute circumstances which have regulated the practice adopted; for "the understanding," as it has been justly remarked, "does not gallop so fast as the physician." It is these truths, gentlemen, which have decided me to confine my remarks, in the series of discourses which I am about to deliver to you, to some only of the cases which shall come before us, and to enter rather minutely into the history and the peculiarities of these, than to attempt a large field of research, or to indulge in discursive observation. The advantages of such a selection is acknowledged in the best clinical hospitals, both in this country and on the continent. In the Infirmary of Edinburgh, which contains nearly two hundred beds, the clinical wards to which the observation of the students is chiefly confined, usually contain only sixteen or eighteen patients; and more advantage is derived by carefully attending to this selection, than by daily walking, as it is emphatically termed, the largest hospitals of this metropolis. It is not the cursory observation of a great number of patients, but the attentive consideration of particular cases, which contributes to form the well-qualified practitioner.

Let me warn you also, gentlemen, against committing another mistake too frequent with the young aspirant to practice—I mean that of disregarding the most common phenomena of diseases, merely because they are common and obvious. It is from these chiefly that the student learns to generalize, and to establish his principles. Every disease consists of a series of symptoms following in a certain order. Extraordinary symptoms may occur, and ought, undoubtedly, not to be overlooked; but it is only by a previous and correct acquaintance with the common phenomena, that the deviations from them

can be recognized, and the knowledge of them become practically useful. Every symptom in the concatenation of phenomena is of value in reasoning upon the disease; and the loss of the smallest link in the chain may disable us from pursuing the mazes of the obscure labyrinth in which the origin of malady is too frequently involved. It is only by such a scrupulous examination of the circumstances characterizing diseases that they can be familiarly recognized—that effects can be separated from causes—and that the influence of remedies can be accurately noted. The disease ought to be as readily determined as the face of a well-known friend; and when the examination is once fully accomplished, the information acquired is not confined to the occasion which called it forth, but is extended to the whole life of the physician. It is this which constitutes experience; and a close attention to which distinguishes one practitioner from another. But in noting, carefully, the ordinary phenomena of diseases, you must note also the diversity of circumstances which, even in the same city, and in the same locality, constantly contribute to vary them in different individuals. To illustrate this by an example: two men, living in the same house, are attacked with acute rheumatism; one of them is a single man, with no cares nor anxieties except on his own account; the other is burdened with a wife and a large family, borne down with the *res augusta domi*, depressed in spirits, and exhausted in frame. Although the symptoms of the disease may closely resemble one another in both cases, yet it would be highly indiscreet to treat them in the same manner. Depletion might be useful in the one, and, although freely employed, yet may be easily borne; in the other it would prove injurious, even in a moderate degree. To cure both of these patients, whilst the general plan must coincide, a different method must be adopted in some particulars. The physician must therefore possess not only a distinct idea of the disease, but he must endeavour to become acquainted with the state of the body in which it found the patient; and with the causes which may have contributed to disturb or injure the ordinary functions, and to predispose it to the attack under which it is labouring; for it must be recollected, that every symptom is not the effect of the immediate exciting cause of the disease, but that some symptoms are the consequences of previous deviations from ordinary health.

I might now, gentlemen, proceed to point out to you the method of investigating diseases; in what manner you are to recognize and to separate symptoms constituting the disease and inseparable from it, from those which are only of occasional occurrence; how you are to be guided in forming your diagnosis, to give the disease a name, and to ar-

range it with its fellows in a nosological sense; how to discriminate the signs which may enable you to predict the event, and acquire that tact which always obtains reputation to the practitioner—skill in prognosis; and, finally, on what grounds you are to found your claims for public confidence, in the performance of the important and responsible duties to which you have dedicated yourselves. But as these are to constitute the business of our frequent meetings in this place, they will, necessarily, form the subject matter of the discourses which I propose to deliver as we proceed in the performance of the task allotted to us, respectively, as teacher and as pupils. In urging you to improve the present opportunity of acquiring the habit of attention and close observation in the examination of diseases, which will make the retrospect of your time spent here an object of satisfaction and pleasure, it shall be my endeavour to make my instructions clear and perspicuous to the least informed amongst you; and whilst I trust you shall be stimulated by my anxiety to set you an example of diligence and attention, you will also be convinced, that the man who undertakes the practice of the profession of medicine, commences a course of study which can only terminate with his life.

REPORTS OF CASES OCCURRING AT PUBLIC INSTITUTIONS.

LONDON HOSPITAL.

*Strangulated Scrotal Hernia, twice operated on.
Death—Post-mortem Examination.*

WILLIAM KING, aged 24, was brought into the hospital July 7th, about one o'clock, with a large strangulated scrotal hernia on the right side. It occurred while lifting a deal plank, about two hours ago, and has since rapidly increased in size. He has never been before subject to hernia. The tumor is very large and tense, and extremely painful; no impulse is communicated to it in coughing. He rejects every thing, his countenance expresses much anxiety, and he complains of pain in the abdomen. His bowels have not been opened since the occurrence of the hernia. The taxis was used without effect.

Ordered warm Bath, 100° Fahr.

While in the bath, further attempts were made to reduce the hernia, but without success. At two o'clock he was carried into the operating theatre. Mr. Luke made an incision, of about four inches, in the direc-

tion of the inguinal canal, and extending to the upper part of the tumor, and then carefully cut down to the neck of the sac, which the tendon of the obliquus externus was distinctly observed tightly girding, so that it was impossible to introduce a director between them; the stricture was therefore divided from without, by means of the point of a scalpel, and then, without opening the sac, the intestines were returned into the abdomen, on which the man immediately expressed himself relieved. The edges of the wound were approximated by two sutures; it was dressed, and a compress applied with a spica bandage.

4 P.M.—Sickness less; slight tenderness of the abdomen. Pulse 84.

V. S. ad $\overline{\text{xxvi}}$:

8 P.M.—Magnes. Sulph. $\overline{\text{3ii}}$.

Aq. Menth. Piper. $\overline{\text{3j}}$.

Syrupi Simp., $\overline{\text{3j}}$.

Cap. 2dis horis donec alvus responderit.

10 P.M.—His bowels have been open twice; the last motion contained a small quantity of blood; skin hot; pulse soft; slight tenderness of abdomen.

Ordered forty leeches to the abdomen.

Cal. gr. i. c. Op. gr. $\frac{1}{2}$ 2dis horis.

8th.—He has slept during the morning. Abdomen slightly tender; pulse 90, and soft.

Rep. Mist. et Pil. tertiis horis.

9th.—He has passed a good night; bowels freely open; wound rather painful.

10th.—He has passed a restless night; much troubled with flatus; bowels not open; abdomen tender; tongue white.

Hirud. xx. abdom. Ol. Ricini, $\overline{\text{3ss}}$.

11th.—No tenderness of the abdomen. The wound was dressed, and looked very well.

Vespere.—He complained of pain and tenderness of the skin on the right side of the abdomen, which was relieved by the application of twenty leeches. The Goulard lotion to be applied to the part.

12th.—Bowels freely open; tongue cleaner; skin less painful.

13th.—Has passed a restless night, and still complains of pain in his side.

Hirud. xx.

14th.—Pain in the side much relieved.

Porter, one pint daily.

The wound continued to heal very slowly, and a large sinus, nearly the length of a probe, was found to communicate with it, from which there was a great discharge of matter.

August 26th.—The sinus and wound nearly healed. About eleven o'clock at night he

complained of being unwell. Four o'clock the next morning he began to vomit; immediately afterwards the hernia descended; his sickness continued; the abdomen and tumor became painful; the pulse small, and quick, and countenance anxious; skin bathed in perspiration. The taxis was used in a warm bath, but failed. About eight o'clock Mr. Luke made some further attempts at reduction, but without success.

Ordered Ol. Ricini, ℥j. and a freezing mixture to be kept applied to the swelling.

On the failure of this, the operation was again proposed and acceded to. An incision was made a little to the inside of the former, down to the neck of the sac, through parts thickened from the inflammation consequent upon the last operation; and attempts were made to reduce its contents without avail. It therefore became necessary to open the sac; on doing which, eight or ten inches of dark-coloured and collapsed intestine, in a state of engorgement, were exposed. The stricture appeared to have been effectually divided, so that a finger could with the greatest ease be introduced into the abdomen. Much difficulty was experienced, however, in returning the intestine, and repeated failures preceded the last and successful effort, from the immense thickness of its coats not admitting of its easy passage through the freely divided stricture. It was at last returned by degrees, and the wound closed with sutures and dressed; pressure being made over the whole with a truss, to prevent reprotrusion.

[He continued to sink gradually, and died on the 7th of September.]

Examination of the Body, 2 P.M.—Upon opening the abdomen, the intestines appeared distended with flatus and were firmly agglutinated by lymph of a yellow colour; their peritoneal surface was very much inflamed; a portion of ileum of a dark colour was adherent to the internal ring; about an inch from the ring a contraction was observed in the ileum, which was thickened; the intestines beyond were not so much distended, or inflamed. The sac in the scrotum was inflamed and thickened, and at the bottom was observed the testicle, so that the opening to the tunica vaginalis had never closed, although no descent of intestine had taken place before the day that he was admitted. There was about a quart of pus in the cavity of the abdomen. The left testicle was situated just outside the external ring, and was small in size.

Fracture of the Skull—Trephined.

—Perry, aged 73, was brought into the London Hospital, August 24th, about two o'clock in the afternoon, having fallen into a ship's hold, the depth of which was

about twenty-five feet. There was a wound through the scalp and temporal muscle two inches in extent, situated above the anterior inferior angle of the parietal bone on the left side; the bone was denuded of its pericranium, and there was a fracture without depression, from the edges of which blood was oozing. There was also a wound through the scalp, in the same situation, on the other side; a slight hæmorrhage had taken place from the left ear. He was sensible; his pupils were rather contracted, and his pulse was quick, small, and irregular. His head was ordered to be shaved, cold lotion to be kept applied to it, and Cal. gr. ii. ter die sumend.

Vespere.—He had gradually become totally insensible, his respiration was laborious, and his pupils were more contracted—the left in a greater degree than the right. Some more bleeding had also taken place from the left ear.

V. S. ad ℥xviii.

25th.—He was completely insensible; pulse irregular and weak; pupils contracted, the left still more so than the right. As, from the situation of the fracture and the symptoms, effusion of blood upon the dura mater, from rupture of the spinous artery or one of its branches, was suspected, after a consultation it was determined to trephine. Mr. Hamilton enlarged the wound, and performed the operation over the seat of the fracture. Upon the removal of a circular piece of bone, the pulsation of the brain was observed, and a considerable quantity of coagulated blood, some of which was with difficulty detached from the dura mater with the handle of a scalpel. A large groove for the lodgment of a vessel was observed in the portion of bone which had been removed. Another piece of bone contiguous to, but above and rather behind the last, was then taken away, which was followed for a short time by some arterial bleeding. After the operation the man's pulse was fuller.

Ordered Cal. gr. v. quartis horis.

26th.—His mouth was slightly affected; he appeared more sensible, but was unable to speak: pulse regular, and soft.

Cal. gr. iii. octavis horis.

27th.—He appeared partly sensible, and could mutter a few words; he had passed a restless night.

28th.—His mouth was very sore; he understood a little when spoken to, and had a calm pulse.

Hirud. xii. capiti.

29th.—Slightly sensible; pulse 85, and soft; mouth very sore.

Rep. Hirud. nocte manequæ. Poullice to the wound, which had an unhealthy aspect. Omit Cal.

30th.—Much the same as yesterday.

31st.—He became quite insensible last night, but was a little improved this morning; pulse irregular and weak.

September 1st.—Quite insensible; takes no nourishment; pulse weaker.

2d.—Died at eight o'clock this morning.

3 P.M. *Examination of the Body.*—The wound in the scalp was in an unhealthy state. Upon removing the calvarium, some dark-coloured blood was observed upon the dura mater on the right side, and a small quantity on the left. The dura mater did not appear inflamed; when removed, the surface of the brain underneath that part of the skull which had been trephined, was, to a considerable extent, in a sloughy state; the substance of the brain was rather more vascular than natural. The fracture extended, on the right side, from the anterior inferior angle of the parietal bone, in two directions; one through the squamous and petrous portions of the temporal bone, the other through the temporal portion of the sphenoid; then uniting and passing through the sella turcica to the anterior inferior angle of the left parietal bone.

WESTERN DISPENSARY,

23, CHARLES-STREET, PARLIAMENT-STREET, WESTMINSTER.

Typhus Fever successfully treated with Tartar Emetic in large doses.

ANN EDWARDS, æt. 30, was admitted under the care of Dr. Lilburn, on the 3d September, 1831, while labouring under typhus fever in a very severe form. The patient had been confined four months since, of her second child, who is still living. She complains of great pain in the head, with occasional delirium; the countenance is pale and collapsed; tongue black, and dry; the teeth covered with sordes; pulse at the wrist small, irregular, beating 136 times in a minute; respiration hurried; secretion of milk suppressed. About ten days since, she was labouring under diarrhoea, which ceased spontaneously. The bowels are at present irregular; urine scanty. Petechiæ have appeared on the neck, arms, and body. The patient lies supine, and appears perfectly insensible to every thing around her, unless powerfully excited. The head to be shaved, and kept constantly cool with cold water.

Applic. Hirudines, vi. temporibus.

R Sulph. Magnesicæ, ʒij. Aquæ ʒj. M. bis indies sumend.

September 4th.—Continues much the same. The bowels have been opened twice.

Cont. remedia.

5th.—Much the same. The cold water to the head to be continued, and the follow-

ing medicine to be given, unless vomiting be produced.

R Antimonii Tartarizati, gr. j.

Syrupi Scillæ, ʒj. M. 4ta quaque horâ s.

6th.—The first dose of the medicine occasioned nausea, but it has been continued regularly, as directed. The tongue is less hard, but continues black; has had no alvine evacuation; urine still very scanty, and high-coloured; pulse 122; the skin is not quite so dry, but no sensible perspiration.

Cont. remedia.

7th.—Appears a little better; has less delirium, and the thirst is not quite so urgent; tongue beginning to get cleaner.

Pergat.

R Antimonii Tart. gr. j.

Syrupi Scillæ, ʒj. M. 2da quaque horâ s.

8th.—The bowels have acted twice; delirium and thirst abated; tongue moister; pulse 110.

Cont. aqua font. Capiti.

Cont. Mist. Ant. Tart. 2da quaque horâ.

9th.—The petechiæ are less livid; pulse soft and regular, 110; thirst lessened; has no vomiting; no alvine evacuation.

R Antim. Tart. gr. ʒss.

Syr. Scillæ, ʒj. M. 2dis horis s.

10th.—Slept well during the night, and is in every respect better this day; has had no return of delirium the last twenty-four hours. Pulse 100; the medicine produces nausea; she is able to sit up in bed.

Cont. Mist. Ant. Tart. co. 2dis horis. s.

12th.—The medicine occasioned vomiting last night, and was in consequence discontinued; she is better, and able to sit up in bed; one alvine discharge; pulse 100.

Cont. Aquâ capit. applicanda.

13th.—Continues improving.

Capt. Magnesicæ, gr. iij. ex aquâ bis indies.

23d.—Has continued to improve daily since the last report, and is now able to walk to the Dispensary with ease; appetite improving; tongue clean; pulse 76; bowels opened once or twice daily; urine natural in colour and quantity.

30th Sept. 1831.—Discharged cured.

NOTICE.

The name of the patient in the case of Aneurism inserted in our last, was different from that in the Clinical Observations upon it. This was a mistake on the part of the journal from which they were taken.

W. WILSON, Printer, 57, Skinner-Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF
Medicine and the Collateral Sciences.

SATURDAY, OCTOBER 29, 1831.

LECTURES
ON
THE THEORY AND PRACTICE OF
MEDICINE ;

Delivered at the London University,

BY DR. ELLIOTSON.

LECTURE IV.

Recapitulation — Predisposing and Exciting Causes of Disease—Temperaments—Idiosyncrasy—Semeiology or Symptomatology—Diagnosis—Prognosis.

I FEAR, gentlemen, that the first three or four lectures will be considered by you exceedingly dry ; but before entering upon the subject of any particular disease, it is quite necessary that you should understand the various terms which will be employed, and I therefore trust that you will not be tired of hearing the definitions which I feel myself under the necessity of giving.

I stated that pathology, or the doctrine of disease, was divided into two branches, general and special ; the latter relating to any particular disease, and the former to diseases in general. The observations I am now making are preparatory to entering upon special pathology—the nature, history, causes, and treatment of any particular diseases. I stated that general pathology was usually said to consist of four parts ;—one treating of diseases in general, their resemblances, and their differences ; another of their symptoms ; another of their causes ; and a fourth of their treatment. I have made all the general observations which I think necessary respecting the first division of general pathology—the differences and similarities between various diseases.

I proceeded to consider in the last lecture the various causes of disease in general. I

spoke both of those which predispose to disease, *causæ predisponentes*, or *proeugmentæ*, and those which are called occasional, exciting, or *causæ proeatacticæ*. The exciting causes of disease have been arranged by many in three classes ;—those which stimulate, those which depress, and those which act only physically or physico-chemically, independently of stimulating or depressing. I mentioned that the chief sources of the exciting causes of disease were derived, in the first place, from what appear to have been absurdly enough called “ the non-naturals ;” namely, the air we breathe, food and drink, the fluids proper to be retained in the body and those which are proper to be discharged, motion and rest, sleep and watching, and the passions of the mind.

With respect to the first of these non-naturals, the *air*, it may injure the body by its weight or levity, by the various electrical qualities which it may possess, by its temperature, by its dryness, and by its moisture. Various winds have various effects upon the human body of the most curious kind, according to the countries, or districts, or parts, over which they pass. With respect to temperature, it injures the body not only by its height or lowness, but also by its vicissitudes, and not only so, but accordingly as it is applied partially or generally to the body. A temperature that would do no harm if it were continued, would be productive of serious mischief if it suddenly succeeded an opposite temperature. A temperature which would do no harm if applied throughout the body, frequently does great harm if applied partially. Every body knows the danger of suddenly cooling, if the body be over heated and fatigued ; and of a draught applied to any one part of the body. Included, perhaps, under the head of air, may be mentioned various effluvia and various odours, as they are transmitted through the air.

Upon *food and drink*, another source of disease, I need say but very little ; they may

be injurious either by their quantity or quality.

As to what are called *excreta* and *retenta*—to speak first of *retenta*: the great thing here alluded to by old authors is the blood, and it was supposed formerly that it might greatly injure the body mechanically by its excess, either absolute or relative, so that *plethora* formerly occupied a very prominent part among the causes of disease. You will find some curious distinctions made by old authors on the subject of *plethora*. They called it absolute or true, or *ad vires*, or *ad vasa*, when the quantity of blood was absolutely in excess; they called it *apparens* when the blood was in its natural quantity, but expanded. They fancied it might be expanded by heat, and in that way a temporary *plethora* be induced. *Plethora*, too, was divided again into a third variety—*relative*. When there was no actual increase of blood, when the blood was not expanded improperly by heat, but when the space in which it moved had become too small, this was called relative *plethora*, or *plethora ad spatium*; where the vessels, it was supposed, became contracted, and the blood was in excess to merely the contracted space. *Plethora*, however, certainly may be either *general* or *local*. A local *plethora* is what in modern language is called an inflammation or congestion, just as the case may be. When the blood is really in excess, I believe that it is generally of too rich a quality; that there is too little of its watery portion, and too much of the crassamentum. I need not say that the opposite of *plethora* is *inani-tion*. This is a state which is induced by excessive discharges, or from the want of food; so that the blood which was in the body is let out, or the blood which naturally escapes in the various excretions is not replaced. Anæmia, or bloodlessness, but more properly a watery state of the blood, will arise from an ill understood, I may say, not at all understood, state of the system, in which, although food is taken, nourishment is but very sparingly extracted from it. Cases of this description are seen continually: chlorosis is one of this kind. But the blood sometimes certainly is in fault with regard to its constituents, not merely as to whether it is too rich or too poor, but, as it would appear by some recent experiments of a friend of mine, Dr. Stevens, who is about to publish a work on Fever, and Dr. Clanny, by the saline ingredients erring in their quantity; so that the blood, in many cases of disease, is absolutely deficient in its saline particles. It is evident, however, that diseases of the solids themselves will sometimes cause these aberrations of the blood; so that the blood itself is not the cause of the disease. It may become an instrument of farther disease, though disease of the solids originally may

have given rise to the aberration of the vital fluid.

With respect, however, to the excreta, you may easily conceive how excessive discharges will occasion various diseases. The excessive discharge may consist of blood, of bile, of secretion from the alimentary canal, fæces, of urine, of perspiration, of saliva—that, however, seldom does much harm—and of semen. These are the fluids which particularly escape from the body, and the escape of which may do harm. But with regard to the latter, the discharge of semen, although one must suppose that the simple excessive loss of the fluid must be injurious, yet it is very certain that the mode in which that fluid is lost exerts a very great influence upon the constitution.

In regard to *motion* and *rest*, I need not point out the ill effects of sluggishness on the one hand, nor of fatigue on the other. Nor with respect to *sleep* and *watching* need I make any remarks, for they come under the same head as *motion* and *rest*; *sleep* and *watching* being to the brain, what *repose* and *motion* are to the body.

With respect to the *passions of the mind*, I mentioned in the last lecture that they are a frightful source of disease; much more so than is commonly imagined. An immense number of cases of disease of the heart and disease within the abdomen, as well as of the brain itself, arise, I am certain, from unpleasant passions of the mind. However, it is not merely unpleasant passions that affect the body injuriously—*violent* emotions of the most pleasurable kind will sometimes operate in the same way. History relates many accounts of persons who have died from excessive joy.

Besides those circumstances, however, which I have just mentioned, the body is liable to injury from two very common sources, one of which is *poison* of various sorts, which poison may enter the system by the surface, may enter it by the breath through the medium of the lungs, or may enter it by being swallowed; and it is a very curious fact, that many which act offensively when admitted into the body one way, have no effect when admitted in another. I believe it is a fact, that the poison of hydrophobia, or the poison of a serpent, may be swallowed with the most perfect impunity.

In addition to poison, the body suffers much from *mechanical injuries*—mechanical injury, of course, is a cause that may affect almost any part of the body.

Among the *causes* of disease which chiefly act by giving a predisposition, we may mention that of age. All periods of life are subject to their own diseases, whether we divide them with Shakspeare into seven, or with those authors who mention five, or those who mention more. Every

age is particularly liable to certain diseases: for example, you rarely see gout in an infant, nor is it common for old persons to have the symptoms of acute hydrocephalus. In short, every age seems marked out as particularly liable to be, if not destroyed, at least impaired, by particular diseases. The same is the case with respect to sex: each sex is particularly liable to its own affections. Hysteria, which occurs only now and then in the male, occurs far more frequently in the female, and even when there is reason to suppose an affection of the uterus. Of course there are certain diseases which can affect but one sex.

As to temperament, disease depends much upon it. We are all of some temperament, or some combination of temperaments, but excess of one temperament gives a great liability to disease. You know that the usual divisions of temperaments are four—the sanguineous, the melancholic, the phlegmatic, and the choleric. The sanguineous is marked by smoothness of skin, softness of hair, a quick pulse, a warm surface, great excitability of the whole frame, and generally a fair skin with florid complexion. The melancholic, on the other hand, is marked by a slow pulse, by a dark swarthy complexion, dark and strong hair, and less susceptibility of emotion—that is to say, of sudden violent emotion, although when certain emotions take place they are said to be more continued. The phlegmatic is marked by torpidity, paleness, and flabbiness. The choleric is characterized by reddish hair and great excitability. However, these divisions, although they were made by the ancients, are not so much attended to now. We certainly often see these temperaments strongly marked, but they are all infinitely combined.

An attempt has been made within these few years to divide temperaments differently, according to the activity of particular parts. For instance, some persons have the chief activity in the head; they have a large intellectual development of the head, as some people say, and that would be called perhaps the *cerebral* temperament. Others, again, have a large broad chest, with a considerable development of all the muscles, and this is called the *thoracic* or *muscular* temperament. And again, others are sluggish, prone to eating and drinking, with large bellies, and are said to have the *abdominal* temperament. Now it is rather an abuse of terms to call these temperaments, for I think the word *temperament* rather refers to the constitution and character of the whole frame together; yet there is no doubt that these divisions occur, for you see some persons all intellect, others all muscle and chest, and you see some portly, with their big bellies and lean pates, their heads suffering in proportion. In truth, Dr. Tho-

mas entitles his work, *Physiologie des Temperaments ou Constitutions*.

However, whether you choose to call these temperaments or not, it is well to know that these persons will suffer various diseases, according to the division under which they come, just as is the case with regard to those who are of a sanguineous, phlegmatic, choleric, or melancholic temperament. They will be particularly liable to diseases of the head if they be of the cerebral conformation; they will be subject to affections of the chest, not of a phthisical, but of an inflammatory kind, if they have a full thorax; and of course they will be subject to various hepatic, and other abdominal diseases, if the abdomen gain the sway. It is worth while to attend to this other division of temperament, although, perhaps, the term itself may be objectionable.

Besides original temperament, acquired peculiarities take place by a long residence in a particular climate: by long residence in a particular spot certain dispositions of the body are engendered, and a second nature, if I may so say, is produced. The various occupations of life have the same effect; the various trades have their various diseases. On this subject you cannot, I believe, read better works than Rammazini's and Thackrah's. Custom and habit, whether referable to trade or situation, or action, or any other circumstance, have also very great effect in producing or in preventing the cure of disease. For example, if a person be in the habit of dram-drinking, and has a sore, nothing is more common than to see that sore not heal, in spite of the best surgical treatment, till the person is allowed his dram. After a surgical operation a person has been known to sink through the surgeon not having been aware that the individual had indulged in some bad custom until it had become a bad habit. From the want of a patient being allowed a quantity of gin in the course of a day, he has died after an operation, whereas he otherwise might have done well. This has been strikingly shewn in other cases where the knowledge was happily attained; and a good allowance of gin after some severe accident has caused every thing to go on well, though the patient at first appeared sinking. Bad habits are, therefore, sometimes to be indulged, although they should certainly never be acquired.

Independently of what is called temperament, an individual may have a particular disposition not acquired; it may be born with him, and be hereditary. A peculiarity is sometimes so exceedingly singular, so totally different from what we observe in other people, that it is called *idiosyncrasy*. It is nothing more than a peculiarity either original or acquired by habit, and sometimes it is only connate, and sometimes hereditary. As an instance of this, you will some-

times find a person who cannot eat a certain article of food. I recollect seeing a young woman who could eat the hardest salt beef, and digest it well; but if she took a raspberry, or currant, or any fruit, she was instantly thrown into the most violent spasms of the stomach, so that a stranger would have fancied her life in danger. These are called idiosyncracies, and it is of great use to know them, because some persons are peculiarly affected by certain remedies; and that which is a proper remedy for the disease may be improper for a particular patient.

So much, therefore, gentlemen, with respect to the causes of disease in general—to general *ætiology*; and I will now proceed to consider the third branch of general pathology—the symptoms of disease in general—which is called *semeiology* or *symptomatology*.

I believe I mentioned that *symptoms* are every thing which are observed in a patient out of the course of health; *singulæ quæ in agro præter naturam observantur res*. The most evident, the most constant of these, are put together, and said to constitute the disease. You will recollect that the disease is not the cause of the symptoms, but that the disease is the collection of symptoms. I mentioned this, and illustrated it with respect to jaundice and to epilepsy, the outward signs of which are jaundice and dropsy, while the inward state is the proximate cause.

Now these symptoms may be *essential*, they may be what is called *pathognomonic*, and they may be *accidental*. The *essential* symptoms of disease are those which are necessary to make the disease—to constitute the idea of the disease. You cannot, for instance, suppose a patient to have pleurisy unless there be a degree of feverishness, with sharp pain of the side, increased upon respiration, together with a cough. These symptoms would be called the *essential* symptoms of the disease, yet no one of them would make the disease. A patient may have feverishness without pleurisy; or a sharp pain in his side without pleurisy, because it may be muscular or it may be neuralgic; or again, he may have cough without any pleurisy. These are then, you perceive, *essential* to the disease altogether, but no one of them constitutes it. If, however, a symptom be so important to the disease that this cannot be present without it, then it is called *pathognomonic*—a symptom without which you could have no conception of the existence of the disease. There are, however, but very few of these symptoms. In jaundice, the yellowness of the skin, of the *sclerótica*, and of the nails, is *pathognomonic*. Without making any farther inquiry, you may be sure that the patient labours under jaundice; but in general we make out the

disease from the groups of *essential* symptoms, and from taking into consideration every circumstance of the case. Besides these symptoms, others are *accidental*. Many cases have symptoms which are merely *accidental*, and are not necessary at all to the disease: the disease has frequently existed without them.

You will find symptoms also spoken of as *positive* and *negative*; and it is of great use to attend to the latter. A *negative* symptom is perhaps, an improper expression: a symptom must, properly speaking, be *positive*, but, from the absence of certain circumstances, of certain symptoms, you make out frequently the nature of a disease. If there be two diseases with certain symptoms common to both, but one of them has a symptom which the other never has, then, if that symptom be absent, it enables you to make out the true nature of the case. Hence the absence of certain symptoms is frequently of as great importance to be attended to as the presence of others.

Now, from the presence of the symptoms chiefly, but not entirely, we make out the character of the disease; and the determination of the character, the name and nature of the disease, is called the *diagnosis*. To speak again of jaundice, from the yellowness of the skin, the whiteness of the *fæces*, and the dark colour of the urine, you make your diagnosis, and say the disease is jaundice, although, indeed, you may do so merely from the yellowness of the skin and the *sclerótica*. But you will frequently find your diagnosis is not easily made from the symptoms alone—you must look into the exciting and predisposing causes of the disease: you must ascertain what the patient is likely to have—what predisposition he has; you must also ascertain to what exciting cause he has been exposed; and then conceiving what it is likely would ensue, and observing what actually has ensued, as far as you can judge, you are enabled much better to form a conclusion as to the real character of the affection. You will find your diagnosis also materially assisted by attending to the history of the case; not merely observing the symptoms that present themselves to you, but inquiring carefully of the patient the whole story from the beginning. You will thus ascertain whether these circumstances confirm what you suppose to be the nature of the case. You will also find it of great importance, in making out the character of the disease, to learn the effects of any previous treatment that may have been adopted. If you learn that vigorous treatment has been properly employed, and has had no effect whatever, you would doubt very much as to the propriety of the view which had been taken with respect to the nature of the affection. In making a diagnosis all these points are to be scrupulously attended to.

It is in making an accurate, careful diagnosis, that the medical practitioner chiefly shines. When the nature of a disease is once ascertained, it is no difficult matter to treat it; other qualities of mind are then required—frequently only courage, or mere perseverance—but it is in making a diagnosis, in ascertaining the character of a disease, that the scientific practitioner outshines the inferior. Unfortunately this is a point not attended to as it ought to be. Many persons pride themselves in being good practitioners, because, without knowing what is the matter, they can say what will do good. It is an unscientific way of proceeding, and, if I could not practise better for making a good diagnosis, still I would be as particular on that point as possible, for the sake of observing a good general rule—for the sake of endeavouring to treat the patient better than I otherwise could, notwithstanding all my conceit—and for the purpose of being ready to meet any unexpected emergency that might arise. A medical man who will not take the trouble to establish a diagnosis, is just like a surgeon who will not condescend to learn the anatomy of hernia, but who says that he knows if he cuts this way and cuts that way, he shall liberate the entangled parts, and do the operation as well as the best. I have witnessed this; but, then, what are such men when a difficulty arises? They are lost in perplexity, and glad to apply to those who know better. It is impossible to be too minute in making a diagnosis.

You have not only to make a diagnosis as to the general character of the disease, but you have to ascertain what variety of the affection it is—whether it is one with strength or weakness—and to weigh the minutest circumstances respecting the case. This is, perhaps, an exercise of patience, but it is an exercise highly gratifying; especially when you find you have made a correct diagnosis.

The judgment you form as to what will happen in a case, is called *prognosis*. In order to make a very correct prognosis, it is best to make an accurate diagnosis; for if you do not know what the affection is, you will often be unable to tell how it is likely to terminate. For the sake of making a prognosis you must observe all the symptoms, and learn the history of the case, exactly as in making a diagnosis; but there is something more to be done than that—you must know the direct tendency of the disease, the course which the disease is always inclined to run, the course which it is likely to run in the particular individual, according to his age, according to the sex, according to his previous habits, according to all those circumstances which I have previously mentioned as to his individual peculiarities, and accordingly as you have it in your power to

put into practice requisite curative means. In some cases, if we were allowed to practise our art freely, we should make a favourable prognosis; but our hands being tied, as they sometimes are, when others ill informed or inactive are called in over us, we must give an unfavourable prognosis, simply because the patient must be allowed to die. In making a prognosis there is frequently less skill required than in establishing a diagnosis; for some persons, with a sort of tact, will tell whether an individual is in danger or not without knowing what is the matter with him. Many persons who have no knowledge of medicine, or at least very little, have such strong perceptions, that, from the aspect of an individual, and from general observations of their own, they are enabled to give a very correct prognosis, either favourable or unfavourable. This I know to be the case. There are a great many intelligent officers who, from seeing their men fall around them in battle, sick in hospitals, have become, in one sense of the term, good doctors; and though they are not able to cure the disease, or tell what is the matter, they will be able to give a shrewd and correct guess as to how matters will turn out. But although some people, by nature, are well qualified for this, while others are not, yet the medical man must be taken as he is; and whether he does or does not possess natural sagacity to make a prognosis, it is his duty to make as careful a diagnosis as he can, in order that he may make as good a prognosis as possible. If his intellect be not of an acute description, yet, by knowing the case, and by the aid of science and minute observation, he will be able to form as good an opinion as the man who by nature is qualified for these things; and if with science and observation is combined native sagacity, then I need not say that his prognosis will be much superior to that of the individual who only trusts to his natural powers.

You therefore see what is meant by diagnosis and what by prognosis; and I cannot urge upon you too strongly the most careful attention in establishing the former. Whether you can cure the patient or not, you should make it a solemn rule to investigate the case to the utmost. It is only by this that we can profit ourselves, or that those who come after us can profit. If we be not careful we cannot advance science, and the next generation will be none the better for us.

With respect now to the symptoms of disease on which diagnosis and prognosis are in a great measure to be founded. The symptoms of disease may be divided, perhaps, in one point of view, into two kinds—those which are discoverable by the patient only, and those which are discoverable by by-standers also, or only.

Now there are some symptoms which a patient alone can ascertain—for example, his feelings. He it is that feels the pain, and it is a lucky circumstance that it is so; for if the doctor felt the pain as well as the patient, there are but few who could stand practising the profession. We know little about pain but from the patient's account; and in many cases in public practice we are deceived. We have, however, only done our duty in listening to the patient's account.

With respect to *pain*, it is of various kinds; it may be sharp, stabbing, darting, shooting, lancing. These are all words used by patients. At other times the pain is not of this character, but dull, and then we call it an *ache*; and sometimes patients add the word *pain*, making it *aching-pain*. Sometimes you will have pain described as throbbing and pulsating; and you will hear patients also complain of pricking pain, as though a number of needles were being pressed against them. Now these things are all of great importance to be attended to, because they shew frequently the seat of the affection; and accordingly as a patient describes his pain, as sharp, or dull, or throbbing, or pricking, so you ascertain frequently whether he is telling the truth or not, because you see for other reasons a probability that, if he have pain at all, it is of a particular character, and you ascertain whether his account agrees with what you would, *a priori*, expect him to describe. You will sometimes hear pain spoken of as smarting, and a very high degree of this is broiling. In some affections of the abdomen you hear a patient complain of broiling pain; it appears to be a high degree of smarting, with a sense of burning.

Now if pain be increased by pressure, or by the application of any stimulants, then we say there is *tenderness*; but perhaps the word *tenderness* is more properly restricted to pain increased upon mechanical pressure. There are other feelings continually described by patients, as, for instance, a feeling of dead weight. This is particularly the case with respect to the head. You will sometimes hear persons complain of fullness, as if a part were excessively full; and now and then this sensation arises to such a pitch, that the part feels as if it would burst; and then we call it a feeling of tension. A feeling of tension appears to be a high degree of a feeling of fullness. You will now and then have patients complain of a sensation of pins and needles. It is very different from the sensation of pricking pain, as if needles were being run in; it is as if the pins and needles were rather blunt; it merely gives the lowest sensation of this kind, not amounting to pain. Pricking pain is of a very different character—for instance, that which people frequently complain of from inflam-

mation of the urethra in gonorrhœa; they experience a pricking pain along the urethra; but the sensation experienced when the hands and feet are asleep is not called pain; patients say they feel as if pins and needles were there. You will frequently hear persons declare, that they have an indescribable uneasiness in a part, a sensation which nearly drives them mad; but they will not allow it to be pain. I have known this sensation occur in different parts of the body. I recollect one lady who had it in the neck, so that she was obliged to walk about the room followed by a servant rubbing her neck night and day. Unless this was done the sensation nearly deprived her of her senses, and her countenance betrayed the greatest distress. You will find other sensations spoken of by patients—a creeping, a crawling, or a feeling as if water were trickling down them. There is also a symptom which approaches nearly to pain, itching and tingling, which may be almost insupportable. Tingling is a high degree, I imagine, of what is called itching, united with a sense of pricking. In regard to other sensations, there is nausea felt in one part—the stomach; and now and then persons have an excessive sensibility of one particular sense—of the eye or the ear.

Now all these are symptoms which the patient alone can know any thing about. We all know something about them, because we are more or less ill at some part of our lives; but the medical attendant must take the patient's word for these sensations, at least he cannot ascertain the existence of them directly—he can only ascertain by other symptoms whether the patient is probably telling the truth.

Besides these, people have a feeling of great exhaustion, of great debility, and this is a point strictly to be attended to in forming the prognosis in an acute disease, for sometimes it is a fatal symptom. When a patient, for example, after inflammation of the bowels has no pain, but a feeble pulse, and complains that he is so weak that he feels as if he should die—that he never felt such weakness in his life—you may be almost sure that he will be dead before the next day. On the other hand, this feeling is very deceptive. Women are subject to a sinking sensation at the epigastrium; they tell you they feel as if they had no inside, and they are sure they are about to die, but the feeling is quite delusive. You might in the former case, after an acute disease, judge from seeing all, or the greater part of the symptoms decline, that the patient would soon get well, and that this feeling of weakness was nothing more than might be expected after severe indisposition; but you must take into account other symptoms, particularly the weakness of the pulse. But with respect to these women you find

the pulse good, and they are able to walk about notwithstanding all the depressive feelings of which they complain. It is a very remarkable circumstance, and I shall find it necessary to draw your attention to it when speaking of diseases of the stomach, and of hysterical affections. Patients also alone can be conscious of drowsiness, want of appetite, inordinate appetite, and depraved appetite, although with respect to many of these things you can often by other circumstances ascertain whether they are telling the truth or not. It is to the patient, likewise, that you must trust for an account of the existence of tenesmus and of strangury—a frequent desire to go to stool, and a painful and frequent desire to make water. All these are instances of symptoms, of which you must receive an account from the patient himself.

There are some such symptoms, too, which are derived from other senses than those of general feelings, and these are referable to the ear. Patients will continually complain of the most violent beating pulsation in their head, and frequently, too, of a snapping and cracking there. Such symptoms undoubtedly do exist, but we can take no cognizance of them. With respect to sight, you must depend on the patient's account for the occurrence of flashes of light before the eyes; and for the symptom so common in many diseases—moats floating before the eyes, and for double vision or diplopia; and likewise for the symptoms of giddiness or vertigo. On the patient also you must depend for symptoms with regard to taste; some have a bitter nauseous taste in their mouth, of which you cannot judge. So with respect to smell; now and then persons have a most disgusting disagreeable smell, which no other person can notice. There was a case of hydrophobia at Guy's hospital, in which there was an instance of an accidental symptom, and that a symptom to be ascertained only by the patient—the patient experienced the most disgusting smell. You will find in cases of insanity that this occasionally happens. Now and then the smell which the patient perceives may be noticed by other persons, because it arises from some disease within the nose.

There are still other symptoms of which the patient alone can give an account—a defect in the senses, a loss of sight, a loss of taste, a loss of hearing; for all this you must depend much on his account. So with respect to many mental circumstances—the loss of many desires, and a feeling of strong desires; for all this you must rely upon the patient. You see, therefore, that in regard to symptoms, they may be divided into two classes, the first of which are evident to the patient alone, and the second those which are perceptible to the practitioner also or only.

At the next lecture I shall proceed to consider those which are of the latter description.

ON THE MEDICAL TREATMENT OF INSANITY.

Being one of the Croonian Lectures delivered before the College of Physicians, in May, 1831,

By EDWARD J. SEYMOUR, M.D.
Physician to St. George's Hospital.

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Blood-letting—Mercury—Cold applications to the Head—Counter-irritation—Sedatives—(Acetate of Morphia—Belladonna, &c.)—Arsenic—Camphor—Purgatives—Diet.

THE application of medicine to mental disease must, of course, be directed, in the first instance, to the nature of the attack. It will become necessary carefully to inquire whether the brain is primarily or secondarily affected, and whether such affection is or is not connected with organic lesion. The physical causes must be carefully separated from the moral, and due weight assigned to each. If, on accurate inquiry, the patient has been exposed to causes sufficient to produce inflammation of the brain and redness of the countenance, injection of the conjunctiva and heat of skin very early in the disease point out increased vascular action, with increased power; antiphlogistic remedies, with blood-letting, principally from the jugular vein, are to be employed; and in such cases, and probably only in such cases, mercury may be employed, to affect the system; such cases will often recover. In the summer of 1827, I had occasion to visit a gentleman, æt. 30, whom I found in the following condition. He was lying on a sofa, in a state of great agitation, and complaining that he had committed some great crime, and was the object of a conspiracy. He complained of pain in the head, and would not permit me to feel the temperature of it. The pulse was quick and oppressed, and I recommended that he should lose blood. The first bleeding was borne very ill, but the second, to the amount of sixteen ounces, was attended with some relief, Cold was applied frequently to the head, and blood abstracted by cupping every day for several days, during which time he took calomel every four hours. As the mercury began to affect the mouth, a visible change in the mental malady came on; this gradually increased in the most favourable manner, and the patient entirely recovered, nor has he suffered the smallest relapse since that time. I had the advantage of the assistance of Mr. Tupper in this case. This case is only, however, an example of a few where the mental disease is going on in consequence of increased vascular action; in the great majority of cases the functions of the brain, in mental derangement, are increased in force, while the circulation is depressed, extremely quick and feeble, and the action

of the heart giving way at the smallest abstraction of blood; and yet these are often attended with raving delirium, great increase of muscular force, and are, in fact, what are termed high cases. The consequence, I am informed, of such practice, is either the more frequent returns of the high stage, or the patient sinks into one approaching idiocy.

"At the admission," says Pinel, "of any lunatic into the hospital, great care is taken to interrogate the relations on the subject of blood-letting; and the question is asked, whether it has been employed, and what was the result? The replies the most constantly made attest that the condition of the lunatic has constantly become worse immediately after bleeding." I think I ought not to omit a curious fact which occurred in the year 1813. Two young persons, of a similar age and temperament, arrived the same day; one of them had not been bled, and her cure was effected in two months; a copious bleeding having been exhibited to the other, she was reduced to a kind of idiocy, and did not recover the use of speech until near the fifth month; her complete re-establishment was not effected until nine months. We saw afterwards a singular example of a melancholic patient, who had been bled five times from the foot and three times from the jugular vein, and had fallen afterwards into such a state of stupor and debility that she passed several days without taking any nourishment." Dr. Haslam informs us, that it is only in the very early stages of the disease that blood-letting is useful, and then he prefers cupping. It is also very remarkable that, according to his testimony, the appearance of the buffy coat on the blood, generally considered to justify such practice, has been found generally absent in cases of lunacy where venesection has been used. In more than two hundred patients, male and female, who were let blood by venesection, there were only six whose blood could be termed *sizy*. The experience of Pinel is very decided against this practice.

The experience of any individual, however zealous, must be extremely limited; and I do not venture, therefore, to detain the College with the result of my personal inquiries. I have endeavoured to obtain the result of medical practice on a large scale, and for this purpose selected Mr. Warburton's house, the White House on Bethnal Green, containing 400 patients, and where the skill of Mr. Beverley, the superintendent, and Mr. Phillips, the resident surgeon, emboldened me to offer to the consideration of the College facts which have resulted from their extensive and assiduous observation, which I could never hope to have obtained from any private source. It is to these gentlemen I am indebted for the principal observations I am enabled to relate.

Messrs. Beverley and Philips state the following as the result of their observations on this subject:—

"The number of patients admitted with vascular excitement, requiring blood-letting, are very few indeed; we seldom or ever use the lancet in cases of excitement, if there is no evident effect upon the brain from increased arterial action, so as to lead us to fear an approaching attack of apoplexy or paralysis. The reason we do not use the lancet in cases without any such symptoms existing of disease going on in the brain, is that we have done so in several instances, and the result was not favourable; the patient became reduced from the loss of blood, and the excitement not abated, the powers of the constitution gave way, the tongue became typhoid, and the patient sank into a state of collapse and died."

As the result, then, of experience in cases of excitement, I presume that these arise from increased nervous energy, not depending on increased action of the heart and arteries, but on increased sensibility of the brain itself, and that blood-letting is not found useful.

Hence physicians have been under the necessity of seeking for means to allay the inordinately increased sensibility principally of course occurring in that division of insanity termed dementia. These remedies are various. I shall commence with cold, which may be administered in three ways—in the form of ice, in the shower-bath, and in a column of water, graduated according to the strength of the patient, and termed the "*douche*," a French word, for which I know of no corresponding one in our language. Of ice to the head, not only in order to diminish vascular excitement, but also to produce a really sedative effect, a diminution of intensity of sensibility of the brain itself, I can speak with some confidence. In the epidemic typhus fever which has recently raged among the lower classes in this town, and which has been of a marked and peculiar character, with scarcely any, if any, stage of excitement, the application of ice in the low maniacal delirium which accompanied it was attended with the happiest effects, and in more than one case appeared to be the principal means of cure; the patient sleeping quietly for hours under an ice cap, who, previously to the use of the remedy, had passed the night in low muttering delirium, or in the constant endeavour to leave his bed. This remedy appeared to me to be useful exactly in proportion as the delirium assumed the maniacal character. The torpor produced by extreme cold may convey a tolerably accurate idea of the modification which the extreme sensibility of the brain may receive from this remedy. The effect which pouring cold water on the head, in cases of affection of the brain with in-

creased vascular action, produces, may be illustrated by the following very remarkable case, which I attended with my friend, Dr. Roupell, physician to the Seaman's Hospital. A young medical man was attacked with symptoms of inflammation of the brain, while pursuing with great ardour his professional studies; and being a medical man he was, of all other persons, the most difficult to treat. Notwithstanding the intense pain in his head, his quick and frequent pulse, he resisted remedies. Venesection he would not submit to, leeches he had an objection to, calomel he thought produced inflammation and ulceration of the mucous membrane of the intestinal canal;—it was proposed to pour cold water from a pitcher over his head; the consequence was diminution of pain, quiet sleep, and, in fact, so beneficial an effect that the patient himself frequently called for a repetition of the remedy. I saw this patient several times, and the impression on my mind was, that to this remedy, and to this alone, he owed his life in that attack.

The shower-bath appears to have a very similar effect, and I am enabled to quote a case of its benefit in mania, from the testimony of the gentlemen to whom I have alluded.

"We have found, in some cases, the shower-bath of great service; but it appears to us, from the experience we have had of it, that it is more beneficial in cases of a very violent nature, with increased vascular excitement, as we have given it a trial in cases of various descriptions, and in some without the slightest benefit.

"There was a case admitted into this establishment, evidently a case that was completely cured by the use of it. A gentleman, aged 30, small, and light complexion, had been studying hard, and constantly confined to one room, was attacked with furious mania; thought he had found out perpetual motion, and that he could make the sun stand still. Pulse very quick, 120, and small; pupils contracted; imagined he could reach any thing he saw, and grasped at them; incessantly talking; tongue furred and dry. We ordered him to go into the shower-bath. His extreme violence put us on our guard, that we got six keepers to take him there. We persuaded him, with much difficulty, to go in, by saying it was only a sentry-box. On hearing that, he immediately went; the door was closed and secured; the shock was so unexpected that he screamed and held his breath for a short time after the shock was over; then gasped, and knocked the sides and the door into pieces, and stepped out; but was immediately secured, rubbed dry, and put to bed. He had a little refreshing sleep during the night. In the morning he vowed vengeance against the doctors for murdering him. We could not prevail upon

him this day to go into the bath, which obliged us to confine and carry him there. He bore the shock better; was taken out, rubbed, and put to bed. Slept better; the tongue appeared cleaner, and he was not so violent. Bowels open; he begged to be released from confinement, which was complied with. He took a little exercise; we put him in the shower-bath almost without any difficulty; sleep returned; gave him a dose of calomel and colocynth; more rational; he inquired what had been the matter; thought he had been asleep, and in the evening begged himself to go into the bath and have more medicine. From that time he became tranquil, took mild aperients, and was discharged well in a fortnight from the date of his admission."

"We have had several cases, nearly of the same nature, where the shower-bath in its results proved invaluable."

It is clear, therefore, from this statement, that it is to cases of dementia that it is applicable. I am not aware whether the douche is much employed in this country; in the houses where I have inquired, I believe not. Pinel speaks of it in high terms; and the power of graduation, so as not to give too violent a shock, allows of its employment in private.

The patient is put into the shower-bath, and I give the detail of the process in the words of the author himself, M. Pinel:—

"A happy combination of the douche with the bath adds greatly to its efficacy, and prevents even the smallest inconvenience which might arise from it. To each bath, and directly over the head of the maniac, is adapted a tube, so constructed as to let water fall three feet in height; the stream of water being proportioned to the end proposed, and graduated according to the symptoms, but in general the stream is very small. It is not until nearly the end of the bath, and during a few minutes, that the douche is administered; when the circulation of the blood has been drawn to the surface of the body, and is to be diminished towards the head by the cold produced. The douche is omitted as the disease begins to decline and during the convalescence, but is resumed at the approach of an accession of mania, or when it has already broken forth. If the appearance of excitement be very moderate, the practice is confined to letting, drop by drop, cold water fall upon the head, which determines a moderate degree of cold, both by the impression of the liquid and the evaporation which takes place."

The cold plunging bath, formerly so much recommended by Van Helmont, and in which the patient was half drowned, in the hope that this short interval between life and death would put an end to the chain of perverted ideas, and that, with new life, the patient would recover new senses, is, as it would ap-

pear, not now employed, at least in this country.

As the use of cold in various forms appears to be productive, under proper management, of the greatest advantage in cases of mania, a corresponding good effect has been said often to be observed in the employment of the warm bath in cases of melancholia. I was informed at Charenton, in France, and the Quakers' retreat, near York, that more marked advantage was derived from the employment of the warm bath in cases of melancholy than from any known remedy in diseases of the mind; and Mr. Tuke states this opinion before the Committee of the House of Commons:—"The warm bath (he says) is used more medicinally than the cold bath; and it has been observed that the warm bath has been found very beneficial, particularly in female cases." Dr. Guislain gives his very important testimony to the benefit of it, especially in cases where lunatics refuse nourishment. Like all other remedies, it is probably often of benefit, and occasionally fails. It appears likely to be more frequently advantageous when employed among foreigners, because from infancy warm baths are used, both as remedies and luxuries, and in either view may become more necessary than to persons who have never been in the habit of employing them.

Producing discharges from the scalp by means of blistering substances or escharotics, have been strongly recommended in chronic cases, both of mania and melancholy; and one of the greatest names which ever adorned the profession of physic has been brought forward in approbation of this practice. Dr. Jenner believed that he had cured cases of insanity by producing pustules on the scalp. A case was related to me, on good authority, where mania after continued fever was cured by this means twice repeated, the symptoms of mania entirely disappearing with the second crop of pustules. I have, of course, endeavoured to inquire whether such beneficial effects are at all frequent. "We have given this a trial, (say Messrs. Beverley and Philips) more particularly the tartar emetic ointment, and with, we thought, some advantage, in a few cases, but we found, from the constant restlessness of the patient, erysipelas came on from the friction against the pillow, so that we were obliged to abandon it. We afterwards tried it in several cases over the biceps humeri muscle with, I think, more benefit than on the head: these were cases of a very violent nature, stout robust habits, and who were, for the most part, subject to periodical attacks. The use of it had evidently the effect of making the paroxysms of much shorter duration. We have also used blisters to the inside of the thighs, and calves of the legs, with advantage." The employment of tartar emetic

internally may be directed on two principles, either to diminish the action of the heart and arteries, where these are excited by keeping the patient in a constant state of nausea; secondly, as an emetic in melancholy, where full and repeated vomiting is of great service in producing a more rapid circulation in the vessels of the abdomen, and relieving the viscera, already gorged with blood. The former application of the remedy appears most applicable to mania; the second to monomania, or melancholy. "Tartar emetic, in doses of one or two grains, given internally every hour, is worth noticing in patients who are subject to violent paroxysms, particularly those who have increased vascular excitement, with great restlessness. The patient generally complains of nausea without vomiting, becomes languid and quiet, rests better, appetite improves, and in a few days is trusted out of confinement."

In addition to these means of diminishing sensibility and irritability, others have been sought for from the class of sedative medicine, and of course from the use of the greatest blessing ever accorded to mankind—opium. The opinions with regard to the use of opium in mania are very various, but may, I think, easily be explained. Where vascular excitement exists, together with increased sensibility of the brain, the restlessness is increased by the administration of opium; where perverted perception arises from disorder of the functions of the abdominal viscera, the constipating effect of this medicine, and the manner in which it acts to diminish all the secretions, obviously renders it hurtful; and it must be added, that in some constitutions it is poison. Hence it is far from generally useful even in the cases in which it is indicated; and as it is often the case that physicians neglect medicines (after a time) which do not entirely fulfil their expectations, so occasionally this medicine has been unduly estimated, and immediately afterwards, as improperly neglected. In some instances mistaken pathology has lent its aid to undervalue the medicine, and one is forced occasionally to listen to theoretic doubts about its efficacy in cases of great and urgent irritability from the prevalent opinion that all disease arises from inflammation, acute or subacute, and the corresponding error that opium necessarily occasions congestion in the brain. There is no question that where opium is not contra-indicated from peculiarity in the individual, or the presence of another disease to which it is ill-adapted, it is of all others the remedy to diminish the sensibility and irritability of the system, to make the wretched forget his grief, the ruined his poverty, and even the criminal the mental retribution of his wickedness: and in the same way it often removes from the imagination of the

maniac his supposed iniquities. Still partly the real, and partly the theoretical objections to this remedy, have caused physicians to seek for some substitute from the class of sedatives which would exhaust that increased sensibility which magnified a hundred fold the objects presented to it.

It is now some years since the French chemists announced to the world that opium contained two substances on which its peculiar properties depended. Narcotine, believed to possess the stimulant, and an alkaline substance, united with a peculiar acid, existing in opium under the name of meconate of Morphia, the alkaline body, morphia, being believed to possess all the soothing effects of opium without its stimulant properties. The morphia was found to unite in preference with the acetic acid, and under this form of acetate of morphia to be more soluble in water. It is remarkable that the combination of opium with vinegar has been particularly approved of by the celebrated Van Swieten* for the cure of this disease, and that vinegar, for what reason I cannot discover, has been considered very efficacious in the treatment of mania. Since the discovery of the French chemists, morphia has been much employed; and if I may believe my own observation, and that of persons of very extensive experience in maniacal diseases, with most complete success. I have repeatedly administered it in disease where opium was ineffectual from the headache and vomiting which accompanied its use, with the most decided benefit. I subjoin the following case, which recently occurred, and which proves the benefit of this medicine:—

A gentleman was attacked, some weeks after a most severe cough and cold, with great difficulty of breathing, attended with the impossibility of lying down at night, together with intermitting pulse and swellings of the legs, the heart and great vessels having become enlarged and dilated after the inflammation. The principal inconvenience in this case arose from the want of sleep, the patient never having been able to lie down during twenty-two nights. Opium, of course, was resorted to, and in very large doses, but the distress produced was so great that the patient preferred his sleepless nights to the dreams and scanty rest produced by this means. The patient came to London, and I recommended him the acetate of morphia, in the dose of three-quarters of a grain. He slept well, and has repeated the medicine for several weeks without its having once failed of the most complete success.

But it is to the subject of mania that this

applies, and I have it in my power to lay before the College the most convincing proof of the efficacy of this medicine, an efficacy which in my mind renders it invaluable, and which I should not venture to praise so strongly had it not been corroborated by the extensive experience of the gentleman alluded to.

“We have found the acetate of morphia useful, both in the excited and the low form of insanity. We have also found it useful in cases of fixed delusions, but not of any great standing, and more useful in the low than the excited form of the disease. Of five cases of melancholy, three got well; the remaining two are certainly improving under the use of this medicine. Of five cases of excitement, two were discharged cured; one remains much improved; two received no benefit. It is necessary to observe that we have used this medicine in several cases without taking notes, and the result was similar to the two cases mentioned, that is, without benefit. It appeared to us that morphia did not produce the same good effect in excited as in other cases, unless there was an occasional interval of reason. In the cases mentioned we have commenced with a fourth, and have not found it necessary to exceed half a grain. At present we have a patient taking half a grain dose every night with decided advantage, and we think the case very interesting, and proving the extraordinary effect of this medicine in cases of melancholy. A woman, of the age of 36, the mother of four children, was attacked with depression of spirits while pregnant of her last child. She did not feel the attack before she quickened, but immediately after she had a strong desire to destroy herself and children. This continued during pregnancy. After she was delivered she became worse, and attempted to commit suicide several times, and described her feelings, which is not common in such cases. She continued in this state, not fit to be trusted without a strict watch. She was sent here about two years ago; and what is extraordinary in her case is, that about noon all the feelings of desire of self-destruction left her. This occurred within the last three months, from which time they have remained the whole of the day. Various means were tried without effect. Our first idea, from the regularity of the attack was, to treat her as an intermittent, which failed. About a fortnight ago we gave her the morphia, beginning with a fourth of a grain, and gradually increasing it to half a grain; after taking the second dose, one-third of a grain. She slept all night; in the morning was cheerful, without feeling the propensity to destroy herself. The third day she had a return, which lasted until noon; the dose was then increased to half a grain. The fourth morning she had not any return, and continued well until

* Van Swieten says, that an accidental case first brought into notice the efficacy of this combination. A girl, who was maniacal, swallowed by mistake a scruple of opium mixed with vinegar: this mistake produced her cure.

the fifth day after the half-grain dose was given, when she had a return from five o'clock in the morning until nine, a paroxysm three hours shorter than any of the preceding. She is now free from any feeling to destroy herself.

"The following is a case of the excited kind, in which this remedy was employed with advantage.

"A. R. æt. 36, was admitted in February 1831, in a very high state of nervous excitement; she was a widow, and mother of four children. When admitted she was much excited, and constantly talking. Tongue dry; pulse very quick; skin moist. She was excited to such a degree that she tore the jacket and clothes to ribbons; refused her food; and would swallow nothing without force. She was ordered a pint of porter daily, with beef-tea and arrow-root. This diet was considered necessary, because if so much excitement continued without support she would fall into a state of collapse, and die. All our efforts being unavailing in giving her food, we determined to try the morphia. The first night it had not the least effect; she was noisy, screaming until morning; on the following day refused her food, and the excitement was unabated; we got the porter and arrow-root swallowed with some difficulty; the morphia was increased to half a grain; did not make any noise during the night, and appeared to be drowsy in the morning; but when she was spoken to answered in a very incoherent way, and the excitement continued; the porter and beef-tea was given with less trouble; the medicine repeated; slept well during the night; appeared, on questioning her in the morning, to have a slight return of reason, such as to inquire where she was; took her food better; tongue moist; pulse not so quick, and bowels open. Ordered two pints of porter, beef-tea, and arrow-root, as usual. Medicine repeated at night; slept very well; more rational; began to cry; took her food much better; drank the porter, and appeared to relish it; the medicine was repeated every night until 6th of March, when she appeared perfectly well; the morphia was discontinued; she employed herself, and was discharged 14th April cured."

The extract of the *hyoscyamus niger*, given in full doses, enjoys likewise on the continent of Europe a great reputation: it is the sedative which is preferred, and the fact of its being a mild purgative, has caused its employment by many practitioners in this country. It is believed to be less stimulating than opium; perhaps it is so; I am sure it is far less effectual as a sedative. The only English physician of great reputation who has spoken of its efficacy in diminishing the excitement of the brain in mania is Dr. Fothergill, who was of opinion he had cured puerperal insanity by this medicine; the

dose was five grains, three or four times daily.

The most powerful of all the class of vegetable sedatives is undoubtedly the belladonna. It is not to be wondered at, therefore, that it has been recommended for diminishing the sensibility and irritability of the brain in mania: it is not an opiate, but diminishes pain. This is the best explanation I can give of its effect. I have used it extensively in St. George's Hospital; and, as far as my practice would permit, in private life, and often with the most marked good effect. The following is one of the cases in which its alleviating powers were most marked:—A general officer was attacked, about fourteen years ago, with the painful affection of the second branch of the fifth pair of nerves—*tic douloureux*. He underwent various remedies from the recommendation of the most celebrated physicians in London. The disease yielded to very large doses of belladonna, and it never reappeared for ten years. At the expiration of this time he had an apoplectic seizure, which left him paralytic of the right side, very imperfect condition of speech, and frequent convulsive and painful affections of the nerves, sometimes choking, and other times startings of the nerves of the affected limb, and occasionally the return of the *tic douloureux*. It is impossible to conceive more acute pain than is experienced by the patient at these times. He always (and I have witnessed at least ten such paroxysms) finds relief in the extract of belladonna. Half a grain is administered every four hours; and always by the time the third dose has had time to operate, the pains and spasmodic twitches disappear. It might be supposed that the pain terminated of itself, or that the spasmodic affection ceased naturally at this time; but distressed at the weak state in which the belladonna leaves him, the patient has once or twice refused to take it, and he has suffered intense pain much longer than he otherwise would have done, being obliged to resort to the remedy at last.

Gréding speaks highly of this medicine in epilepsy; and in one or two cases in the hospital it has appeared to have rendered the intervals much longer, but I cannot lay claim to a single cure. It appears to me well worth the trial of physicians in mania, especially that which arises from moral causes, and is attended with pain and increased sensibility of the brain.

Various other substances from the class of sedatives have been adopted and relinquished alternately. The most powerful sedative with which I am acquainted—hydrocyanic acid, has not, I believe, been used in this country. The unhappy consequences which followed its exhibition in Paris would deter physicians from any but the most cautious experiments with this medicine. It is, how-

ever, in my opinion, from this class of medicines that the greatest improvement will be derived in the treatment of disease of the mind. It requires that practical physicians should turn their attention to it; first discarding the idea which is hostile to all improvement—either that mania is incurable, or that it always, or even most frequently, depends on organic disease of the brain.

There is another powerful medicine whose efficacy cannot be explained in our present state of science—arsenic. It appears to alter the sensibility and irritability of the brain, and has long been known as very efficacious in intermittent diseases. I have used it several times, with great benefit, in cases of chronic pain in the head, trembling and sleeplessness, nearly allied to mania. A woman, *æt.* 40, (servant to a lady in the Regent's Park,) was attacked with constant pain in the head, confusion, and giddiness, attended at times with loud noises, which she described like breaking stones; at other times images presented themselves to her mind, and her nights were entirely sleepless; pulse quick and weak. This condition continued for months, during which time I employed every means in my power for her relief; at length I resorted to the employment of arsenic. The patient took ten minims of the liquor arsenicalis twice daily, subsequently increased to the same quantity three times in the day. In about a week very great improvement had taken place; the pains had diminished, and the nights were tranquil. In a fortnight her ailments entirely left her.

The next was the case of John Graham, *æt.* 30, admitted into St. George's Hospital, under my care, November 24th, 1830. The report on admission was as follows:—Ill since June last with pain in the forehead, diverging to each side, very excruciating; and reported to feel as if the head was forced or burst open. The pains are worse at night, so as entirely to prevent sleep. The pupils are dilated and the conjunctivæ injected with blood; sense of smell increased in acuteness; bowels constipated; pulse 100, very weak; skin cold.

The bowels were ordered to be kept open, and sedative and antispasmodic medicines were employed without effect; on the 6th of December he was ordered five minims of liquor arsenicalis twice in the day.

11th.—The pains were reported greatly relieved.

13th.—The pupils contracted naturally; the pain and pressure in the head nearly gone, except when he lies down.

16th.—The medicine increased to five minims thrice daily: from this time the pains entirely disappeared. He remained in the house a fortnight, to see if any relapse took place, when he was dismissed free from ailment.

It has occurred to me to see several

similar cases; but these, perhaps, are as good examples as I could select, of this peculiar effect in the medicine; and I cannot but think it might, in proper hands, be very beneficially employed in some maniacal cases. Although I am quite aware that this medicine is similarly employed by many physicians in practice, yet I cannot learn that it has hitherto been tried in any of the numerous similar, and more severe cases, which are found in lunatic asylums.

In female cases, and especially those kinds which approach nearest to hysteria, the fætid gums and antispasmodic medicines have been principally used, and there can be no doubt that such means may tend very materially, by proper administration, towards a cure; but of all the diffusible stimulants which have been recommended, camphor has had a peculiar reputation. This was the remedy on which Dr. Perfect relied in the treatment of numerous cases of insanity. In 103 cases he conceives the patient derived essential benefit from it, and he administered it in the solid form. The following is one of these cases:—

Mrs. S. B., a married woman, became melancholic. Her complexion was pale, eyes red, tongue dry, pulse small, hard, and irregular. After bleeding and vomiting, Dr. Perfect ordered two scruples of camphor to be taken morning and evening. An eruption came out after this treatment all over the body. The catamenia returned; nitre was added to the camphor, and the patient recovered completely.

Purgative medicines have been from all antiquity greatly praised in the treatment, and above all the helleborus niger. In our present state of knowledge there are many purgative medicines far preferable to this, although it is consecrated by centuries of experience. Moderate, and frequently-repeated purging appear, as far as I can collect, to be very useful in monomania. One of the most advantageous discoveries in the modern art of medicine, has been the oil of croton, because there is scarcely any condition in which it cannot be administered, and very large doses of other medicines are required to fulfil the indication. I have repeatedly had occasion to administer this medicine in diseases of the brain, after blood-letting, in acute cases and in chronic cases, alternately with sedatives, and with the greatest possible good effect. Dr. Abercromby affords likewise his powerful testimony in favour of the derivative, as well as purgative, effects of this medicine. A drop may advantageously be administered in a little jelly, which covers the acrid taste; when made into pills it appears to lose partially its beneficial effects. Messrs. Beverley and Phillips have afforded me the following information on the curative treatment by purgatives:—“Purgatives are, we think, of the

greatest importance. We have not tried long courses of drastic purgatives; we have tried courses of mild purgatives in melancholy with advantage. There appears almost always a want of energy in the constitution of melancholic patients; the circulation is languid; the absorbent system sluggish; there is a furred tongue and swelling of the legs,—in those cases we have found a course of laxative medicine, with gentle exercise, of great service. The exercise is generally enforced, as they will not comply without it, frequently requiring two assistants to take them by the arms, and oblige them to walk. The patient's health by this means improves, and the mind by other medicine, particularly the morphia, which we have given at this period with advantage."

The *oleum terebinthinæ*, a medicine combining purgative, anti-spasmodic, and stimulant properties, has been much employed in cases of hysteria, connected with epilepsy; it has been recommended by Dr. Latham, senior, Dr. Thomas Young, and Dr. Percival; and in the latter cases especially, by Dr. Prichard, of Bristol. Nevertheless, it has never become an ordinary practice. I lately had the opportunity of seeing very decided effects from it, in a case—an out-patient of St. George's Hospital, æt. 15. The patient had never menstruated, and was in a state very nearly approaching to idiocy, being unable to speak or take notice, except occasionally when struck with the appearance of some glittering ornament, when she expressed her surprise by remarkable and violent gestures. The pulse was quick and weak; great pain in the head; skin cold. She was ordered two drachms of *ol. terebinth.* and the same quantity of castor oil every morning, and in about a fortnight she recovered the use of her senses; but she remains in a weakly state of body, and the catamenia have not occurred. She is now able to take steel. I am inclined to think that great benefit may be derived from the employment of this medicine in similar cases, and such are not very uncommon.

The belief that cases of insanity always depend on organic causes, or a state of acute or subacute inflammation of the brain, has not only led to the erroneous treatment of many of these forms of disease, by venesection, but has suggested an antiphlogistic diet. We have, I think, the best evidence from the greatest experience in France and England, of the evil consequent on such practice. M. Pinel draws a terrible picture of the afflicting consequences of the short allowance on which the patients of the Bicêtre were placed in 1796; and it has been seen that a full diet, and even considerable quantities of stimulants, are often productive of the utmost advantage in what are termed high cases. In the cases arising

from child-bed, or during nursing, Dr. Gooch has very clearly demonstrated that these occur either in consequence of, or during, an exhausting process, and are relieved by tonic and antispasmodic medicines and restorative diet.

I have now spoken cursorily of the principal means of cure, from medicine, which may be adapted to mental disease; any improvement in treatment, and consequently any collection of facts, to serve as guides to future observers, must obviously depend on the judgment exercised in discriminating the causes, both moral and physical. The small bleedings, for instance, which would be useful in mania depending on chronic disease of the heart, and steatomatous or bony depositions in the smaller arteries of the brain, would be a fatal error in those frequent cases connected with hysteria; and the large evacuations and antiphlogistic treatment imperatively called for in inflammation of the membranes of the brain, would change mania into furious madness, by increasing the already excited morbid sensibility of the brain and nervous system. I hope, little as I have been able to say upon these subjects, that it is enough to prove that it is to the educated physician, to the man who is engaged in the constant discharge of the duties of his profession, that such cases should be made a subject of deep importance; that all his experience should be brought to bear; that all his faculties and observation should be concentrated in improving this portion of the medical art; not by studying it exclusively, but in conjunction with the other diseases of the human body, and thus removing a great source of quackery and imposture. The educated physician is too often called upon to stand between the public and its prejudices: this must be done, not by deserting our colours, but by shewing that, with every desire to serve the public, neither rewards nor honours, nor fleeting popularity, can make us lend our countenance even to innocent imposture; and that that independence which is the distinguishing mark of an honourable profession, will support us through difficulties, and vindicate the integrity with which we practise that profession. If we carefully investigate disease, and neither resign that interesting and useful study to artful and designing persons, nor suffer ourselves to be overcome by the momentary prejudices of the world; all experience has shewn that medical practitioners so acting have received, and will continue to receive, the respect and homage of society.

CASE OF EPHIDROSIS IDIOPATHICA.

To the Editor of the London Medical Gazette.

SIR,

I know not whether the opinion is founded in correct observation, but I am disposed to believe that there is something very peculiar in the constitution of the present year—every variety of disease appears to present unusual phenomena. Fevers, if inflammatory, quickly assume the typhoid character; or from their commencement give strong evidence of their malignant tendency. The active stage of inflammatory diseases appears to pass as a shadow, and the disturbance which a mere attack of diarrhoea will produce in the vascular and nervous systems but ill accords with the usual and observed effects of that disease. How far these circumstances may be associated with the pestilential state of atmosphere, which at present is depopulating many parts of Europe, it may be difficult to decide, and it remains a point of no trifling moment for our consideration whether these phenomena are to be considered as precursors of future and more fatal mischief, or are to be regarded as modifications of the same calamity. With such impressions, it is to be supposed that every unusual occurrence will be adduced in support of the belief; nor can I think that the following case, which I offer for insertion in your journal, in any way contradicts the opinion.

A lady, whose mind and body had been harassed by unremitted attendance on a sick and valued relative until his death, through the day subsequent to that event was troubled with repeated attacks of cramp in the legs, and with general feelings of indisposition. Soon after going to bed she was seized with rigor. No perceptible hot stage followed this state of collapse; but a most profuse perspiration within the space of half an hour covered the whole body; her back and limbs became painful, with pulsating pains in the head. I saw her for the first time (later than I intended) the evening subsequent to the attack. The pulse was ranging at 130; the countenance very anxious; the perspiration literally pouring

from the face, the skin of which was exquisitely tender (and still remains so) from the constant use of the handkerchief; and it had been necessary to keep a napkin under the chin to receive the perspiration as it ran down the face; every part of her body was in the same state; she had had no sleep: there was great sense of weakness in the abdomen, with unusual and most distressing flatulency. I ordered the following medicines, directing that she should be kept moderately cool, and no warm fluids to be given.

R Confect. Arom. ʒss.

Tr. Hyos. ʒss.

Sp. Ether, Nit. ʒss.

Tr. Card. c. ʒj.

Aq. Puræ, ʒx. M. fiat haust. stat. sumend et post hor. quatuor repet.

Early the next morning I found her with the pulse reduced to 100 beats in the minute; the perspiration had considerably decreased since four o'clock in the morning, though up to which time it had continued with unabated violence; head still painful; great uneasiness in the back; the flatulent state of abdomen greatly relieved.

R Extract Conii, gr. xvj.

Tr. Cardam. comp. ʒss.

Inf. Rosæ, ʒiij.

Aq. Cinn. ʒiiss. M. fiat. mist. cujus. capt. 4tam part. 4tis horis.

In the evening I found her with the skin in a natural state; pulse 80; no pain in the head or back, but excessively weak. I continued the sedative, and ordered a mild aperient for the morning. On my next visit I found she had passed a comfortable night; skin in a natural state; pulse regular; I continued the sedative every six hours. Two days subsequent to this visit she complained of nothing but debility.

I remain, sir,

Your obedient servant,

HENRY GEORGE.

22, Lower Phillimore-Place,
Kensington, Oct. 21st.

CHOLERA TREATED WITH CAJEPUT OIL.

To the Editor of the London Medical Gazette.

SIR,

If you consider the following case of English cholera, which was clearly at-

tributable to improper ingesta, and in which the oil of cajeput was administered externally and internally, with apparent benefit, sufficiently interesting for insertion in your journal, it is at your service.

I remain, yours, &c.

R. R. ROBINSON,
Surgeon.

13, Cooper's Row, Trinity Square,
October 24, 1831.

N. Brennan, æt. 60, a thin and feeble man, of bad constitution, and rather intemperate habits, who had been sometimes subject to mucous diarrhœa, was purged more than usual on the 13th September, notwithstanding which he eat pork sausages for dinner, and drank porter at 2 p. m. and at 11 p. m. took a pint of ale, and went to bed, as he thought well, about 12. He awoke, however, about two the following morning, with violent cramps in calves of both legs, vomited frequently, and was purged three times. The pain increasing, I was sent for between five and six: he was then suffering severely from cramps in legs; he was very languid, his countenance anxious; there was no pain in abdomen, where he could bear to be pressed; skin warm and perspiring, tongue coated, great thirst, pulse soft and regular, between 80 and 90.

Pulv. Rhei gr. xxv. Pulv. Cinnam. co. gr. v. statim. Applicentur frictions crucibus.

12, noon. — Contrary to orders, he had taken port wine and water; had been constantly sick; the fluid vomited appeared to consist chiefly of mucous tinged by the wine; cramps have been frequent and severe, but have always been diminished in duration by friction.

Cal. gr. iij. Opii gr. j. statim.

6, p. m. — Constantly sick; no motion; cramps frequent and severe.

Ol. Cajeput. gtts. xxx. Tinct. Opii gtts. xv. statim ss. The same to be used as an embrocation.

10, p. m. — The cajeput immediately returned, and he has been constantly sick since; has vomited altogether since last report about $\frac{3}{4}$ of clay-coloured fluid, strongly impregnated with the oil; cramps, which before invariably followed each attempt to vomit, have occurred only five times since last report, and have not been so severe; thirst

great, tongue much furred, skin warm and perspiring, pulse soft, 90; no motion.

Pulv. Opii gr. j. — Repetatur Linim.

Sept. 15th. — Slept well, but little vomiting; cramps occasionally on moving, not otherwise; less thirst; urine scanty, but high coloured; tongue coated; pulse soft, regular, 84.

Ol. Ricini $\frac{3}{4}$ ss. statim.

16th. — Cramps gone; sick only once; two motions; tongue still coated; some thirst.

Cal. gr. j. Ext. Colocynth co. gr. v. o. n.
Ol. Ricin. $\frac{3}{4}$ ss. p. mane.

18th. — Up, and convalescent; sickness and cramps gone; tongue clean; bowels open.

Inf. Gent. co. $\frac{3}{4}$ ss. Mag. Sulph. $\frac{3}{4}$ ss. bis die.

20th. — Recovered his strength rapidly, and has had no return.

REMARKS. — The spasms appeared in this case to be chiefly relieved by the outward application of cajeput and laudanum as an embrocation, which I am not aware has been hitherto used in cholera.

OPIUM IN CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

It appears to me to be generally the practice to give opium in a very wrong form in cases of cholera; and as the public is at the present time under so great an apprehension of this dreadful disease coming to this country, I think it my duty to ask, whether it would not be much better to give it in its hard purified state, or to administer the acetate of morphia, for it is well known that light, according to the experiments of Vogel, has the power of decomposing the tincture, and I make no doubt that it is the common practice for the druggists to adulterate the powder. Should you think the above observations worthy of notice in your valuable Gazette, you will oblige me by inserting them.

I remain,

Your obedient humble servant,
JOHN GREENING, Surgeon.

Worcester, Bridge-Street,
Oct. 24th, 1831.

YELLOW GUM, OR GRASS TREE, OF
NEW SOUTH WALES (XANTHOR-
RHŒA.)

ANALYSES & NOTICES OF BOOKS.

THE Xanthorrhœa, or yellow gum tree, is found abundantly in New South Wales, where, among the colonists, it is named the grass tree, from its long, pendant, grassy foliage. It is a tree of curious growth, and I have seen some of the species of from eight to ten feet in height. From the centre of the foliage rises the straight flowering stem, which is frequently ten feet and upwards in length; it is round and very light, combining lightness and strength; it is used by the natives for making their spears, fish-gigs, &c. The scape terminates in a cylindrical spike; the flowers are white, small, and numerous; an albumen is secreted from them, which is greedily devoured by the natives, and numerous birds; it is said to be very agreeable to the taste. The capsules are triangular, and contain three flat, black seeds, in distinct cells.

It derives its name, "Yellow gum tree," from secreting a gum-resin of a bright yellow colour, and resembling in appearance the resin of the Stalagnitis Cambogioides, or gamboge tree. Externally the yellow gum has a dull appearance, but breaks with a shining bright yellow fracture, and is sometimes streaked internally with red. The resin is soluble in spirits of wine, but is insoluble in water; it is light and brittle, and is, in its natural state, without any fragrant smell; but by the action of fire it yields an aromatic odour, resembling the frankincense, for which it might be used as a substitute. It exudes spontaneously from the trunk, but only in very small globules; yet it may be melted into large masses. At the base of the tree are congeries of scales, between which the gum is found conereted in small pieces. One tree yields a considerable quantity of the gum.

This resin (more commonly known as the gum acaroides) is slightly bitter, pungent, and astringent, and has been used in dyspeptic, dysenteric, and other cases; but not, I believe, with such success as to cause it to be admitted into our materia medica.—*Mr. G. Bennett's MS. Journal.*

London, Sep 1. 27, 1831.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Essays on the Effects of Iodine in Scrofulous Diseases; including an Inquiry into the Mode of preparing Ioduretted Baths. Translated from the French of M. LUGOL, Physician to the Hôpital St. Louis, by W. B. O'SHAUGHNESSY, M.D. With an Appendix by the Translator.

THIS volume consists of a translation of three Essays published by M. Lugol, physician to the Hôpital St. Louis, of Paris; followed by an Appendix by Dr. O'Shaughnessy, containing some important additional information, which we shall lay before our readers in the sequel. Of the opinion entertained in France of these papers, some idea may be gathered from the circumstance of their having received the warm eulogies of the reporters appointed by the Académie Royale, as well as the unanimous approbation of the medical press—to say nothing of six thousand francs voted to the author by the Institute. The intelligent translator of M. Lugol's Essays attributes much of the success attending the use of iodine at the St. Louis, to the care, skill, and patience of the physician—to the period of treating the cases being unfettered by the limits as to time, which prevent investigations of this nature from being satisfactorily conducted in the hospitals of this country. The cases, indeed, are numerous and astonishing—so much so, that farther trials will be required before practitioners can become convinced that similar success, in scrofula especially, is to be hoped for among us.

M. Lugol seems to be a person of eccentric manners, and an enthusiast, affecting in his discourses the most preposterous forms of expression. Thus we are informed that he will sometimes personify his favourite medicine, and exclaim, "*O iode, iode, O mon fils! pourra-t-il se rencontrer jamais une langue capable de célébrer dignement tes vertus anti-scrofulenses!*" And again, on pointing to a patient in whom the iodine had, in his estimation, gained a great triumph over the disease, "*Voici la bataille d'Austerlitz de l'iode!*"—

Our Gallic neighbours may, indeed, say of this high-flown style—

Ce style figuré dont on fait vanité,
Sort du bon caractère et de la vérité—

but to us it seems wretched bathos and downright nonsense. A knowledge of these circumstances leads us to trust more to the report of the commissioners than to the statements of M. Lugol, who, without intending to deceive, may yet mislead. Now the report is really very favourable, and the profession is indebted to Dr. O'Shaughnessy for having given, in so convenient a form, all that is valuable in the observations of the French practitioner.

The preparation of iodine which M. Lugol regarded until lately as most certain, was a solution in distilled water; observing, however, that the vehicle was never carried in quantity much beyond what was necessary in order to obtain a perfect solution; the dose being half a grain to begin with, gradually increased to two-thirds of a grain, and finally a grain, in twenty-four hours. This, it will be perceived, is greatly under the maximum doses of M. Coindet, who gave to the extent of three grains a-day. Indeed our author particularly objects to the tincture so much used by the Swiss physician, which, being administered in an aqueous vehicle, leads to the precipitation of the iodine in the solid form, and thus incurs great risk of serious injury to the stomach. A pound of water was the quantity employed by M. Lugol in making his solutions, and to these he generally added a few (twelve) grains of common salt, or, if we must use the more scientific language of our author, of chloruret of sodium. For external use he employed an ointment, having three degrees of strength: No. 1, containing, in two pounds of fresh lard, hydriodate of potass, $\mathfrak{Z}\text{iv}$. and iodine, $\mathfrak{Z}\text{iv}$. No. 2, hydriodate of potass, $\mathfrak{Z}\text{v}$. and iodine, $\mathfrak{Z}\text{xiv}$.; and No. 3, $\mathfrak{Z}\text{v}$. and $\mathfrak{Z}\text{xvi}$. * Occasionally, as a substitute for these ointments, was used a solution—viz. No. 1, gr. ii. of iodine, gr. iv. of hydrod. of potass, to lbj . of water; No. 2, gr. iii. and gr. vi. to lbj .; and No. 3, gr. iv. with gr. viii. to lbj . But upon these modes of exhibition, though much superior to

what had been previously used, farther experience enabled M. Lugol to improve, and he has latterly abandoned the simple aqueous solution for one made by dissolving the iodine in a solution of potass, thus:—

Ioduretted Mineral Water.

“ R No. 1.—Iodine, gr. $\frac{3}{4}$; Hydriodate of potass, gr. $1\frac{1}{2}$; Distilled water, $\mathfrak{Z}\text{viii}$.

No. 2.—Iodine, gr. i.; Hyd. of potass, gr. ij .; distilled water, $\mathfrak{Z}\text{viii}$.

No. 3.—Iodine, gr. $1\frac{1}{4}$; Hyd. of potash, gr. ij . $\frac{1}{2}$; distilled water, $\mathfrak{Z}\text{viii}$.”

This preparation is quite transparent, of a fine orange colour, and keeps for a considerable time; while it is so little disagreeable that children take it readily if mixed with sugar. This addition, however, it is worthy of remark, ought only to be made when it is just about to be administered, as a decomposition results if it be allowed to stand for some hours. Two thirds of No. 1, as above, are to be taken in the course of the day, to begin—and this ought to be continued for a fortnight; after which, the whole of the mixture may be taken daily.

The external effects of iodine, when applied to ulcers, were greatly increased local action, sense of pricking and smarting, which was often prolonged. In a few days followed an improvement in the aspect of the sores, with or without suppuration. The local action generally lessens in activity as the surfaces heal, but in rare cases it continues, however long it may be used, to produce undiminished excitation.

The internal effects of iodine vary: one of the best, though not one of the most common, is that of increasing the appetite to a great degree. A more general consequence of its use is an increase in the urinary secretion: sometimes the diuresis is very considerable indeed. More than one person in three is purged by it, and in some this is attended with colic. In several cases it produced salivation. Some patients, particularly females, complained of it producing pain of the stomach; and when this happened, the inconvenience was removed by administering two or three ounces of the kina wine after the mineral water.

Such are the principal forms of exhibiting iodine, as adopted by M. Lugol;

* No. 1, made up in smaller quantity, and with rather less iodine, may be prescribed thus:

Iodine, gr. xiii.; Hydriod. of Potass, iv . scruples; fresh lard, ii . ounces. M.

and we shall conclude our notice of his part of the volume by quoting a portion of the Report upon the subject, by MM. Serres, Magendie, and Dumeril.

"The author of the Memoir has carefully noticed the effects produced by the iodine on the animal economy. Applied externally, its local action has always been very sensible: it determines on the surfaces of the ulcers a feeling of smarting, accompanied with painful itchings. *This application to the diseased surfaces changes their appearance, and frequently produces as appreciable an effect as that determined by mercury on venereal ulcers.* Moreover, the mode of its action does not appear to be invariably the same: sometimes the iodine seems to melt down and resolve the tubercles, sometimes, on the contrary, it urges them on to rapid suppuration. At other times the painful sensation appears to diminish in proportion to the healing of the surfaces, an effect which is perhaps dependent on habit; nevertheless, some ulcers remain sensible while the curative process is not at all established.

"Internally administered, and always in small doses, and with the most prudent slowness, the ioduretted water constantly excites the appetite, and appears to increase the urinary and salivary secretions. Sometimes, but very rarely, it has become purgative to so considerable an extent that its use was necessarily suspended, at different intervals, from two to three days each. In other and still rarer cases, in which the solution of iodine appeared to occasion pain in the stomach, the wine of quinquina, given according to the directions of M. Coindet, in a dose of two or three ounces, put an end to the troublesome symptoms. M. Lugol, however, always declined, as much as possible, this association of remedies, in order to avoid complexity in the results of his treatment.

"Iodine, administered in this diluted form, has never caused emaciation nor produced the expectoration of blood or other accidents, which many have imputed to its action.

"From the contents of the first Memoir, it appears that M. Lugol has treated with iodine alone, in seventeen months, at the Hôpital St. Louis, 109 scrofulous patients, of which sixty-one were males and forty-eight females.

"That at the close of last year, thirty-

nine (twenty-nine males, ten females) were still under treatment.

"That thirty (seventeen males, thirteen females) had quitted the hospital with marked improvement.

"That in four cases (two males, two females) the treatment was ineffectual.

"Finally, that thirty-six (thirteen males, twenty-three females) were discharged completely cured.

"The author concludes, from all the facts he has collected, and the researches he has conducted, that iodine deserves to be considered as the most efficacious remedy in scrofulous diseases, since it has constantly arrested their progress, or at least exercised a salutary action in the treatment of all tubercular tumors, even when it has not evidently accomplished their cure. He therefore believes that the introduction of this remedy into medicine is one of the most valuable acquisitions the healing art has made in modern times.

"We shall then confine ourselves to say, that after having made ourselves acquainted with the facts cited in the memoir, we have been enabled to confirm the evident action of the remedy; and that we believe M. Lugol to have effected a work of great utility by availing himself of the facilities afforded by his situation, in seeking for a remedy for a disease hitherto so deplorable and desperate. We consequently propose to the Academy to encourage this physician to persevere in the researches which he has hitherto pursued with so much zeal and sagacity."

An appendix by the translator contains some farther evidence in favour of iodine, from the practice of various other physicians and surgeons in France; but the part of it which appears to us most valuable is the author's own, namely, a clear and succinct account of the chemical properties of iodine and hydriodate of potass, with their adulterations, &c.

"*Adulterations, &c. of Iodine.* — From the high price at which iodine was sold on its first introduction into medicine, a great inducement was afforded for its adulteration, and accordingly, with a clumsiness and ignorance only equalled by their shameful cupidity, some druggists were in the habit of mixing the iodine with a liberal per-centage of charcoal, the protoxide of iron, plumbago, or the carburet of that metal.

"Although the price of the article has now fallen to 1s 6d. the ounce, with many eminent and honourable wholesale houses, still, perhaps, from the difficulty of abandoning old habits, some dealers yet practise the admixture above-mentioned. The fraud may be at once discovered by heating ten grains (accurately weighed) of the suspected sample in a glass tube by a spirit lamp, and continuing the heat till no violet vapour is evolved; if any residuum remain, it indicates an adulteration, the extent of which may be ascertained by the weight of the fixed matter.

"Perhaps an easier process is by throwing the suspected specimen into strong alcohol in sufficient quantity. All the foreign ingredients above enumerated remain undissolved.

"By either of these methods the mixture of charcoal, the protoxide of iron, or the carburet of iron, may be discovered. I should not omit to add, that I have met with one specimen much more artfully and scientifically adulterated, and in which the fraudulent ingredients were, at the same time, soluble in alcohol, and volatilised by heat. I forbear, however, alluding to this mixture more minutely, because the fraud is comparatively so difficult of detection, that describing it would, perhaps, only lead to its more general practice. I should add, that in the last edition of Gray's Supplement to the Pharmacopœias, the editor states that iodine is sold at the absurdly extravagant price of 1l. 5s. per ounce."

"Adulteration of Hydriodate of Potash.—The falsifications of this salt are so extensive and peculiar that the practitioner, who is not aware of their existence and the mode of their detec-

tion, need never attempt to place a patient under Lugol's treatment with any reasonable prospect of success. Had the adulterations been contrived for the sole and express purpose of destroying any anti-scorfulous virtues which iodine may possess, the method could not have been more ingeniously devised.

"It was long known that hydriodate of potash was apt, during its preparation, according to the method of boiling with the alkali, to be mixed with a slight excess of potash, probably from the weakness of the spirit used to dissolve away the hydriodate from the iodate. Little attention, however, was paid to the subject until Mr. Pereira published a paper in the London Medical and Physical Journal, in which he described additional adulterations of much moment. A short time before Mr. Pereira's paper appeared, I had observed the same facts in Scotland, and demonstrated them to my pupils.

"From these conjoint observations it appears that much of the hydriodate of potash at present vended by druggists, &c. contains large quantities of the muriate of soda, carbonate of potash, traces of the sulphate of soda, and other salts in minute quantities. One specimen of which I made a quantitative analysis, contained 64 per cent. of the carbonates alone; and this examination was fully confirmed by Dr. Cristison, who also investigated the subject.

"The pernicious effects thus exerted on the therapeutical applications of iodine in the internal and external treatment of scrofula, goitre, syphilis, &c. may at once be understood by consulting the subjoined comparative statement of the chemical properties of the pure and impure solutions of this salt.

PURE.

- "1. Dissolves iodine in the cold, forming an active ioduretted dydriodate of potash for internal use.
- "2. When warmed with iodine and diluted with water forms an active bath, which excites powerful local action.
- "3. Affords by double decomposition pure iodurets of lead or mercury for internal or external use.

Conclusion.

When pure, a valuable medicinal and pharmaceutical agent.

IMPURE.

- "1. Does not dissolve iodine in the cold.
- "2. When warmed with free iodine, converts it into the hydriodate of potash, a compound proved by Lugol to be nearly inert as a local or general bath.
- "3. Affords by double decomposition carbonates, chlorides, and sulphates of lead or mercury compounds either inert or opposed in their action to the iodurets of these metals.

Conclusion.

"Impure, possessed of no medicinal or pharmaceutical value whatever.

"In order to detect the presence of the carbonates and muriates of soda, or potash, the best method is to add a solution of the nitrate of silver to a solution of the suspected hydriodate; if impure, a white precipitate of the carbonate, chloride, and ioduret of silver, subsides. Filter, and when dry project the precipitate into ammonia, which dissolves the chloride and carbonate of silver, and the ioduret remains; of this ioduret of silver 220 parts correspond to 137.42 of the hydriodate of potash; the amount of the adulteration can, therefore, be readily ascertained by comparing the weight of the hydriodate of potash corresponding to the ioduret of lead obtained, with that of the weight of the specimen before the experiment.

"As a trial test to detect these adulterations, a little acetate of lead should be added to a solution of the suspected specimen; if impure, a copious *white* precipitate is formed; if pure, the deposit is a fine *yellow* colour of a crystalline texture, and perfectly soluble in hot water, from which it crystallises on cooling in hexagonal scales of a golden splendour.

"I trust I have now said nearly enough to guard the public and the medical practitioner from the mortifying effects of this shameful practice. All M. Lugol's experience has been placed in vain before the community, unless this sophistication be diligently sought for, and its authors efficiently punished*.

"As it is more than probable that the management and preparation of ioduretted baths will fall into the hands of the proprietors of large bathing concerns, it is highly desirable that the patient should possess the means of ascertaining whether the baths are properly prepared—that is, whether they contain free iodine dissolved in pure hydriodate of potash. For this purpose the bather may procure a specimen of the water, and add to it, when cold, a cold solution of starch in excess: if free iodine be present, the blue colour will be produced, and the filtered colourless liquid will yield a *yellow* precipitate with the acetate of lead. As the patient's sensations will be his principal guide to the quantity of free iodine necessary to excite the cutaneous action of the remedy,

I do not feel it necessary to describe here the mode of analysis by which the quantities of the respective ingredients in a given quantity of water may be ascertained. The analysis is, moreover, so complicated that I could scarcely hope to render it intelligible but to practised chemists, who, of course, do not require the information."

A Practical Guide to Operations on the Teeth. To which is prefixed a Historical Sketch of the Rise and Progress of Dental Surgery. By JAMES SNELL, Dentist, Member of the Royal College of Surgeons, &c. &c.

THE first fifty-five pages of this work are devoted to a history of the healing art, rather misplaced, in our humble opinion; but that is matter of taste. "It is idle (says our author) to inquire with whom, or in what country, medicine took its rise;" and we think it is idle to spend the fourth part of a thin volume, which professes to be a "practical guide to operations on the teeth," in discoursing about Confucius, Zoroaster, Hermes Trismegistus, and other, no doubt, very respectable personages, but who have heretofore been little heard of as dentists.

Those of our readers who practise in the department of surgery on which Mr. Snell's essay treats, will find some useful instructions on the mode of extracting teeth, and some strong objections to the plan lately so much recommended of excision, together with a description of some ingenious mechanical contrivances for facilitating the performance of the various operations. The only subjects of more general interest are referred to in the following short quotation on tooth-brushes and tooth-powder.

"The various opinions which are held relative to the shape and texture of brushes, would lead us to suppose that the matter was a much more important one than it really is. There are even patent tooth-brushes. A late author insists upon the necessity of hard brushes, and states that he has tried for some years to do himself all the injury he possibly could by using a hard brush, and finds that he can inflict none. I confess I am not yet acquainted with any necessity for using a hard brush, presuming upon its doing no injury:

* "When the hydriodate of potash is found to be adulterated, Lugol's aqueous solutions (see formula in Part I.) should be used in the internal treatment."

the teeth do not require it. A brush too hard is as useless, from having no elasticity, as a very soft one, from its having no firmness. There is a medium between the two, which should be chosen. A brush for the anterior part of the teeth should have its bristles cut lower in the centre than at each end, so that when looked at sideways, the bristles should form the segment of a large circle, the external row inclining outwards a little. In texture it should be as elastic as is consistent with a proper degree of firmness, where the gums are not diseased and the teeth not loose ; but when these defects exist, a softer brush should be used. Under any circumstances, however, the hairs should not be so soft as to loose their elasticity by use. The brush should be used as much as possible in a perpendicular direction, not as it regards the brush but the teeth. Brushing the gums until they bleed is recommended by a late author. I see no necessity for this rude method of phlebotomizing ; notwithstanding I believe that the moderate use of the brush upon the gums is highly beneficial.

“ As the grinding surfaces of the molar teeth are apt to retain matter in their indentations which might be injurious, a brush rather larger than that above-mentioned should be used for the purpose of cleaning them ; the bristles should be much stronger, although by no means so strong as some which are sold for this purpose, as it is totally impossible that they can act but upon the projecting points of the teeth. The object is to have the bristles of a sufficient length, strength, and elasticity, that their points may insinuate themselves in the little indentations of the grinding surface. A hard brush, with short bristles, such as are sold for this purpose, is consequently altogether unsuitable. The back part of the teeth are best kept clean with a small square brush, with bristles rather long and elastic, and left highest in the centre, the corners of the square being cut off. The handle of the brush should be bent a little below where the bristles terminate. A flat brush also, with one row of bristles, or a pointed one, the bristles being inserted like a camel hair pencil, is useful for inserting between the teeth, to remove as soon as deposited the tartar which, without care, is very apt to accumulate in that situation.

With these brushes, if well chosen as to their shape and texture, the teeth may be kept clean in every part.”

“ Bearing in mind that the use of a tooth-powder is to assist the action of the brush, in taking off any extraneous body which may adhere to the teeth, and for which purpose the brush alone is frequently found insufficient, those substances should be chosen which can be levigated finely, and which will not have a tendency to wear away the enamel. The testaceous powders, therefore, in combination with some alkaline, should be preferred. Charcoal is highly spoken of. There are, however, two strong objections to it. First, it cuts through hard substances rapidly, consequently will destroy the enamel ; and secondly, it is a very dirty application to the mouth, and others much less so may be readily found. Where, however, it is used, that which is made from the areca nut or from the vine shrub is to be preferred. This, mixed with creta, may be used without injury, where there is a *penchant* for this substance. The best tooth-powders, however, in my opinion, are composed of such ingredients as the following formula :—

“ R Pulv. Cretæ. Prep. ʒiij.
Saponis Hispanici, ʒj.
Pulv. Rad. Iris. Flor. ʒij.
Sodæ Carbon. ʒj. Misce.”

MEDICAL GAZETTE.

Saturday, October 29, 1831.

“ Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri ; polestas modo veniendi in publicum sit, dicendi periculum non recuso.”—CICERO.

CAN CHOLERA BE EXCLUDED OR CONFINED ?

EVEN before the words we now write have come before our readers, facts may have given a practical answer to our question ; but though we apprehend not so speedy a solution, our deliberate opinion is, that protection by exclusion, though within the range of possibility,

and therefore to be attempted, is no longer within the pale of probable events. The destructive malady has now traversed nearly an eighth part of the earth's surface—it has gone on heedless of man's contrivances—it has baffled every human precaution—it has come even from the banks of the Ganges, and approaches the *divisos orbe Britannos*; and now that it is nearly upon our shores, it scarcely remains for us to say, "Hitherto shalt thou come, and no further!"

We must confess we have long since lost all confidence in the continental system of quarantine and cordons—so far as they might be supposed to protect the nations of Europe against the invader. Russia tried them amply, and to its cost; and if any care was neglected, Prussia guarded cautiously against such neglect. Another powerful nation—Austria, hoping to profit by the experience of both, followed their example in an amended form. Yet what has been the result? Why, Berlin and Vienna—which are even still struggling with the monster—were after all unable to protect themselves from his fatal ingress. Nay, the cordons of Vienna nearly occasioned a civil war; and at Berlin, where they were only maintained by the rigour of strong despotism, the King of Prussia was soon convinced of his mistake, and hastened to repeal those "sanitary laws" which experience taught him were so fruitless, so expensive, so ruinous to commerce, and so tyrannical. In his proclamation (dated from Charlottenburgh, Sept. 6th, 1831) he complains that, "the Asiatic cholera had penetrated into his dominions in spite of measures the most rigorous, precautions the most active, and vigilance the most sustained, which had all proved useless and unsuccessful in averting or even checking its progress." He adds, "and now, since the disease is in our kingdom, and the eyes

of government have been opened by experience, I have ordered the rules, hitherto prevailing, to undergo a complete revision, and a conscientious examination as to all the circumstances worthy of being entertained, in order to arrive at a proper modification of those rules, partly from an acquaintance with the most approved modes of treating the disease, and partly from the exigencies of the occasion. The rigorous measures of isolation by cordons, established on the frontiers, and in the interior of the country, have hitherto acted *unfavourably* on the industrious habits of my people, and threaten, if they be maintained much longer, to destroy the comforts of numerous families, and in short to become *more ruinous to the country than the malady itself*." The Board of Health, at Berlin, was accordingly enjoined to publish forthwith such changes in the "rules" as their experience had suggested to be beneficial; while, meantime, the military cordons were almost everywhere discontinued, and such other restrictions as originated in the (so called) "sanitary laws," were considerably modified.

It is very remarkable, that at the very time the King of Prussia was taking these measures, dictated by his sad experience, and lopping off the heavy expense of cordons, the French minister was procuring a grant of a million from the Chambers to try all the same experiments over again; he was urging even then the salutary effects of *cordons sanitaires*, and insisting that sufficient precaution had not been taken wherever the disease had got admission. He took Prussia for his example. "The Russians, Poles, Hungarians, and Wallachians," said M. d'Argout, "have fallen victims to their own negligence—they have shewn the most culpable contempt for the sanitary measures prescribed by the contagionists; while

the *Prussians*, with persevering and most praiseworthy rigour, have followed up the ordinances of their government—they have spared no sacrifice—they have omitted no precaution—they have resisted the invasion of the disease—and behold their *most signal success!*” Even then, as we have said, the disease was settled in Berlin, and the king was abolishing the cordons. But the knowledge of this fact came too late, and the million was already voted.

To do the French justice, however, all their attention is not now turned to frontier regulations; they seem to be rather intent upon the best method of grappling with the foe when he gets in amongst them: and they have profited considerably by the example of their continental neighbours.

And shall not we, then, weigh well the consequences before we adopt the strict enforcement of similar regulations? Shall we not consider cautiously the certain injury to trade, and the extremely doubtful advantages to the public health? For our own parts *we* have spoken; keep it out, if our fleets on the broad seas can accomplish this; but once among us, we have little faith in any attempt to lock it up, while the whole economy of society must be destroyed in the very attempt.

But if we cannot shut out this fierce intruder, it remains to be considered what we are to do with him when amongst us. Can we hold him in durance? Can we make him our prisoner, and strangle him in his dungeon? Alas! we fear not. He has never yet been subdued in that way. He becomes more bloated and venomous by confinement; he thrives on the thick foul vapours of the lazar-house; he is the monster “that doth make the meat it feeds on.” We should beware, then, how we attempt to isolate the disease. Experience has shewn that it has never yet been shut up, that it has not come forth from its

confinement with increased severity; whilst, on the other hand, it is enfeebled and it languishes in the pure air of heaven. It will not be restrained; it cannot be extinguished; it is in progress; and we will be wise in suffering it, as far as may be, to “pass peaceably.” It will abide with us, we doubt not, in its progress; and it is to the event of that visitation that our attention should be principally turned. We repeat that we can repose no faith in any measures taken for the exclusion of cholera, except the forlorn hope afforded by our quarantine; and we have no confidence in any means of arresting it, if once introduced, except on those which have for their immediate object the abolition of every thing like a *nidus* for its reception. It is in this respect, we think, that the labours of the Board of Health will be most usefully put in requisition. They cannot announce a specific remedy for the disease, because none exists, or at least has been discovered. It has become the fashion to censure this body, for no other reason apparently than that they have not afforded to the public “satisfactory information,” when there was nothing satisfactory to give. Periodicals and journals may, without impropriety, publish every fact or suggestion as it occurs, for the public will estimate these for themselves; and we have been among the foremost to present them with the most interesting details. But the Board of Health are very differently situated; their first care must be, not to mislead. They must sift statements, to separate those which are solid; they must weigh conclusions, to ascertain their real value. To them the public anxiously look, as to an authority by which they are to be guided; and one premature recommendation might be productive of evil which could never be remedied. The public cannot be too strongly cautioned against the nostrums

with which the press is overloaded; they are almost all the products of men writing in their closets, without having seen a solitary specimen of the disease. But, again, it has been said, and with great apparent reason—why are there not among them some who have seen the disease in India? and we confess, that so strong did this objection at first appear to us, that it seemed to be unanswerable. A more extensive acquaintance, however, with the accounts given by the Indian practitioners, and in particular, the circumstance of their opinions having been, in some essential points, refuted by the history of the disease in Europe, have led us to doubt very much whether any advantage would have been derived from the assistance of these gentlemen in the Board. The doctrine that cholera is not contagious, held by the majority of the Indian practitioners, is rendered more than doubtful by its European invasion. The treatment by large doses of calomel and opium, enjoined by most of them, has been tried, and found wanting. The opinions of the Indian practitioners may be of value as regards cholera in India; but the cholera in Europe has refused to conform itself to their views, and we therefore think it would not be prudent to be guided by their opinions. We cannot avoid the conclusion, that a set of intelligent men, with minds free from preconceived notions, and with no favourite theory to maintain, are more likely to arrive at safe conclusions. All the information that is to be had upon the subject the Board of Health possess; and they have availed themselves of the assistance of several intelligent men, who have seen the disease in Asia. They know that a trust of fearful responsibility is reposed in their hands, and that the eyes of the public are anxiously fixed upon them. They number among them some of the most experienced and talented physicians of the metropolis, and they will soon have

the co-operation of the enterprising men now on their return from Russia. While, therefore, we see fully the difficulty of their situation in a period of such alarm, and while we are satisfied that more is expected of them than, in the nature of things, they can effect, we feel assured that a general attention to the directions which—certainly not without due deliberation—they have issued, will do all which foresight can accomplish to mitigate the evil, should all efforts prove insufficient entirely to avert it.

RESUMÉ OF THE POLISH CHOLERA.

At a recent sitting of the French Academy of Medicine, MM. Chamberet and Allibert, members of the Warsaw Medical Commission, assisted; and upon being invited to give the meeting some account of the results of their experience, M. Chamberet rose, and said:—that he had no doubt of the identity of the Polish and Indian cholera: that with regard to *symptomatology*, the most striking circumstances were the epigastric anxiety, the excessive and multiplied discharges from the stomach and bowels, the cramps, the sinking of the features, &c. The disease, he continued, suddenly attacked those who were apparently in the most perfect health. On the second or third day the nervous symptoms prevail: the patient is like one *deadly sea-sick*: he is in a state of idiotism. If, however, he gets over the fourth or fifth day, he will probably recover.

The great predisposing cause, M. Chamberet is of opinion, is a peculiar state of the atmosphere; but besides this, there are poverty, want of food, and frequently indigestion from surfeiting, which act as occasional causes. The Polish soldiers, who get their rations for three or four days at a time, gorge themselves on the first day, and thus expose themselves an easy prey to the disease.

With regard to the *morbid anatomy* of cholera, M. Chamberet states that inflammation of the digestive tube is never absent except when the malady has proved rapidly mortal. The mucous membrane of the stomach and intestines is lined with a pultaceous liquid, of a greyish colour, mixed with mucous and alimentary matters. The liver is soft; and its membrane detachable with the greatest ease. The gall-bladder distended with a large quantity of greenish bile. The venous system is gorged with thick black blood; the urinary bladder generally empty, and contracted, sometimes to the size of a common nut; the meninges are generally injected; and a quantity of limpid, and sometimes bloody serum, is contained by the arachnoid and rachidian membranes.

As to *treatment*, numerous have been the articles of medicine employed, but all of them apparently with equal success. Calomel and nitrate of bismuth have been much lauded. Dr. Leo put twenty-two patients on the nitrate, and twenty of them died. Thirty were put on calomel, twelve of whom recovered, and the remaining eighteen died. But the approved method adopted by the committee was this:—bleeding; warm infusion; spirituous frictions; and sinapisms applied to a large portion of the surface of the body.

And as to the question of *contagion*, the whole body of the Polish physicians reject the doctrine *in toto*. The disease, it is true, broke out at Warsaw on the 10th of April, after a bloody engagement between the Poles and Russians, but weeks and months before this, it has been ascertained that the cholera prevailed sporadically in the country. One physician, in particular, distinctly noted three fatal cases; there is at least, then, some uncertainty about its origin. But it is remarkable, that there were about

a hundred physicians, French, English, and German, employed about the sick in Warsaw, none of whom suffered from the cholera; ten of them even inoculated themselves with the blood of choleric patients. Nor were the porters or nurses of the hospitals, nor the undertakers, less exempt from the disease than the physicians. M. Chamberet added, that he had never seen cases that had been left totally destitute of medical aid; but the physician-general had, and he affirmed that the mortality was not greater among them than among those who enjoyed medical assistance; and that mortality was, probably, on the whole, about fifty per cent.

The thanks of the Académie were voted to M. Chamberet for his very satisfactory communications.

CLINICAL LECTURES,

Delivered in the Dispensary of the London University,

BY

DR. ANTHONY TODD THOMSON.

LECTURE II.

October 22, 1831.

Rheumatism.

GENTLEMEN,—In pursuance of the plan sketched out in my introductory lecture, I have selected four cases from amongst those admitted during the last week, as the subject of this discourse: they are those of James Pennifather, twenty-two years of age, a poulterer; Sarah Parker, a laundress, aged thirty; Elizabeth Sparrow, a laundress, forty four; and James West, a bricklayer, fifty-four. The disease under which these four persons are labouring is marked *rheumatism* in the case-book. They were all, with the exception of Pennifather, who had been for some time ailing, previously in a good state of health; and I am anxious to direct your attention to this fact, as it is one of the features which distinguishes Rheumatism from Gout. In one only of these cases, that of West, did the mode of attack correspond with the usual nosological description of acute rheumatism: this patient was seized with rigor, followed by pain in the knees, shooting along the limbs in both directions, which continued with more or less severity for nearly a week; it then sud-

denly left the limbs, and affected the head, particularly over the forehead, where it has ever since continued. It increases in the afternoon, and attains such a height as to prevent him from sleeping until near the morning, when he perspires profusely. He feels languid during the day; the bowels are lax; the urine is nearly natural; the tongue furred; the pulse 84, and sharp; and the skin clammy. In Pennifather, the rigor was slight preceding the pain, which suddenly attacked the side and the back part of the head and neck, and has been unattended by any remission or intermission. He perspires much at night. The bowels had been freely opened by a Calomel pill and purgative, three days before he presented himself at the dispensary, and they are still open; the urine is high coloured; the tongue clean and moist; the pulse 108, and soft. In the cases of Parker and Sparrow, the pains, which also followed a shivering fit, and were succeeded by heat and perspiration, attacked the pectoral muscles, and the scapula, passing, as it were, through the chest. In Parker, slight pains, extending from the shoulder to the wrist, were felt before the febrile attack commenced, and these gave way when the thorax became affected. The increase of pain at night, the perspirations, and other symptoms, were nearly the same as in the other cases. Sparrow is troubled with a slight cough, which leads me to remark that Rheumatism has been observed to alternate with habitual cough and expectoration, these always recurring after the decline of the rheumatic attack, and being as regularly suspended during its presence. It also, occasionally, alternates with cutaneous eruptions; and I have witnessed more than once its alternation with Hæmorrhoids.

Now, gentlemen, the history of these cases displays the difficulty of framing any general definition of a disease. In one circumstance only, that of the augmentation of the pain by external heat, do they all accord with the definition of Dr. Cullen; and in none of the cases, except West's, were the pains first felt in the large joints. Another exception to the description of authors meets your eye in the appearance of the urine, which is generally said to be high-coloured in the beginning; whereas in these cases it was natural, and only in Parker's did it assume a high colour, on the second day after she presented herself at the Dispensary; and which it has since maintained. This may be accounted for by the sweats in her case being more profuse than in the others. In none of these cases has it yet deposited the pink sediment which is usually perceived in it after the fever assumes an intermitting character, but which is not, however, characteristic of Rheumatism, as it occurs in Ague, Gout, and many other acute inflammatory diseases. The most copious de-

posit of this sediment which I ever witnessed occurred in a case of Hypertrophy of the heart, with Dropsy, which proved fatal. But you will find that in Rheumatism the urine is neither always high-coloured nor deposits pink sediment, and that the pains attack every part of the body. In youth they are said to affect more generally the head, the muscles of the chest, and the upper extremities; and in adult age, the hips, thighs, loins, and back; and this observation is verified by the cases before us. Parker's case, also, apparently confirms the remark made, I forget by whom, that, when the breathing only is affected the pectoralis major is the seat of pain; but that, when both the major and minor are affected, there is, besides, an acute pain, which seems to dart through the thorax, between the breast and the scapula. When first Parker was attacked with pain under the breast, the breathing was impeded; but when it had continued for twenty-four hours, and may be supposed to have extended to the deeper-seated muscle, every inspiration was accompanied with pain, passing from the breast through to the scapula.

In these recorded cases the pain was accompanied by neither swelling nor redness; but in one of those not recorded, that of Sarah Padwell, a young, plump female, the wrists were much swelled, and painful to the touch, although not at all red. When redness occurs in Rheumatism, it differs from that which characterizes Gout, in not presenting the shining appearance, nor being accompanied with the tension always more or less present in gouty inflammation.

The age of the youngest of these patients is twenty-two; but I have met with instances of the disease in children of seven years; and you will find that it is limited to no period of life, from five years to extreme old age, but the greatest number of cases occur between fifteen and thirty. Besides these four patients, three other cases of Rheumatism have been admitted during the week, which is rather at variance with the received opinion that the disease prevails more in the colder than the warmer seasons. I am disposed to ascribe its present prevalence to the sudden transition from dry to rainy weather. In the year 1826 the summer was unusually hot and dry; but after the 18th of August a considerable quantity of rain fell, and numerous cases of Rheumatism appeared, although scarcely any were seen before the rains set in.

Few as these cases are, they enable me to point out to you, gentlemen, the frequency of metastasis in Rheumatism. In West's case the pain suddenly left the limbs, and attacked the head. This individual is of a sallow complexion, and of a comparatively weaker habit of body than the other three patients. Nothing is so much to be dreaded in Rheumatism as a translation of the pain

and inflammation from the joints or muscles to a vital organ—the brain, for instance, or the lungs, kidneys, or stomach. It is sometimes the result of the improper application of rubefacients; and it is also said to follow copious and repeated venesection, but occasionally it occurs without any obvious cause. When it affects the brain, and death *immediately* follows, which I have seen twice happen, no appearances indicative of previous inflammation can be detected either in the membranes or in the substance of the organ; but when the fatal issue is not so instantaneous, the membranes generally display great vascularity, with adhesions, a deposition of gelatinous matter on the pia mater, and also a deposit of fibrine, forming adventitious membranes. There is commonly great dryness of the dura mater, and a smaller quantity than usual of fluid is found in the ventricles. In a case of metastasis to the lungs, followed by Chorea, which came under my notice in private practice some years since, and terminated fatally, the left phrenic nerve was found to be in a highly inflamed state, with considerable congestion of the pulmonary vessels, but no other appearances of diseased action were detected. Fatal metastasis, however, is of rare occurrence; in upwards of a thousand cases which I have seen, four only proved fatal, from the translation of the disease to a vital organ.

In two only of the cases of Rheumatism admitted in the last week, including those under consideration, have I been able to trace the exciting cause of the disease. West ascribes his attack to working two days in a damp cellar; and Elizabeth Sparrow refers her's to having lately removed into a lodging which is damp. Water, indeed, in a state of vapour, seems to be the principal and most frequent remote cause of Rheumatism. A query thence arises, at what temperature does it acquire its noxious influence? From the disease occurring, even in warm climates, in rainy seasons, we might, *a priori*, suppose that a low temperature is not necessary for this purpose; but that there is some fallacy in this inference may be concluded from the results of a series of observations and calculations made by Mr. Gorrie, in the Carse of Gowrie, in Scotland, on the rain gauge and thermometer in 1823-4-5; from which the fact appears to be established, that the greater the fall of rain in a given season, the colder that season will be found to be.—(*Horticult. Trans.* vol. vi.)

I would venture here, gentlemen, to throw out an hypothetical conjecture, which, if it do not enlighten you, may afford an exercise for your minds; knowing that much hydrogen is evolved in fogs, is it not probable that this is also the case whenever the atmos-

phere is surcharged with aqueous vapour? and as Professor Leslie has ascertained that the process of cooling proceeds more rapidly in hydrogen gas than in atmospheric air, how far is this likely to operate as a cause of Rheumatism? It is a well-known fact that the disease is often produced at sea, by frequent washing the decks, a custom now judiciously frequently dispensed with. This observation is also confirmed by the accounts of voyagers in different parts of the globe. In Weddel's voyage towards the South Pole, we are told that when the ships were in latitude $61^{\circ} 44'$, longitude $31^{\circ} 13' 15''$, having had a long course of dense fogs and fresh gales, the decks of the vessels were constantly wet, which produced amongst the seamen colds, agues, and *rheumatism*. As the voyagers in this case were far from land, and yet agues appeared, it may be asked whether the vegetable matter of the decks can affect the decomposition of the water, and produce something analogous to marsh miasma? In the account of Kotzebue's voyage of discovery, we are informed that near the Coral Islands, during frequent rains and gusts of wind, a sailor was attacked with Rheumatism, and another with Sciatica, (vol. 2, p. 338, Trans.) Near Luçon, in the Chinese seas, Rheumatism manifested itself on account of the great dews that fell even before sun-set, (*ibid*, p. 339); and again, "immediately after we left Manilla Bay, (says Kotzebue) the heavy night-dews, in the neighbourhood of the high land, caused catarrhal and *rheumatic fevers*," (*ibid*, p. 342). Rheumatism is very common in Switzerland, in situations confined between chains of high mountains, exposed to damps, and great variations of temperature; and in Canada the lumberers, who fell pines in the woods, and are exposed to great damps and chills, are generally the victims of Rheumatism. There can be no doubt, therefore, that moisture is the principal cause of acute rheumatism; but, it may be asked, does this operate in producing the disease by checking perspiration? To reply to this query, gentlemen, would lead to a long theoretical discussion, rather out of place in a practical discourse; it is of more importance to determine the length of time which elapses between exposure to moisture and the attack of the disease. In West's case it was six hours; and in general I have remarked that it is within twelve hours. Dr. Haygarth, (*Clinical History of Diseases*, Part I.) in noticing this period, which he terms the "latent period," referring to his tables, says, "out of twenty-one cases only four exceeded the period of forty-eight hours. The shortest time noted is half an hour, but I believe that the cause and effect are sometimes connected together without any interval of perfect health."

With respect to the treatment of this dis-

case we can yet say little, as far as relates to the cases before us, as Pennifather only can be considered in a state of convalescence. By looking into the Case-book, you will find that the same means were employed in his case, West's, and Parker's. He and West were cupped upon the nape of the neck, and sixteen ounces of blood abstracted; and the bleeding was immediately followed by the administration of a pill containing five grains of Calomel and two of extract of Opium; a mixture was also prescribed for each, containing four fluid drachms of the wine of the seeds of Colchicum, a fourth part of which was taken every sixth hour. In both, the pains were much relieved on the following day, and in Pennifather they were completely gone on the 18th; at which time also the pulse had fallen to 72, and was soft and regular. He was then ordered three grains of sulphate of Quinia, in ζij . of infusion of Gentian, to be taken three times a-day. On the 20th he continued still improving, and his strength much increased; $\mathfrak{M}vii$. of diluted sulphuric acid were added to each dose of his mixture. To day (22d) he says that he felt strong enough to go to work again; but in the morning he had a renewed attack of the pain in the back of his head, which, in an hour afterwards, moved to the forehead, and then left him. As his bowels had not acted for thirty hours, he was ordered a pill with two grains of Calomel and three of extract of Henbane, and a purging draught for the morning, and, after their operation, to continue his mixture. In Parker, twenty ounces of blood were taken by cupping between the shoulders, and the same medicines prescribed as in Pennifather's case. Here the Colchicum produced nausea and vertigo until it operated. One stool followed each dose. She was much relieved, and continued free from pain until the morning of the 20th, when the pain, but in a less degree returned, and continued the whole of the day. She perspires on the least exertion; the urine is high coloured, but clear; the tongue reddish, and slightly furred. A pill, containing seven grains of Calomel and four of extract of Henbane, was ordered for bed-time, and a strong purging draught for the morning, with the mixture to be continued.

The employment, in acute rheumatism, of Opium and Calomel after bleeding, was, I think, introduced by Dr Robert Hamilton, of Lynn Regis, in 1764, but his dose of Opium seldom exceeded one grain; and he occasionally combined tartar emetic with it. In my own practice, I have long since discontinued the free use of the lancet in Rheumatism, and also the constant use of diaphoretics; and have trusted to a single local or general bleeding, where any was necessary, following it with a large dose of Opium, from

two to three grains, in combination with from six to ten grains of Calomel, and a purge in the morning; which, notwithstanding Sydenham's condemnation of opiates, however acute the pain, has almost in every instance been followed by relief.

In the early part of my practical career, when fresh from the schools, I bled freely in Rheumatism, and repeated the bleedings; but I soon found that the buffy coat and cupped state of the blood continued to shew themselves on each bleeding, until the strength of the patient was reduced to an alarming degree. This was sufficient to set me to think for myself, and I have scarcely used, or ordered to be used, the lancet for upwards of twenty-five years, in acute rheumatism. When the symptoms authorize the abstraction of blood, I find every indication answered by cupping over the seat of the pain; but it is only when the pain attacks the head, or thorax, that I find it requisite.

I have rarely seen much benefit derived from Colchicum until it purges, provided this do not occur too soon after it is taken; in which case, although it purges, yet it does not bring away bilious stools, and little or no rosacic acid; the pink sediment appears in the urine. When this rapid purgative effect occurs, it is necessary to combine the wine of Colchicum with Magnesia, or some alkali, by which the irritability of the stomach and duodenum is allayed; and thence the Colchicum has time to stimulate both the hepatic and pancreatic ducts, and to be taken into the system. If it still run off by the bowels, I order the powder of the bulb, and combine it with the compound powder of Ipecacuanha. Were I to venture to theorize respecting its action, I would refer much of the benefit derived to its operation on the liver, and the influence of this organ over the function of the kidney, causing the excretion of rosacic acid, which appears to be critical in this disease. The reciprocal influence of the liver and kidney on one another, is well illustrated by an experiment of Dumas and Prevost. They cut both kidneys from a dog, and on examining the body after the death of the animal, they found the liver inflamed and the gall-bladder turgid with greenish or deep brown bile. The effect of the Colchicum in augmenting the proportion of the rosacic acid which is passed by the kidneys, is illustrated by observations of Professor Chelin. In a severe case of Gout, he ascertained that, before the use of the wine of Colchicum, the quantity of the acid deposited, both free and combined with Ammonia, was0.069

On the 4th day of using the wine, 0.076

8th day 0.091

12th day 0.112

When the disease assumes its chronic or protracted form, I have seen the solution of tartar emetic more useful than any other

remedial agent. I order gr. vi. of the tartrate of antimony and potassa to be dissolved in \mathfrak{z} vi. of distilled water, and taken in doses of two table-spoonfuls every fourth hour, so that the six grains may be administered in twenty-four hours. The two first doses generally produce vomiting; but the others only slight nausea. The disease commonly yields to this treatment in the course of six or eight days.

I shall reserve any remarks on my observations respecting the use of bark and Quinia in the latter stages of Rheumatism, until we have seen the result of the cases now under treatment*.

REPORTS OF CASES OCCURRING AT PUBLIC INSTITUTIONS.

GENERAL DISPENSARY.

Operation for Stone, with Clinical Remarks.

WILLIAM HENRY LOOKER, aged three years and eleven months, was admitted under the care of Mr. Coulson, October 21st, 1831. The mother says that the child has been afflicted for these last eighteen months with a "complaint in his water," and that latterly the child's sufferings had been most acute, particularly when it passed its urine. He was sounded and a calculus distinctly felt. The boy's health had of late very much suffered, from the constant pain and irritation under which he had been labouring.

Saturday, Oct. 22d.—Mr. Coulson operated on the child. A lithic acid calculus, of the size of an almond, was extracted; and, during the extraction, a small one, of the size of a large grain of wheat, fell on the ground. The operation was completed within the minute.

This is the third dispensary patient on which Mr. C. has operated for stone since April last. The child is doing extremely well.

After the boy was put to bed, Mr. Coulson made the following observations:—Gentlemen, you are aware that a mode of relieving a patient with stone is now coming into use, by which the cutting process is avoided, and the stone is reduced to minute fragments in the bladder, either by crushing or drilling it.

Nothing, I can assure you, would give me greater pleasure than to see the necessity for

performing the lateral operation entirely dispensed with; for though a surgeon may well be proud of performing operations with celerity and safety, there is an object of still higher importance, and more worthy of his ambition, which is the lessening, as far as he can, the necessity for their performance at all. But there are cases in which, I fear, the lateral operation for stone will always be necessary. The child on which I have just operated is an illustration of this.

The clergyman of the parish in which the mother resided, from the most humane motives, recommended her to take the child to Baron Heurteloup. The Baron saw the boy two or three times, and after mature deliberation on the case, sent a note to the mother, of which the following is an extract:—"For such young children his operation is less favourable and safe than for grown-up persons; and the old operation is, in cases of very young children, less dangerous and less to be dreaded. Under these circumstances the Baron would recommend Mrs. L., as the most prudent and safest plan which she can follow, to have the stone taken out by the cutting operation, and that without delay." Such are the observations of the Baron Heurteloup, and I am sure that you cannot fail to be struck with the frankness and candour with which this ingenious man delivered his opinion on the merit of his own mode of operating. The ardour of enthusiasm has not blinded his judgment, but he candidly acknowledges some of the cases to which his operation is least, or not at all, applicable. Prior to performing the operation for stone, the surgeon should be extremely particular in preparing his patient, by those means which experience has shewn to be best conducive to this end. Celsus was aware of the necessity of bringing the patient into a fit state for the operation, by diet and abstinence; in fact, the most successful lithotomists are those who are most attentive to this point. Mr. S. Cooper, in his *Surgical Dictionary*, or at least in the edition which I have, has given no practical directions on this most important point. Obsolete instruments, and obsolete modes of operating, are detailed with minuteness, and this indispensable part of the treatment of a lithotomy patient is not at all alluded to. If the patient's health is much impaired, and the bowels are relaxed, which is not unfrequently the case in children, you must, before the operation, lessen the irritability of the bladder and bowels by an anodyne, and improve, as far as you can, the patient's health, by medicine and attention to diet. I generally give, on the night preceding the operation, a few grains of hydrarg. c. creta, with the pulv. rhei, and early on the following morning some castor oil. Two clysters should be given before the operation, one two or three

* In Dr. Thomson's Lecture inserted last week, for "*res angustie domi*," read "*res angustie domi*."

hours after the oil, composed of gruel, olive oil, and salt; and the other about an hour before the operation, with common gruel and twenty or thirty drops of laudanum in it. If, when the surgeon arrives to perform the operation, the last injection has not come away, he should urge the patient, if he be an adult, to go to stool; if, however, it is a child, it will generally happen that, if the injection has not passed off, it will do so at the time the staff is introduced. On no account should the surgeon undertake the operation till the injection has come away. The patient should be placed on a table of sufficient height, so that the perineum be opposite to the breast of the surgeon. The table should rather be a little too high than too low, for it will be an advantage to you in the operation to be a little under your work than above it. After the patient is bound, his shoulders and back should be raised and supported with pillows; he should be brought to the edge of the table, and his thighs be widely separated by an assistant; but it is of great importance that the nates be kept straight, and that an inclination be not given to one side more than the other. I now introduce a curved staff, and give it to the care of an assistant. You no doubt observed that Mr. Pereira, who held the staff for me (and has done so on two or three other occasions), gave to the instrument a different direction to that in which it is usually held. Instead of holding it perpendicularly, or nearly so, Mr. P. inclined the handle a little towards the ground, with the groove turned towards the left side. By this inclination of the handle, you certainly make the groove of the staff less prominent in the perineum; but there is this advantage, when you have cut into the groove, you have no occasion to alter the position of the staff, and the fore-finger of your left hand is quite at your disposal for protecting the rectum, and guiding the knife. I find that I can perform the operation much more rapidly in this way, than by taking the staff into my own hand. M. Langenbeck is a strong advocate for this mode of holding the staff; he, however, advises the handle to be inclined still more towards the ground than I do. I begin the first incision rather low, about two fingers breadth above the anus; the bulb of the urethra will be then avoided. In fact the external incision, if commenced higher up, can be of no use to you; and I find that this, the upper part of the wound, is often the slowest to heal. Respecting the remaining steps of the operation, I have little new to offer. For the division of the prostate I use the long straight knife, with the beak in the middle line of the point. I began in 1828 with the gorget; but whatever merits this instrument may possess in the hands of an experienced operator, I feel confident that, for a beginner at

least, the knife is the safest and best instrument which he can employ. If any hemorrhage occur, you must endeavour to compress the bleeding vessel with your finger.

The after treatment consists, if no inflammatory symptoms ensue, in merely keeping the patient quiet, though it is extremely difficult to carry this into execution with children. The knees by some surgeons are tied together; by others they are not; but they should always be kept raised, and the scrotum supported. No application is required to the wound; but the urine should be received on sponges, and the parts kept clean. Some urine soon begins to flow through the natural passage. In a man above sixty, on whom I operated some time ago, a considerable quantity of water came through the urethra on the morning following the operation. The time, however, varies at which the water entirely ceases to pass through the wound; and in some rare cases urinary fistulæ remain.

PRACTICAL DIRECTIONS REGARDING CHOLERA.

[The following memorandum on the subject of cholera has been issued by the Board of Health. The "rules and regulations" intended to prevent its introduction, and which recently appeared in the London Gazette, we shall probably take another opportunity of bringing before our readers.]

BOARD OF HEALTH.

College of Physicians, Oct. 20.

The following are the early symptoms of the disease in its most marked form, as it occurred to the observations of Dr. Russel and Dr. Barry, at St. Petersburg, corroborated by the accounts from other places, where the disease has prevailed:—

Giddiness, sick stomach, nervous agitation, intermittent, slow, or small pulse, cramps, beginning at the tops of the fingers and toes, and rapidly approaching the trunk, give the first warning.

Vomiting, or purging, or both these evacuations, of a liquid like rice-water, or whey, or barley-water, come on; the features become sharp and contracted, the eye sinks, the look is expressive of terror and wildness; the lips, face, neck, hands, and feet, and soon after the thighs, arms, and whole surface, assume a leaden, blue, purple, black, or deep brown tint, according to the complexion of the individual, varying in shade with the intensity of the attack. The fingers and toes are reduced in size, the skin and soft parts covering them are wrinkled, shrivelled, and folded; the nails put on a bluish pearly white; the larger superficial veins are mark-

ed by flat lines of a deeper black; the pulse becomes either small as a thread, and scarcely vibrating, or else totally extinct.

The skin is deadly cold and often damp; the tongue always moist, often white and loaded, but flabby and chilled like a piece of dead flesh. The voice is nearly gone; the respiration quick, irregular, and imperfectly performed. The patient speaks in a whisper. He struggles for breath, and often lays his hand on his heart to point out the seat of his distress. Sometimes there are rigid spasms of the legs, thighs, and loins. The secretion of urine is totally suspended; vomitings and purgings, which are far from being the most important or dangerous symptoms, and which, in a very great number of cases of the disease, have not been profuse, or have been arrested by medicine early in the attack, succeed.

It is evident that the most urgent and peculiar symptom of this disease is the sudden depression of the vital powers; proved by the diminished action of the heart, the coldness of the surface and extremities, and the stagnant state of the whole circulation. It is important to advert to this fact, as pointing out the instant measures which may safely and beneficially be employed where medical aid cannot immediately be procured. All means tending to restore the circulation and maintain the warmth of the body should be had recourse to without delay. The patients should always immediately be put to bed, wrapt up in hot blankets, and warmth should be sustained by other external applications, such as repeated frictions with flannels and camphorated spirits; poultices of mustard and linseed (equal parts) to the stomach, particularly where pain and vomiting exist; similar poultices to the feet and legs, to restore their warmth. The returning heat of the body may be promoted by bags containing hot salt, or bran, applied to different parts of it. For the same purpose of restoring and sustaining the circulation, white wine-whey, with spice, hot brandy and water, or sal volatile, in the dose of a tea-spoonful, in hot water, frequently repeated, or from five to twenty drops of some of the essential oils, as peppermint, cloves, or cajeput, in a wine-glass of water, may be administered; with the same view, where the stomach will bear it, warm broth, with spice, may be employed. In very severe cases, or where medical aid is difficult to be obtained, from twenty to forty drops of laudanum may be given, in any of the warm drinks previously recommended.

These simple means are proposed as resources in the incipient stage of the disease, where medical aid has not yet been obtained.

In reference to the further means to be adopted in the treatment of this disease, it is necessary to state that no specific remedy

has yet been ascertained; nor has any plan of cure been sufficiently commended by success to warrant its express recommendation from authority. The Board have already published a detailed statement of the methods of treatment adopted in India, and of the different opinions entertained as to the use of bleeding, emetics, calomel, opium, &c. There is reason to believe that more information on this subject may be obtained from those parts of the continent where the disease is now prevailing; but even should it be otherwise, the greatest confidence may be reposed in the intelligence and zeal which the medical practitioners of this country will employ in establishing an appropriate method of cure.

HENRY HALFORD,

President of the Board.

CAJEPUT OIL.

M. LAMARE, an old practitioner in the Isle of France, has lately written a strong letter to the Paris Académie de Médecine inveighing against the use of cajeput oil. It is not at all employed by the Indians, he says, in the cure of cholera: it is an *incendiary* medicine that cannot be too carefully guarded against: it may, indeed, be used with some advantage externally in chronic rheumatism; but it contains copper, and is dangerous, &c. In short, he is indignant at the pains which seem to have been taken in the French journals to cry it up, which he attributes altogether to cupidity and ignorance.

The reading of the letter elicited some facts from members of the Académie, who entered into a discussion upon it. The great specific gravity of cajeput oil is, according to M. Laubert, a characteristic of its genuineness, which fraud cannot imitate. Its green colour does not always prove that it contains an oxide of copper, according to M. Caventon: it is frequently produced by an organic colouring substance inherent in the leaves; and the quantity of copper found in it is no more than 1-22nd part of a grain in each *gros*. Neither is the oil caustic, according to M. Pelletier, except when taken quite pure, like the essential oil of peppermint, &c. M. Lamare's letter, however, was thought worthy of being communicated to the journals.—*Gazette Médicale*.

THE
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SATURDAY, NOVEMBER 5, 1831.

LECTURES

ON

THE THEORY AND PRACTICE OF
MEDICINE ;

Delivered at the London University,

By DR. ELLIOTSON.

LECTURE V.

Explanation of the term Diathesis—Symptoms of Disease—Mode of examining these—Appearances of the Face—of the Tongue—of the Eyes—Risus Sardonicus—Facies Hippocratica—Expression of the Countenance—Examination of the Hand—the Pulse—its Characters.

THERE is a term, gentlemen, which, on reflection, I think I omitted to explain, or even to mention at all—namely, the word *diathesis*.

When the body is particularly predisposed to any kind of affection, it is said by some to have that particular *diathesis*, or a disposition to it. However, you will generally see this term limited to imply two states—an inflammatory state, and a state of weakness, of debility, if not of putrescency. You will hear the one called the *phlogistic*, or *sthenic*, which you know means strong, and the other called the *asthenic*, or weak. When a person, for instance, is in a high state of excitement, with a full and strong pulse and heat of body, and under any particular complaint, he is said to have the *sthenic* or *phlogistic* diathesis—in fact, an active inflammatory state of the body. If, however, the state be one of debility, so that any disease which is actually present is attended with weakness, and still more if by extreme collapse and a disposition to putrescence in its progress, then it is called the *asthenic* diathesis. I believe, in modern times, these terms are rarely used, except

when a certain disease is present, so that they really imply, not the disposition to disease, but the character of the disease—and, indeed, as to whether it is actively inflammatory, attended by strength, or whether it is attended by weakness; but even of these two, you seldom hear persons speak of either, in this country, excepting the *phlogistic*.

I may mention that the word *diathesis* is used by old authors in other senses—to signify the general disposition to disease at any particular season, so that the constitution of the period was said to be its *diathesis*. If the disposition of any particular season of the year—not the regular seasons—were to produce such and such kinds of disease, the word *diathesis* was applied to that disposition—to the disposition, if I may so speak, of the atmosphere at the time. But, generally speaking, the word is used as I have pointed out above.

However, you will find a constitutional disposition to certain diseases, when present, called a *diathesis*. For instance, it is not uncommon, in practice, to hear persons talk of a *scrofulous* diathesis. Where a person has every look of *scrofula*, with its actual presence in one part, you will hear individuals say, that is a *scrofulous* diathesis. A disposition to many other kinds of disease, when present, is sometimes called a *diathesis*, but it is the *phlogistic* diathesis of which we hear the most.

This term is sometimes used to signify that full state of the body which would render a person very liable to an active inflammatory affection; but is certainly more frequently limited to that state when the disease actually has begun. When a violent disease begins, and there is quickness and strength of pulse, and heat, the person is said to have the *phlogistic* diathesis. Now and then, persons with merely a full pulse are said to be labouring under the *phlogistic* diathesis; but that state is characterized more particularly by the word *plethora*, of which I spoke in the last lecture. I stated, that when the blood was

in excess to the strength and to the vessels, true plethora was said to exist—*plethora vera*.

Speaking of plethora, you will pardon me for recapitulating what I mentioned in the last lecture—viz. that there is true plethora, *plethora vera* or *absoluta*—that is to say, out of proportion to the strength or the vessels—*plethora ad vires*, or *plethora ad vasa*. I also stated that a fanciful kind of plethora was mentioned in old authors, where the blood is not supposed to be excessive in quantity, but to be expanded; and that is called *plethora ad molem*, or *plethora apparens*;—and another species of plethora was supposed to exist when the containing vessels were diminished in capacity, so that, though the quantity of blood was the same, the vessels became too small for it; and that has been called *plethora ad spatium*—the space being too small.

In the last lecture I began the consideration of symptoms in general. I may mention that, in some writers, you find symptoms and signs distinguished from each other, but I think it mere trifling to dwell upon such matters. With respect to symptoms, I was going to consider them as they were observable by the patient, or observable by others, because many are observable by the patient alone, and are not cognizable to others; those, however, which others observe, of course the patient, for the most part, may observe likewise. I have considered the chief of those of which a patient alone can take cognizance, and I will now proceed to consider those which the by standers, and particularly medical by-standers, observe.

Now, in observing the symptoms of disease, it is right to remember that providence has blessed us with five senses; and all these five senses it would be ungrateful, as well as unscientific, in us not to employ. I am led to speak in this way on account of its being a modern practice to employ the ear extensively. Those who do so are laughed at as innovators—as people who take unnecessary pains, and are perhaps attempting a piece of quackery, in order to produce an impression upon patients: at any rate they are laughed at: but it will be impossible for them to be laughed at long. If nature have blessed us with hearing to observe certain phenomena around us, there can be no reason for not employing it when we come to observe the phenomena of disease; and if it be a fact that symptoms take place of the most useful nature that are cognizable by the ear only, it is our duty to employ our ears for the purpose of detecting them. We are bound, I think, to employ all our five senses in the investigation of disease. The chief, however, of which we make use, are the sense of sight, the sense of hearing, and the sense of touch; but the smell, and in some instances even the taste, may be called in to our aid; the smell, however, much more frequently than the taste. It would be as ab-

surd to shut our ears as to shut our eyes, yet, because the extensive employment of hearing has only been introduced lately, those who were not instructed in this point when they were students, are, some of them, too proud to condescend to learn in their old age, and are good enough to pity us younger men.

As to those symptoms which are cognizable by our various senses, they discover themselves and are apparent in every part of the body, from the head to the foot; but the two parts which give the chief symptoms to the sight, and I may say to the touch, and the chief symptoms not only in reference to themselves but to all other parts of the body, are the *head* and the *hand*. It has become the custom, in all civilized nations, to cover all parts of the body except the face and hands; and these two parts afford far more information respecting the state of the body, and respecting diseases that are present, and not seated in these individual parts, but at a distance, than any others. It is certainly a striking coincidence; but we must remember that while necessity compels mankind to keep the face, and almost always the hand, uncovered, Providence has ordained that these same two parts shall convey the chief information to others respecting our mind and body at all times.

With respect to the face, we observe that it gives us two sorts of information—first, as a mere portion of the body only, a portion of the surface which is far more affected by every change than almost any other excepting the hand, and therefore so far as it is *face*; and, secondly, it gives us another description of information so far as it is *countenance*, so far as it expresses the state of the mind and the state of feeling altogether. The observations which you make upon this part of the body may be considered as they respect simply *face*, and as they respect simply *countenance*.

With regard to the face as really a part of the surface of the body, if there be fullness of blood it is shewn particularly there. If you were to take the same quantity of surface in any other part of the body—the abdomen, back, or thighs—you would not get the same information from the part, simply as surface, as you do from the face. If the body be at all full, you see it in the face. If the circulation be rapid and the heat considerable, it is the face that shews it; the cheeks become tinged much sooner than any other part. If there be the least degree of coldness, you perceive it in the cheeks, nose, and lips. You discover hectic fever in an instant by the red patch upon the cheek. If the patient be labouring under difficulty of breathing, you find the eyes immediately suffused, you find the lips more or less livid, and the whole face frequently of a leaden hue; sometimes, indeed, I have seen it black. If it be jaundice that the patient

labours under, you discover it instantly in the face; by one part of the face you will detect it long before it appears in any other part of the body except the hand; and when the disease has nearly declined, it is in the face as well as the hand that it lingers the longest. In the white of the eye you perceive the presence of jaundice before it is apparent any where else, except at the root of the nails. If the patient be bloodless, you first discover it in the paleness of the face, the want of the usual vermilion of the lips, and in the absolute paleness of the tongue; there is not a white crust upon the tongue, but that organ is absolutely blanched. One of the chief marks of scrofula is shewn in the face; there is the dilated pupil and the tumid under lip.

Then, again, in the *tongue* you have an immense number of symptoms. The body cannot labour under inflammation in any part but the tongue becomes affected by it. If there be strength, generally the tongue becomes white; if there be inflammation within the stomach and intestines, it frequently becomes red—red at the tip, red at the sides, perhaps red all over, and perhaps there are red stripes upon it. If the stomach be simply in a state of dyspepsia, and the bowels confined, you have the tongue covered with a yellow thick mucus. The indications of the tongue are innumerable; they have been observed from the remotest ages; and no one would think of finishing his attention to a patient without looking at that organ. The tongue sometimes will become brown, sometimes it will become absolutely black. In what are called putrid diseases, where there is a high degree of debility, and a disposition to putrescence, the tongue will become black. In other cases it will become glazed throughout, shining, and glassy; and in this state it will frequently be cracked into the bargain. In other diseases, again, as, for instance, in delirium tremens, it is frequently covered by a creamy mucus—not a thick yellow mucus—it is moist, not dry—but a mucus more like cream than any thing else, and so liquid that the tongue is moist. I need not insist upon the great importance of that part of the face or head—the tongue.

Then, again, if we take the *eyes*. We discover in a moment whether there is great excitement of the brain going on by the sparkling of the eyes, by their redness, and by their being watery. We can discover whether a person has headache by the oppression shewn in the eyes; and in some affections of the head, the eyes squint—are contorted in various ways. Frequently, too, when a person is asleep, he does not completely close his eye, and that is often an indication of disease. Then, again, the state of the pupil is very characteristic of many affections of the head. If you take

the eye altogether, there is generally an appearance of brilliancy, or of dulness; or if you take particular parts, there is a dilatation of the pupil, or on the other hand, an extreme contraction; and all these things it is of the highest importance to observe, because they point out particular affections. For example, if you take apoplexy, you see the pupil usually dilated, and you know that there is great oppression of the brain; but if the pupil be amazingly contracted—reduced to a pin's point, you may almost to a certainty say that the patient will die: I do not say it is *absolutely* certain, because it is wrong to speak positively. If a person have taken opium in a large quantity, there is the same effect produced—generally a great contraction of the pupil; and where you are not with certainty informed of the patient having taken this narcotic, the appearance I have just described will assist you materially in forming the diagnosis. Then besides the strabismus, and the open state of the eye during sleep, you have contortions in various instances, particularly if a child have convulsions of another part of the face—convulsions of the muscles of the lower jaw, so that he gnashes and grinds his teeth together during sleep, and his eyes roll.

From the state of the *orbit*, or at least the contents of the orbit around the eye, you judge of the extreme debility, exhaustion, and emaciation of the patient. If the parts around the eye become absorbed, the eye sinks back in the orbit, and a hollowness is produced, which is characteristic of extreme exhaustion and inanition, and this alone will enable you to say in a moment how much worse the patient has become.

From the *face altogether*, simply as a part of the body, you are able frequently to tell at once that the lungs are affected, or that there is a great disposition to affection of the lungs. It is impossible sometimes to doubt for a moment that if the patient be not labouring under phthisis, he soon will. From the peculiar appearance of the countenance altogether, particularly from the appearance of the eye, its transparency, together with the languor of the cheeks, or the flush upon them, no doubt can exist as to the nature of the disposition. And with respect to other parts, you can tell that the patient is labouring under organic disease of the abdomen from the look of the cheeks: I do not say it is so always, but the appearance of the face is of great use in enabling us to detect that circumstance. In organic diseases of the stomach, intestines, and surrounding parts, you have often a faint greenish yellow appearance of the face, something like that of a faded leaf, with minute vessels, not forming a patch of red, as in hectic fever, but reticulated, like net-work, on the cheeks. Where

you observe this, there is generally some part of the abdomen in a state of organic disease. You discover the strength of a patient in a moment by his countenance. When you are attending a case of fever, there is frequently no occasion to ask a question: you can tell whether a patient is better or worse by viewing his countenance. So with respect to many other diseases, when the patient is getting either better or worse, you can tell immediately by his face, what change has taken place subsequently to your previous visit. Every body makes this observation; and hence, when an individual meets a friend, he tells him that he looks well, or he is sorry to see him ill. If the appearance of the face be so characteristic of the state of an individual's health to an inexperienced person, how much more so must it be to the medical man who studies minutely every shade of change and difference that takes place in each particular part!

Speaking of convulsions of the face—such as squinting or contortions of the eye, or a grinding and gnashing of the teeth, I may state that there is one set of convulsions about the mouth which is mentioned under the term *risus sardonius*: it is a horrid sort of grin which you often see in persons who are about to expire. To me it is the most terrible of all sights. It has been thought to characterize inflammation of the diaphragm, but I do not know whether that is the case—inflammation of the diaphragm is a rare affection. It is common, however, for persons at death's door to grin frightfully. The phenomenon receives its name from a herb which grows in Sardinia, of a poisonous quality, and said to produce in those who eat it this horrid contortion of the mouth just before death.

You likewise see a singular appearance of the countenance, mentioned by almost every writer under the title of *facies hippocratica*; it is the usual appearance of the face immediately before death, when the body is at last completely exhausted, and life about to be resigned. It is so called because Hippocrates gave a most accurate description of it. His description is, sharpness of the nose, hollow-ness of the eyes, collapsed state of the temples, collapsed and contracted state of the ears; the edges of the nostrils everted; the skin of the forehead firm, tense, and dry; paleness of the whole face, perhaps even blackness, or a livid and a leaden appearance. These are the characteristics of the *facies hippocratica*. When you see a person dying from a disease that has exhausted him gradually, this is exactly the appearance of the face.

I may mention, although it is not in regular order, that among the symptoms of phthisis, one is taken from the teeth; not that all phthisical patients have the same state of

the teeth, but in many there is a beautiful whiteness of the teeth, with a degree of transparency and brilliancy which is comparatively seldom seen in persons who are not disposed to phthisis.

Speaking of eruptions, I may mention that the exanthematic and acute inflammatory cutaneous diseases, shew themselves much better on the face, for the most part, than anywhere else. It is on the face first that you discover a child is going to have the measles. When a child is in a state of pyrexia, and you are sure it is going to be ill of something or other, the face will first disclose it, if it be the measles. There you have the running of the eyes—there you have the fulness; and the first papule that present themselves are generally seen on the face. Most acute cutaneous diseases attack the face among the earliest parts; or, if they do not, yet the face, if affected, discovers the true nature of the disease better frequently than any other part; the reason of which is, that the face is much more vascular than other parts. The cheeks are more or less inclined to be red in most individuals, and the least feverishness reddens the face more than any other part of the body; and therefore it is that acute cutaneous diseases generally shew themselves best in that situation.

With respect to the face simply as a part of the surface of the body, you discover paralysis in it more easily than in any other part. If a person be sitting still, you cannot tell whether his arm or his leg is paralyzed; but if his face be paralyzed, you will generally in an instant discover it by the part affected being drawn to the opposite side, and perhaps from his inability to swallow his saliva. And so with regard to the tongue; the moment the person begins to speak, you frequently in paralysis can discover the existence of the disease; the tongue frequently in paralysis moves to one side, but it generally moves in an opposite direction to that in which the mouth is drawn. In fever, and other diseases, the tremor of the tongue is very characteristic. In general the whole body trembles, but the tongue does so particularly. In paralysis agitans, if the person lie still in bed, yet if he put out his tongue, it is seen to be in a great degree of tremor. In cases of dyspnoea and apoplexy the tongue is swollen.

But the face is particularly indicative of the general state of the individual's mind, and of the general state of the strength likewise; and we then, perhaps, should rather call it the *countenance*, and speak of the countenance than of the face—the countenance being the term generally used in reference to the state of the feelings. You will judge directly whether a patient is in pain or not, whether he is suffering, by the countenance. In colic, and in enteritis, the

agony is so depicted in the countenance that you in an instant discover that something must be wrong. I need not say that in insanity that passion of the mind which is predominant discovers itself in the countenance. You may at once detect a good-natured, a furious, a proud, or a desponding maniac. You may also discover much by the way in which his head is carried, to say nothing of his countenance. Those that are proud, carry the head upright; those that are cast down, carry it the reverse. But the countenance, independent of the position of the head, is indicative of the character of the disease; and again, an idiot is almost immediately discovered by the expression of his face. You ascertain in a moment by the countenance what the person's feelings are with respect to strength as well as to comfort, and what passions of the mind are predominant. You discover, as I before-mentioned, whether a person is better by observing his face, by noticing whether it looks fuller; but you discover much from the very expression of the countenance. You see, therefore, what a vast source of information is given by the face, not with respect to affections of that particular part, but with respect to all other parts of the body.

Speaking, however, of the general expression of the face, I must mention that you learn a great deal of the patient's feelings by the attitude of his body. In fever nothing is a more unfavourable omen than for a patient to be seen lying on his side at first, and gradually getting on his back till he lies altogether on his back; but when he sinks also in his bed, the case becomes still more alarming. The reason is simply this—more muscular strength is required in order to lie on the side than on the back, and to remain in a firm posture than to sink down; and therefore, when a patient sinks down, it is an indication that his strength is nearly exhausted. You discover a great deal, too, by the restlessness of the whole body. One of the most unfavourable symptoms in many diseases is extreme restlessness. You may have restlessness in women from mere fidgets; but in acute diseases, when great debility has come on, restlessness is often one of the fatal symptoms.

Of course, if disease take place in the face locally, it will shew itself there as in other parts of the frame, whether it be swelling, mortification, local cutaneous disease, or whatever else happens to it.

But again, to go to the *hand*, the part which, together with the face, in all civilized and uncivilized countries is most exposed. In the hand we have also very great indications, though certainly they are not so strong as in the face. The hand is one of the extreme parts, and shews the first fall of temperature. It is one of the first parts that be-

come cold, exactly as the nose and ears do in the head. I believe the difference between its temperature in fever and in health is often greater than the difference of the temperature of most other parts of the body. A cold sweat is shewn in the hand amongst the earliest parts, and the very hectic fever which flushes the face flushes also the palm of the hand, and indeed the sole of the foot; but it is shewn particularly in the palm of the hand, from the greater fineness of the integuments. Difficulty of breathing again, discovers itself by the appearance of the nails. If you observe in the difficulty of breathing a purple hue of the lips, a fullness and a leaden colour of the face, and a purplish colour of the eyes, you discover likewise a purple hue of the nails, at least the parts under the nails. As I mentioned, in jaundice, that the disease appears first at the roots of the nails, as well as in the sclerótica, and after the disease has disappeared from the rest of the surface, you will discover it still lingering in the system by the yellowness observed in the same two parts, so I believe the changes go on, *pari passu*, in dyspnœa with respect to the eyes and nails. The dryness of the hand, its mordent heat, its clamminess, its firmness and softness, the very grasp of the patient, are all significant. If phthisis is often seen in the eye, the ends of the fingers are no less indicative of it or other internal scrofula.

Besides all this, it is in the hand that we easily make the most important conceivable observations upon the state of the circulation. It is in the hand, or at least at the wrist, that we feel the pulse much more easily than at any other part of the body. The same observation might be made any where else; but it would be done far less accurately, and with far less ease, than at the wrist. With respect to all other observations, about temperature, about heat, about cold, about clamminess, and about sweats, I think they are not only all made much more accurately in the hand than any other part, but the hand appears to be affected by these particular symptoms more altogether than most other parts of the surface of the body.

In regard, however, to the *pulse*, great indications of disease are taken from it. You will find the pulse generally spoken of in the first place, as great or small—*pulsus magnus*, *pulsus parvus*; you find it mentioned likewise as strong or weak—*pulsus validus*, *pulsus debilis*; and as hard or soft—*durus*, *mollis*. A pulse may be large, may be considerable, and yet not hard; a pulse also may be hard, and yet by no means large,—on the contrary, it may be exceedingly small. A pulse is not necessarily strong because it is large; neither is it necessarily weak because it is small in volume. If a pulse be both hard and large, it is a strong pulse also; if a pulse be small and soft together, then it must be considered

as weak; and mere softness may be such as to amount to weakness; and if it be exceedingly weak, you may extinguish it by the least pressure of the finger. This pulse has been called *pulsus vermicularis*, and among us *fluttering*, if also quick. It is a pulse easily recognized in practice, and indicates death to be at hand in many cases. If a pulse be exceedingly hard, and at the same time small, then it has been called a *wiry* pulse. A wire is necessarily fine and hard, and a pulse which has the same attribute is called a *wiry* pulse. This is different from a *thready* pulse, because thread is different from wire. A *thready* pulse is merely that which is small, soft, and weak—a *vermicular* motion—hardly worthy of the name of a pulsation. It is chiefly in violent inflammations of the abdomen that you have the *wiry* pulse; however, you must not depend upon the pulse only in such cases. If there be other symptoms of inflammation of the stomach, or the intestines, or the peritoneum, you must not say that is not the disease because the pulse is not *wiry*. These are the qualities of the pulse with respect to its volume, solidity, and force,—large or small, hard or soft, strong or weak.

But there are other varieties of pulse, taken from its *rate*. A pulse may be quick, and a pulse may be slow; there is the *pulsus frequens*, and the *pulsus rarus*. The pulse will sometimes go up to 160; sometimes it will go beyond that, so that you cannot count it, especially if weak; but then you can ascertain the state of the circulation by the heart, where you can count 200 and upwards with perfect ease, when you cannot count the pulse at the wrist. You find a distinction made with regard to a frequent pulse—you find a *pulsus frequens* and a *pulsus celer*; *frequens* is where there is a large number of strokes in a given time, and *celer* is where the strokes take place in a short and abrupt manner. *Pulsus frequens* and *pulsus rarus* are opposite to each other; the former being a rapid, and the latter a slow pulse. The opposite of the *pulsus celer* is the *pulsus tardus*, in which, whatever number of beats take place, they occur in a gradual manner. In England practitioners call this pulse a *jerk*ing pulse: *jerk*ing and *sharp* are our terms corresponding to *celer* in regard to the pulse. No matter whether it is quick or not, we say it is a *sharp* or *jerk*ing pulse; whereas, on the other hand, that which takes place as it were deliberately is called *pulsus tardus*.

I may mention the great importance of feeling the pulse at both wrists. If you feel the pulse at the left wrist only, you may often think that the patient, from extreme debility, cannot bear bleeding; but if you feel it at the right also, you will frequently form a very different opinion. Nothing is more common than for the pulse at the two wrists to vary exceedingly; and it is the pulse at

the right wrist that, in the greater number of cases, is the correct guide. The difference is so great in so large a number of cases, that it should be a general rule to observe both pulses. I never myself have seen the pulses different in point of time, but there is a great difference in respect to force. It is also certainly right, in cases of great nicety, where you are in doubt as to the means you are to use, to examine likewise at the heart; and in cases of local disease of that organ, it is also right to examine the heart itself; for in some diseases you will have simply, from an affection of the heart, a very violent pulse, such as would lead you to bleed to a great extent; and in those cases, the pulse is not an indication of the general strength of the patient, but receives its character solely from the disease of the heart. On the other hand you will, in some diseases of the heart itself, find a very small pulse, and on examining the heart you hear it beat violently, but through a diminution in the aperture the blood escapes in small quantity. It is right to examine not only both wrists where there is great nicety required, but I also advise you to listen to the heart.

I should likewise mention with respect to the pulse, that its rate, size, and force, are sometimes irregular; it will vary in its strength and size at different beats, and it will also vary in its frequency. You will sometimes have a pulse merely irregular, beating always with nearly the same force, but beating at an irregular rate, and that is an irregular pulse in the common acceptation of the word; but besides that, you frequently find it irregular in force, size, and frequency, so that it will give a great thump or two, and then you have a number of strokes of no strength at all, and then again it will beat violently. Irregularity, therefore, you must remember, does not refer to frequency only, but likewise to force. Sometimes the pulse intermits.

You must also of course remember, as physiologists, that the pulse differs in every age; that the younger we are, the more quick is the pulse; that the pulse of the female is quicker than that of the male; and that, generally, the inhabitants of a warm climate have a weaker pulse than those in a more temperate latitude. It is necessary to take all these things into consideration, because, if you were to find the pulse of a child 120, and that of an adult labouring under fever likewise 120, you would commit a great error in supposing that the child likewise laboured under fever. It is necessary, therefore, to remember that the pulse varies according to age, sex, and climate, and that in young children it is particularly quick. I had an instance last week of the great use of examining a patient in bed, and not being contented without what is contemptuously called a *mechanical* exami-

nation, in an enlargement of the abdomen. I was called to visit a lady in the family way, about thirty years of age, whose pulse was about 80 or 90, but on listening to the abdomen—she being single, and having some disease, as she said—on listening to the abdomen, there was another pulsation about 123. The pulsation did not arise from any of the branches of the iliac arteries; that was impossible; for if they had beaten, it would have been at the same rate as the pulsation at the wrists. She had a pulse of 80; something within her had a pulse of 123; and what that was, I left her to settle by herself. Being a single lady, all that I could say was, that if she waited patiently, the whole of the disease would come away to a certainty in two or three months.

ON THE USE

OF

PURGATIVE MEDICINES IN INFLAMMATORY AFFECTIONS OF THE STOMACH AND BOWELS.

To the Editor of the London Medical Gazette.

SIR,

THERE is scarcely any probability that medicine will ever become quite free from the imputation of being a "conjectural art." The human frame is liable to be affected by so many agents, both physical and moral; the functions of its several organs are performed in a manner so complicated and mysterious, that, when any morbid change takes place in it, it is often extremely difficult, if not impossible, to discover either the cause or the nature of that change. But, in addition to this grand obstacle in the way of medical science, there are other subordinate ones, with which it is of more importance to be acquainted than with even the primary one itself. For this last, originating in the nature of the subjects which the science embraces, is inseparable from it; while the others, being purely accidental, admit of being removed.

Among the most powerful of these accidental impediments may be reckoned the readiness with which the generality of those who cultivate medicine submit to be influenced by the example and the authority of their predecessors. For thus their thoughts, starting from the same point, and running in exactly

the same line, reach, of necessity, the same conclusion; and by this repeated iteration the track is, in process of time, worn into so deep a groove that none but an intellect of extraordinary powers can effect a deviation from it. The consequence of this is, that when once an error is adopted by a few persons of reputation, or even by a single person of marked celebrity, the great body of the profession is drawn after, and a long time must elapse before that error can be got rid of. For it is not often that we meet in any scientific pursuit with a mind possessing that unusual degree of vigour, without which it is impossible both to resist the attraction of multiplied examples, and at the same time to overcome the obstacles that present themselves at the very entrance of a hitherto untrodden path. But let a new track be once struck out, and such is the fondness for novelty, that though it wander from the way to truth far more widely than the old one, yet forsaking this latter, almost the whole crowd will press forwards upon it. Thus it is, that both in the theory and in the practice of medicine, improvement is retarded by a slavish submission to authority on the one hand, and by an inconsiderate attachment to novelty on the other, qualities which, however opposite to each other they appear, are here, at least, closely united in their origin.

The truth of the above observations will, I think, be rendered sufficiently evident by a reference to that particular measure in the practice of medicine of which I am about to treat, namely, the use of purgative medicines. It is only a very few years since these were thought an essential part in the treatment of almost every disease. "The alimentary canal," it was said, "being the place in which the process of nutrition commences, it is absolutely necessary for the well-being of the whole system, that its functions be performed in a regular and healthy manner." Again, "the stomach and bowels, but particularly the former, are connected with every other part of the body by a sympathy so close and so powerful, that when a disease occurs, be its seat where it may, they cannot escape being more or less implicated in it." Now these arguments are just and reasonable; and the practice founded on them, restricted within due limits, was experienced

to be highly beneficial. But not satisfied with this, one or two persons, of considerable eminence in the profession, proceeded much further. According to them, not only is the alimentary canal affected in almost every disease, but it is from some derangement in its functions that most diseases originate. Entertaining this opinion, they regarded attention to the state of the stomach and bowels as the main, if not the only, curative measure indicated, no matter what was the nature of the affection, or where it was seated; and thus they framed a system of therapeutics which had, at least, the merit of being pre-eminently simple.

But this very circumstance ought to have rendered the profession wary in receiving it; for a remedy so simple and so general, implies simplicity and uniformity of derangement, a thing scarcely to be expected in a machine so complicated as the human frame is, both in its structure and its operations. Such, indeed, was the opinion respecting it entertained by many reflecting minds; but by the great majority of the public, both professional and otherwise, it was received, cherished, and maintained, with a zeal bordering upon enthusiasm. Every fact that militated against it was treated with contempt, or entirely overlooked; while all those which seemed to lend it support were brought forward, insisted on, and magnified in importance. Did you hear a physician belonging to this sect interrogate a patient? All his questions related to the state of the secretions, the frequency, the quantity, and the quality of the stools. The answers of the patient went, perhaps, to shew that here all was healthy and natural; but this was quite disregarded by the physician. He felt quite certain, whatever appearances to the contrary existed, that the stomach and bowels were the theatre of the first movements of the disease; or that they had become, at least, the head-quarters of its concentrated forces. To these parts, therefore, his operations were entirely directed. Blue-pill, calomel, jalap, and black-draught, were the instruments with which he worked, and he used them without much regard to moderation. Even of those who ridiculed the theory many soon came, almost insensibly to themselves, to adopt to its fullest extent the practice. Thus were all the

distinctive marks of diseases broken down; or if any were allowed to remain, it was only to divide them into two classes, those which admitted of violent purging, and those in which aperients only ought to be used. In this manner affections of the most opposite nature were treated with precisely the same remedies; and practitioners, so far from doubting the reasonableness of such a proceeding, prided themselves on their ingenuity in discovering, as they thought, the seat of the disease, when not a single symptom pointed to it.

Such was the method of practice which but a little time since prevailed throughout the profession. Now that the mania which established and extended it has passed away, we easily behold its extravagance; and wonder how, in opposition to reason and the evidence of well-known facts, it ever came to be so generally adopted. And yet if we examine attentively the doctrines respecting this part of medical practice, which are at the present day even more generally received and acted on, we shall, if I mistake not, find them to be still less consonant with reason and with truth.

It is a fact, unquestioned by the supporters, either of the reigning or of the deposed doctrine, that in a great proportion of the diseases to which we are subject, the mucous membrane of the intestines is more or less affected. The controverted point, then, is respecting the treatment of such cases; and here, if it be proved that the tenets of Hamilton, Abernethy, and their disciples, though confessedly extravagant, are nevertheless not only more consistent with reason and analogy, but are actually found to be more beneficial in practice than those of Broussais and the French school, it will scarcely, I think, be disputed that in adopting the latter we betray greater weakness of judgment than those did who put such implicit faith in the former.

It cannot have escaped the notice of any one who has been in the habit of reading the reports of cases treated in the French hospitals, that since the "physiological doctrine," as it is called, has come to be generally acted on, there have occurred a far greater number of instances in which ulceration of some part of the digestive canal has been the only, or the principal morbid appearance after death, than an equal number of similar cases ordinarily

present in the hospitals of this country, where such an extreme dread of purgative medicines has not yet seized upon the minds of the profession. Now it seems to me that this fact alone is decisive of the question; for I am not aware that there exists any other cause adequate to account for the difference; and, indeed, if we only reflect that in mucous membranes ulceration is a very frequent consequence of unchecked inflammation, and then consider the extremely inefficient method of treatment practised by the Broussain school, we will find it, I think, unnecessary to search after a different cause than this. What, for instance, can their lavements, their diluents, and their ptisans avail when violent inflammatory action has been established in the stomach or in the bowels? These measures, it is true, are aided by leeching and a pretty rigid system of abstinence; but, if I am not greatly mistaken, the application to the external surface of the abdomen of even a larger number of leeches than they usually have recourse to, cannot have a very powerful influence over the circulation in the internal parts; and the flame of inflammation, when once lighted up, does not require constant supplies of fresh food to maintain its existence; the system itself is almost always sufficiently well stored to furnish it with an adequate quantity of fuel. In what I here say, it is far from my intention to contend that abstinence, and the application of leeches, are measures altogether powerless. On the contrary, I am well aware that they are often very beneficial, but then they should be used merely as auxiliaries to other more efficient remedies, and not be trusted to alone, or in conjunction with means infinitely less active than themselves. I am not ignorant that, to the generality of persons, the application of thirty or forty leeches appears a very active kind of treatment; but in the particular affections of which we are treating, it exerts, in my opinion, by no means so powerful an influence. The grounds upon which I have formed this opinion are the following:—

1. The muscles and integuments of the abdomen, to which of course the application of the leeches is made, are principally supplied with blood by vessels which originate at a considerable distance from those furnished to the in-

testines, and which have very little connexion with them.

2. The slowness with which the blood moves through the intestinal capillaries, as well as through the portal system generally, and its tendency to congestion there, render them less easily affected by any diminution in the circulation than other parts much more remote from the place where the blood is drawn, and much less intimately connected with it.

This last objection, it may be urged, equally applies to the abstraction of blood generally, and I am ready to concede that it probably does. Where, then, are we to look for a remedy capable of achieving what neither of these measures, nor even both of them together, can properly effect? Before answering this question, which I expect to be able to do in the most satisfactory manner, I may remark here, that even in cases where the inflammation was of the most acute form, I have seen the application of a large blister succeed in checking it after blood-letting, both general and local, had been tried to no purpose. I can scarcely pretend to explain why it is that blisters should here prove so pre-eminently useful; but it may, in some measure at least, proceed from their rousing the capillaries of the intestines, and thus effecting a re-establishment of the suspended secretions. It is most probable, however, that had they been applied before the removal from the system of a proper quantity of blood, their effects would not have been so beneficial.

Having made these observations upon a practical fact, which is not, I think, in general sufficiently attended to, I proceed to state, in reply to the question above proposed, the means by which we have it in our power to effect what blood-letting, both general and local, fails in accomplishing. These means are simply the use of purgative medicines, by which alone we can hope effectually to relieve the overloaded intestinal vessels. Nor is there, as far as I am able to judge, the slightest cause for apprehending an increase of inflammatory action as the result of their operation. Unnatural vascular distention, if it do not itself constitute the inflammation, is at least a very prominent feature of it; and I have already shewn, that it is only by the use of purgative

medicines that that distention can be thoroughly removed. Indeed, the manner in which nature herself not unfrequently succeeds in terminating the disease by an increased secretion from the inflamed parts, indicates clearly enough the propriety of attempting, by similar means, to attain the same object. To this argument it may be objected, that there is a wide difference between the process by which this increase of the secretions is produced naturally, and that by which it is effected when artificial means are employed. But in this objection there is stated, as a positive fact, what is nothing more than conjecture, and conjecture, too, without any probable foundation in truth, for there is not the slightest reason to suspect that purgatives operate by exciting an action different from the natural one. They do nothing more than cause an increase of the natural action; and this is the identical process by which the disease is often spontaneously terminated. I am not here opposing mere assertion to assertion, for the effects in both cases are strikingly alike; and similarity of effect is a pretty sure proof that the causes are not very different.

I am aware that artificial purgation, to whatever extent it may be carried, will not, in many cases, succeed in arresting the progress of the disease; but neither does spontaneous purging invariably effect the same end; so that this circumstance merely goes to prove that vascular distention does not alone constitute the disease; but that, in addition to it, there is some other morbid affection (probably of the nerves of the part) which may continue to exist after the distention has been removed. But after all this, it may still be urged that active purgative medicines, though they produce merely an increase of the natural action, cannot but irritate the inflamed intestinal surface; and that such irritation must, of necessity, aggravate the affection. Now this is a question which could never be satisfactorily decided by mere theoretical reasoning, for scarcely an argument could be advanced on one side that might not be met with an antagonist one from the other. Let us look, then, to the facts furnished by experience, and see to what conclusion their evidence will lead us.

If it were my purpose to make a general attack on the doctrine of Brous-

sais, I could easily convict it of extravagance in supposing the presence of intestinal and gastric inflammation in numerous cases where no such inflammation really exists. Often and often have I seen tenderness over the stomach and abdominal region assumed as an undoubted proof that inflammatory action was going on in those parts when I had the utmost possible certainty that the tenderness was the result solely of the pressing and the pommelling which had been practised at a previous examination. But it is confessed, even by the most ardent supporters of the doctrine, that in many cases where they contend for the presence of inflammation, no sensible signs of its existence are afforded; while, at the same time, they cannot deny, that even in rude health, when the digestive functions are performed in the healthiest and most regular manner, the slightest pressure over the stomach will often be followed by a very keen sensation of soreness. These are facts which it is much easier to wink at than to meet with a successful opposition.

But the object which I hold in view is, to prove that even in those cases in which inflammation of the alimentary canal does really exist, the use of purgative medicines not only is not pernicious, but is actually productive of better effects than any other measure that we can have recourse to. And here I shall adduce a single fact, which, if the above observation be not strictly true, appears to me utterly inexplicable. In *enteritis*, attended with constipation, the general practice, at least in this country, is, after a due abstraction of blood, to throw in the strongest purgative medicines, almost without any regard to quantity, till several evacuations have been procured. And so far is the operation of these medicines from proving pernicious, that until it has taken place to a considerable extent, the patient cannot be considered free from danger. If it be objected to me, that in such a case as that to which I have alluded, there are hardened feces, which act as a mechanical obstruction, irritating the parts, and that the utility of purgatives consists in their procuring the expulsion of these, I answer that the objection does not very much affect my argument; for it is inflammation that constitutes the disease, of which the constipation is, in general, but a mere

symptom; yet we find that copious purging is the most effectual remedy for it, the principal use of the different other measures, blood-letting itself not excepted, consisting in their disposing the intestines to be more easily acted on by the purgative medicines. But even though I were to concede that in every case of enteritis the obstruction precedes the inflammation, and is the cause of it, my argument would still, nevertheless, hold good; for in *enteritis*, inflammation, by whatever cause excited and maintained, must be allowed to exist; and yet it is not aggravated by purgatives, but, on the contrary, is more relieved by them than by any other remedies. But it has been contended, that in *enteritis*, attended with constipation, the disease is entirely, or for the most part, seated in the serous membrane; so that in it purgatives cannot by contact irritate the inflamed surface. Now if it were true, that in this disease the serous membrane is exclusively the seat of the inflammation, the above reasoning might, perhaps, be admitted; but as morbid anatomy proves the case to be otherwise, its rejection ought not, of course, to cost us a moment's hesitation.

Another argument, proving unanswerably the utility of purgative medicines in inflammatory affections of the bowels, may be derived from the benefit following their use after the operation for strangulated hernia. It is the practice with most experienced surgeons to get the bowels thoroughly opened as soon as possible after the intestine has been reduced; and when this has been once effected, the danger of the patient is considered to be greatly diminished. This is a fact which will scarcely, I think, be disputed; and, as in all cases where the strangulation has existed even for a few hours, there must necessarily be inflammation, I cannot possibly see how the Broussais could attempt to explain why that inflammation is not aggravated by the purgatives.

The utility of large doses of calomel in the various dysenteric affections, so common in tropical climates, is rendered unquestionable by the concurrent testimony of Johnson, Annesley, and a whole host of other witnesses, many of them scarcely less eminent than these for closeness of observation and correctness of judgment. Now that these affections are of an inflammatory na-

ture; that, in fact, they consist essentially in indammation of the mucous membrane of the alimentary canal, scarcely admits of a doubt. How, then, does it happen, that in them calomel, so irritating in its nature, so drastic in its operation, and therefore so peculiarly the horror of Broussais and his disciples, proves beyond all comparison the most useful remedy with which we are acquainted? Whence, too, is it, that within certain limits, its good effects are more and more conspicuous in proportion as a larger dose of it is given. According to the "physiological doctrine," every increase in the quantity administered ought to produce a proportionate increase in the severity of the symptoms. The very reverse, however, of this is, as has been already observed, what really takes place. Small doses, by augmenting vascular action, without carrying it to such a height as to produce copious secretion, only torture the disease, which, out of resentment, as it were, for the injury done to it, always assumes additional violence, and not unfrequently succeeds in demolishing the fabric which contains it; while a single large dose is often sufficient to strangle it on the instant.

In the catarrhal affection which prevailed so generally throughout the country a short time since, I had numerous opportunities of proving the truth of the opinion for which I am here contending. The gastro-pulmonary mucous membrane, including, of course, that which lines the nostrils, mouth, and fauces, was the part principally affected. In general the symptoms were not very severe; yet there were some cases in which there prevailed excessive vomiting and purging, with violent internal pain, as well as external tenderness. Here, then, were symptoms the most unequivocal of inflammation—and inflammation, too, of rather an acute nature; and yet the cases in which recovery took place soonest were those that had been treated, from the commencement, with extensive purgation. Six or eight grains of blue pill at bed-time, with a drachm of compound jalap powder on the following morning, universally succeeded in ameliorating the symptoms. Repeated once or twice, with merely an antimonial powder, or draught, given occasionally in the intervals, they seldom failed in effecting a cure. In general, half a grain or a grain of opium

was administered with the pill, both to ease the pain and to pacify the stomach, which was irritable and averse to retaining the medicines.

Contrasted with the success of this method of treatment, that of the Broussaian plan, which was tried in a few similar cases, shewed poor in the extreme. In these cases, the purging and vomiting, together with the pain and sickness which accompanied them, instead of being soon considerably mitigated, or of ceasing entirely, as usually happened after the operation of the purgative medicines, continued day after day, exhausting the patient, till at length nature succeeded in effecting, by a copious secretion from the intestines, what the exhibition of one or two smart purgatives might have easily accomplished on the first or second day after the attack. It is scarcely necessary for me to add, that when the disease was thus allowed to run its course, it left behind it a degree of debility, particularly in the digestive organs, which was not recovered from till after the lapse of a considerable period of time.

JOHN M'DIVITT.

Kegworth, Oct. 24, 1831.

BOTANY OF NEW ZEALAND;

BEING A

Description of Trees, Plants, &c. indigenous to that country.

By GEORGE BENNETT, Esq. M. R. C. S. &c.

[With Engravings.]

THE vegetable productions of New Zealand are very numerous. Among the lofty timber trees which adorn the New Zealand forests, the "Pines" are the most conspicuous, from their stately, erect, and elegant growth: in the form of plank and spar, they are an article of commerce between that country and the colonies of New South Wales and Van Diemen's Land, and profitable voyages have been made with a selected assortment of spars even to England. I am acquainted with six New Zealand pines, and have specimens of them in my Botanical collection: they bear the native appellations of Kowri, Remu, Tánakáá, Káwaka, Tótara, and Kái-

kea-tea, the whole of which are found growing on elevated land and in good soil.

The timber produced from the New Zealand pines varies in quality: the Kowri is generally preferred for the yards and masts of ships, as well as for other purposes; the Tánakáá is much heavier than the Kowri, sinks in the water, and is not very durable. It has been stated that the spars brought from New Zealand have been found on trial to be of equal gravity with Riga spars, and to possess a greater degree of flexibility than the very best species of fir procured from the north. The wood of the Kowri is much finer grained than any timber of the pine tribe; and the trunks are of such a size as to serve for the main and foretop masts of the largest three-deckers. The Prince Regent, of 120 guns, is supplied with them: they have also been used in sea-going ships, and the reports of their qualities are most favourable*.

Among other timber trees are the Knightea excelsa, with its clusters of beautiful crimson flowers; the Edwardsia microphylla, with its pendant golden blossoms; the Puredi (Ephialis Sp.), Pohu-tukawa (Metrosideros Sp.) &c. &c. Ferns abound in the New Zealand forests, among which may be recognized the genera Polypodium, Trichomanes, Lycopodium, Cyathea, Asplenium, &c.; the beautiful Todea pellucida of Brown, Trichomanes reniforme, Lindsia trichomanoides, &c.; and on the declivities of the hills, a beautiful species of Astelia, flowering in May, June, and July, is found very abundant. A great variety of the mosses and fungi are found also in the New Zealand forests, the latter attaining very large dimensions.

Dammara Australis, LAMBERT.

Dammara RUMPHIUS.

Agathis SALISBURY.

Kowri of the natives of NEW ZEALAND.

Natural Family, *Coniferae*.

This magnificent tree is an ornament to the New Zealand forests. It is the *Agathis* of Salisbury, the *Dammara Australis* of Lambert, and is named

* Quarterly Review, vol. xxxi. p. 64, Dec. 1824.

Kowri by the natives of New Zealand, which name, it is said, was given on account of the great diameter it attains. It grows very erect, and attains the height of from eighty to ninety feet, and in diameter is considered the largest tree in New Zealand. It is said to attain twenty-four feet in circumference, and I have seen several cut down measuring from sixteen to twenty feet. The timber is of an excellent quality, close grained, durable, and valuable for a variety of purposes, either in plank or spar: as yards and masts for shipping, they have been found, by repeated trials, superior to all others, both in their flexibility as well as durability. The wood is of a white colour.

The leaves, when young, are alternate, but in form lanceolate, which change, as the tree increases in growth, to an elliptical or oblong form.

This tree yields a great quantity of resin*, of a white or amber colour, very transparent, burning with a bright flame, and having a very agreeable smell. It exudes spontaneously and concretes on the trunk, but an incision being made in the tree and left for a night, on the following morning a large quantity is found to have exuded, which continues still to exude from the incision for a long time afterwards. This tree yields the largest quantity of resin of all the New Zealand resiniferous trees. It resembles the resin named Dammar in the East Indies, and is, indeed, produced from a tree of the same genus, and might consequently be employed for similar purposes for which that resin is used in India—as a pitch for ships, &c. &c.; and might form an article of commerce from New Zealand to the colonies of New South Wales, Van Dieman's Land, &c.; and, from the quantity I have seen exude from one tree, it may be collected abundantly in a short period of time. The natives name the resin the Wai (Wy), or water of the tree.

The pigment, or soot (Ngarahu), which is the colouring substance used by them in the operation of tatauing, is usually prepared from this species of pine, from its possessing more resin than any of the others. The manner of

preparing this soot among the New Zealanders is as follows:—

An oven is formed of stones, in which a fire is made of the wood of the pines, but more usually of the Kowri pine; from this a soot collects on the roof, and after sufficient quantity has been collected the fire is extinguished, and a mat being placed at the bottom of the oven, the soot is scraped off the upper part and collected in the mat placed underneath. It is then placed into a vessel, and sufficient water added to form it into lumps; in which state it is preserved until required for use. The soot yielded by this process is of a fine black colour, and a gum resin (damuh) is also used by the Javanese, which, being burnt and the soot collected, is used by them in the manufacture of their ink, which is beautifully soft and black, and is said probably to equal, in most respects, that of the Chinese. Our process of obtaining a lamp-black* is not far different from that adopted by the savages of New Zealand, and it is the resiniferous among trees that yields the most beautiful lamp-black.

When I inquired of a New Zealander, why they did not use any other kind of tree, and why particular pines were preferred? his reply was, because the pines contain more Wai (Wy), or Resin; and the Kowri was preferred because it contained more than any other species. When Resins burn they yield a quantity of smoke, from which much soot is deposited; this was observed by the savage, and adopted as an opportunity of acquiring a pigment for adorning his person. Among savages I have invariably found a very intimate knowledge to exist of trees, plants, &c. and the various uses to which they were capable of being applied†. The natives of New Zealand

* Thus mentioned by Dr. Maton, in Lambert's work on the Genus Pinus. "A sort of box is made, nicely closed in every part, with the exception of some holes on the top, which are covered with a sort of linen cone. At a little distance from the box a furnace is constructed, with a very small mouth, and the inferior part communicating with the inside of the box by an horizontal chimney. Into this furnace are put the dregs and coarser parts left in the preparation of tar; and in proportion to the consumption of these a supply is kept up, so as to furnish a constant draught of smoke into the box. The smoke goes chiefly into the cone, when it deposits its soot, or lamp-black, which is employed almost exclusively in printing and dying."

† The black pigment used for tatauing at Tahiti, is procured from the nuts of the Tiari, or Candle nut-tree (*Aleurites triloba*), and also at Tongatabu

* Vae is the term used in the New Zealand language for the Gum-resin; it also signifies the gum which exudes from the eyes. Wai (Wy), or water of the tree, is also used.

also collect the resin of the Kowri tree and use it as a masticatory, similar to a curious bituminous substance, a kind of Asphaltum (with a smell of Naptha), named Mimiha*.

There is a magnificent coloured engraving of this tree in Lambert's splendid work on the Genus Pinus; but it is to be regretted that the expensive style in which that work is published renders it almost inaccessible to the botanist, and consequently renders it less useful to science.

Knightea excelsa—BROWN, Lin. Trans. vol. x. 1811.

Riwa-riwa of the natives of NEW ZEALAND.

Natural Family, *Proteaceae*.

This tree is abundant in the New Zealand forests, and is named Riwa-riwa by the natives; it is more usually found growing on the declivities of hills. It is a handsome tree, and attains the height of from sixty to seventy feet, but seldom exceeds more than seven or eight feet in circumference. The trees that came under my observation were invariably straight in their growth. The timber is of an excellent quality, but, on account of its small diameter, is not cut into plank, but is used for making planes, &c. by Europeans. The flowers are of a beautiful crimson colour, and the capsules are of a dark brown colour, growing in bunches of from twenty to thirty, each capsule containing from four to six winged seeds. I collected several specimens of this tree (with the capsules in a mature state) at Wyshaki Cove, River Thames, New Zealand, in June 1829. A branch of this tree is figured in the Linnæan Transactions, vol. x. 1811, both in flower and fruit, but the mature seed has not been delineated.

THE HOROEKA TREE OF NEW ZEALAND.

This tree, curious in form and its mode of growth, has not yet been ob-

served by botanists either in flower or fructification. During a diligent search during the months of April, May, June, and part of July (1829), although I collected nearly a hundred specimens (thirty of which I brought to England), I was unable to procure it in flower or fruit. It is found growing usually in shady situations, on elevated land, as well as in vallies, and in good soil: it attains the height of from twenty to forty feet. In the very young trees the leaves are scattered; but in those which have attained more maturity and elevation, the leaves become tufted. No branches are thrown out, unless a short stalk, from which the tufts of foliage arise, can be so named. The leaves are about a foot in length, and an inch in breadth, and are irregular at their edges, and abrupt at their terminations, of a dark green colour above, the centre stalk being of an orange colour, and underneath the leaves have a brownish-red tinge.

In young as well as in more mature specimens, I have observed a change in the form of the leaf, on some the whole of the foliage being *ternate*, on others some of the leaves only being *ternate*, and others being of the usual form; others again have *ternate*, *biuate*, and *single* leaves, on the same tree. This anomaly was more usually observed on young specimens than in mature trees.

The leaves on the summits of those trees which had attained a great elevation, became of increased breadth and decreased length; but this was not confined to old trees, young ones often exhibited similar anomalies in their foliage*. Some of the trees grow very erect, others bend towards the ground.

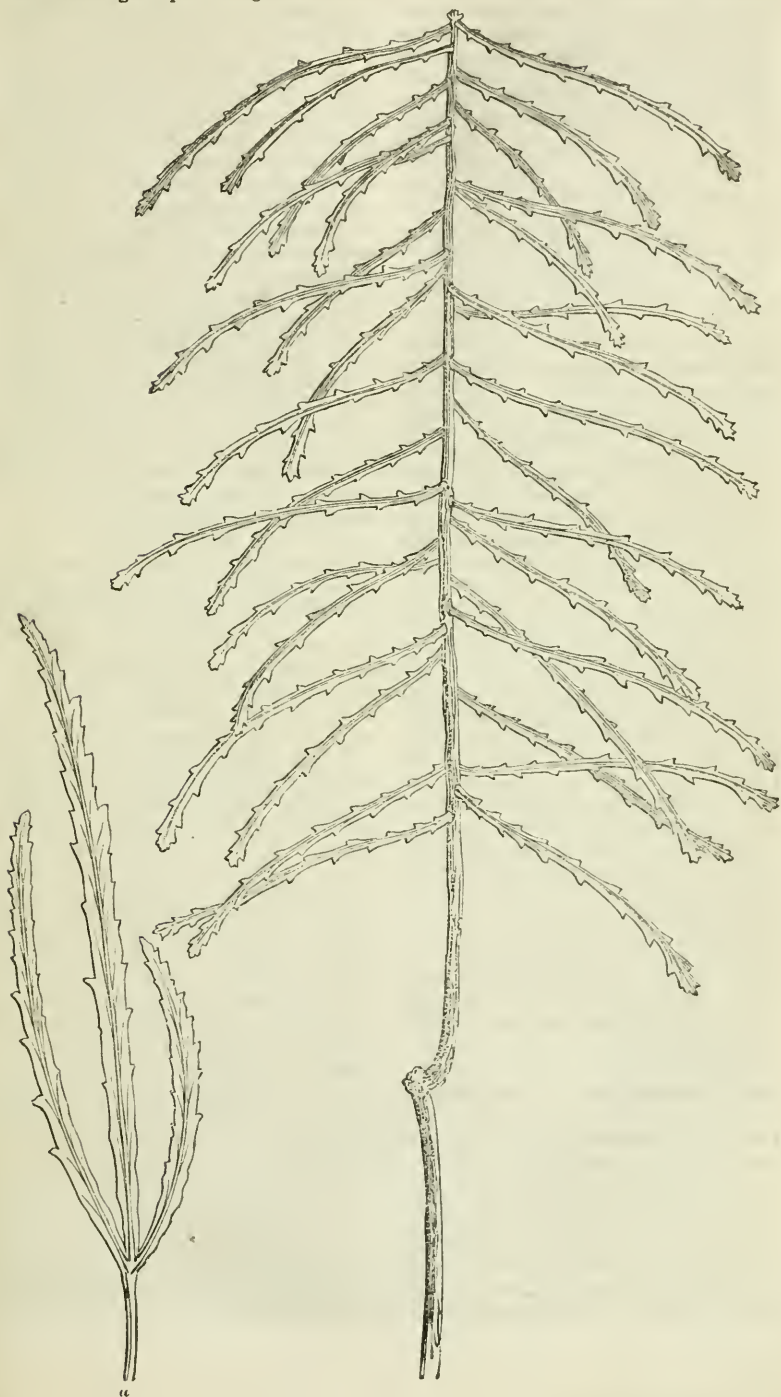
Some of the trunks of trees that I measured were only a foot to a foot and a half in circumference at the base, although in height they were thirty feet and upwards. The wood is very close-grained and flexible; and some Europeans residing at New Zealand informed me that they used it in boat-building. The accompanying sketch, taken from a recent but young specimen, will show the peculiar form of the leaves; (a) the *ternate* leaf.

from the nuts of the same tree, where it is named Tul tul. At the Island of Rötuma, from the nuts of the Fifiu (*Calophyllum inophyllum*). All of these nuts contain oil. The nuts are first baked to a charcoal; afterwards pulverized, and mixed with oil.

* An account of which was published in the Medical Gazette of June 4, 1831, vol. viii. p. 392.

* It was at Wyshaki, river Thames, that the principal opportunities were afforded me of observing the growth and changes of foliage in this tree. Although I observed numerous specimens in a valley near the watering place at Kororadeka, Bay of Islands, I did not observe the anomalies in the foliage at that place.

Since writing the preceding I have been informed that a specimen in fructifica-



tion has been procured at New Zealand by one of the missionaries, and sent to Mr. Cunningham at Sydney, who has since brought it to England; we may therefore expect a description of this curious tree from that able botanist, and learn to what natural family it belongs.

OBSERVATIONS ON HERNIA.

To the Editor of the London Medical Gazette.

54, Stamford-Street, Oct. 24th, 1831.

SIR,

IN the Medical Gazette for February 19th last, there are some observations on hernia by Mr. Lizars, in which he has done me the honour of quoting my opinions; but as he appears to me to have misconceived the real nature of those opinions, I trust that it will not be deemed otherwise than an act of courtesy to him and your readers, to communicate (through the medium of your journal) a further explanation of my views upon the subject. I should have done so earlier, but other matters diverted me from the purpose.

Mr. Lizars commences by testifying to the difficulty in deciding upon the propriety of operating, and to the difficulty of diagnosis and prognosis, in cases of hernia. His words are, "there are four species of hernia which, in my opinion, demand an operation, viz. the acute or inflammatory strangulated, the slow or chronic strangulated, the incarcerated hernia, and the obstructed hernia." With due deference to Mr. Lizars, I conceive the whole difficulty in understanding the nature of hernia arises from the multiplicity of names used by surgical writers without any adequate or proper idea attached to them. I believe that if the terms, strangulated hernia, chronic strangulation, incarceration, engouement, &c. were abandoned, it would conduce to a better understanding of the subject. For example, all the symptoms which are said to denote a strangulated hernia, may exist without any strangulation, or even without any stricture whatever; such symptoms being common to all mechanical obstructions of the bowels, whether from adhesion of the intestine, intussusception, or to a contraction of the calibre of the canal, and by no means peculiar to strangulation. The

term slow, or chronic strangulation, only applies to a state of constriction *not amounting to strangulation*, and is therefore incorrect. Incarceration conveys no idea beyond that of irreducible hernia: it does not express the morbid condition (obstruction) to which an irreducible hernia is subject. Engouement also is but the name for an effect: it does not express the cause of the disorder. Whence arises the engouement, or accumulation, but as an effect of some cause impeding the action and passage of the bowels? Engouement is, therefore, the *effect* of obstruction.

We should consider hernia first in its simple state, hernia meaning a simple protrusion; second, in its obstructed state, obstructed hernia meaning a hernia affected by some cause obstructing the function and passage of the intestinal tube. If the cause is stricture, the symptoms will be more or less acute and violent, depending upon the greater or less degree of constriction, and this may very properly be called acute obstructed hernia. If the cause is a confinement of the intestine in an unfavourable position, *without any stricture*, whether from adhesion, or from the intestines being in too narrow a space to carry on their peristaltic action, or transmit their contents, the symptoms, although the same in kind, will be more mild and protracted, and this will be properly defined by the term subacute obstructed hernia. The term obstructed hernia expresses the morbid condition, obstruction, under which a hernia, when requiring an operation, labours. The terms, acute and subacute, attached, express the *different degrees* in which they are affected. The only other term, in my opinion, which need be applied to hernia, is that of inflamed hernia, which term I have, in my Treatise on Obstructed and Inflamed Hernia, &c. given to denote a state of inflammation among the contents of a rupture, independent of any direct mechanical obstruction, but arising as a consequence of the morbid changes which the parts undergo from long protrusion.

I cannot but dissent from the opinion of Mr. C. Bell, quoted by Mr. Lizars, that "the obstruction kills by the violence of the inflammation, occasioned by the accumulation in the upper parts of the canal, and the violent workings of the muscular coat of the intestines."

The obstruction kills by the stoppage of an important vital function without the necessary intervention of any inflammation, as I have proved in the Treatise I have published, and which will be still further proved in an appendix to a forthcoming edition. Mr. Lizars has quoted from my book a passage commencing, "the character which the disease assumes is that of peritoneal and general inflammation," &c. The above passage Mr. Lizars has quoted as a part of my description of obstructed hernia. This is an oversight, such passage belonging to the description I have given of inflamed hernia, a state of the intestine particularly distinguished from obstructed hernia; I having also particularly stated that inflammation among the intestines is by no means a necessary consequence or attendant upon obstruction.

Requesting an early insertion of the above, I am, sir,

Your obedient servant,

HENRY STEPHENS.

ANALYSES & NOTICES OF BOOKS.

"L'Auteur se tînt à allonger ce que le lecteur se tînt à abrégier."—D'ALEMBERT.

The History of the Contagious Cholera; with Facts explanatory of its Origin and Laws, and of a rational Method of Cure. By JAMES KENNEDY, Member of the Royal College of Surgeons, London.

IF we were disposed to pay Mr. Kennedy an ambiguous compliment, we might recommend his volume as the most convincing in itself, and the most satisfactory that we have met with for some time. The contagious character of cholera is here clearly proved, from the facts stated; and the treatment, of which the author approves, is fully borne out by the cases adduced. The professions of the title-page are largely fulfilled; and as the book is meant to be intelligible even to unprofessional people, we think the author may congratulate himself on having accomplished his object. The reader has but to be persuaded that here is the *whole* history, and here are *all* the reports worth giving, and nothing can be more conclusive than Mr. Kennedy's labours.

205.—1x.

But, unfortunately for us, we have dipped rather too deeply into the sea of evidence, in the shape of multiplied reports, despatches, and official documents of all sorts and sizes, to permit us to be so easily satisfied, or to come from the perusal of the work untinged with some remnants of scepticism. Our pages, for months past, have shewn how much attention we have bestowed on all that was to be said on both sides; and though, to confess the truth, we are strongly inclined to adopt Mr. Kennedy's view of the question, to which we think the balance of the evidence will be found to preponderate, yet candour forbids us to declare ourselves perfectly satisfied as yet, and our duty as reviewers obliges us to remain unwedded to any particular set of opinions. This, however, by no means prevents us from appreciating the straightforwardness with which our author hoists his flag and announces the party to which he belongs. Mr. Kennedy, in fact, spends little time in reasoning directly on the contagiousness of the cholera; his historical matter and his collection of reports are arranged with sufficient ingenuity to carry the desired inferences along with them; and he is willing, he says, to consider the two following general facts as amply sufficient to justify his views:—

"The first is connected with the local progress of the disease, as when it begins in a camp or a town. Here, its first appearance is announced in the attack of one or of a few individuals, and the number of the cases gradually increases. This course cholera has universally pursued. Now, had the cause of the disease been generally diffused in the atmosphere of the camp and town, would not great numbers of the people have been attacked, almost immediately, on the occurrence of the first case?

"The second general fact is the following:—Among the islands of the Indian Ocean, it was observed that the cholera uniformly commenced its ravages in the sea-port towns, or in those towns seated a few miles inland, which have a constant intercourse with their harbours at the shore. In the island of Mauritius, the disease first appeared in the town of Port Louis. In Bourbon, the town of St. Dennis was the first attacked. In Java, the town of Samarang, and so of the islands Sumatra, Penang, Borneo, Celebes, Lucon, &c.

M

In the Persian Gulf, also, the same order of infection was observed. Muscat, the principal trading port town, first received the disease. Then the port of Bahrein, and Busheer and Bassora. How can this extraordinary and uniform partiality which the cholera exhibited in its choice of sea-port towns for its first inroad be explained, unless on the principle of imported contagion?"

It is to be particularly noted, that by far the largest portion of the volume is occupied with the history of the *Indian cholera*, and the principles and mode of cure which the author adopted when he was out in Hindostan. The disease, as developed in Western Europe, Mr. K. seems to think is identically the same as the Indian disease; nor does he seem to consider that the treatment, by rigorous depletion and dosing copiously with calomel and opium, so familiar in Indian practice, should be in any degree modified in the management of cholera among ourselves. Upon this subject we have more than once expressed our decided opinion, nor need we repeat our reasons, founded upon experience and the strongest testimonies, for deeming that herein lies Mr. Kennedy's most essential faultiness. We doubt not, however, but that most readers will make this observation for themselves: were we but sure of it, we should have no hesitation in recommending the volume generally, as one of the most interesting on its awful subject that has yet fallen into our hands.

The work is furnished with two maps, from which we copy the following dates of first visits of the cholera:—

Jessore and Calcutta	Aug. 1817
Delli.....	July 1818
Bombay	Aug. —
Madras.....	Oct. —
Palamcottah	Jan. 1819
Manilla and Canton	— 1820
Pekin	— 1821
Busheer, Shiraz, Yezd, and Ispahan'	— —
Indian Islands.....	— 1823
Orenburg	— 1829
Astracan (up the Volgar) to Moscow	July to Sept. 1830
Down the Don to Azof, Sept. to Oct.	—
Warsaw	April 1831
Dantzic and Riga	May —
Archangel and Petersburg, June	—
Pesth and Bucharest	July —
Berlin	Aug. 31, —
Vienna	Sept. —

Between the years 1823 and 1830, Persia seems to have been the *nidus* in which the slumbering pest was kept alive, and from which it got into Europe through the port of Astracan.

Reports of Medical Cases. By DR. BRIGHT. Price 9*l.* 9*s.*

[Continued from page 88.]

PRESSURE ON THE BRAIN resumed.

Obstruction in the Lungs.—The next source of pressure on the brain adverted to by Dr. Bright is obstructed circulation through the lungs.

A bulky gentleman, above 70, who for several winters had been subject to chronic bronchitis, consulted Dr. Bright. He was in his drawing-room, walking about, but talking frequently; knew the doctor when he entered, and answered some questions about his health coherently; then at once seemed to become forgetful, and talked wildly. His attention, however, was immediately recalled, but as speedily was gone again. The pulse was large and full, about 80; the tongue dry, brown at the centre, and white towards the edges; the cough gave him great pain in the forehead; his expectoration was copious. Under the use of calomel, with purging mixture, ipecacuanha, and blisters to the chest, he was completely restored in a fortnight. Another case, however, which was very analogous, did not end so favourably. When Dr. Bright was sent for his patient was sitting up in his drawing-room, dressed as usual; his countenance was dusky, and his lips and tongue purple; he answered questions rationally for a few moments, and then became incoherent; his breathing was very difficult, and he complained much of weight at the chest; his pulse was large. He was cupped to eight ounces on the pit of the stomach, and had expectorant medicines; but he continued to get worse, and died early next morning.

We confess that both the above cases seem to us rather to shew cerebral disturbance from the circulation of imperfectly arterialized blood than from pressure.

A man, aged 46, was admitted into Guy's Hospital with dyspnœa, cough, and abundant expectoration of frothy mucus; his shoulders required to be elevated; his extremities were cold and œdematous; his pulse feeble and irre-

gular. Stimulants were administered, but he grew more and more oppressed, and died within two days. On examination after death, general turgescence was found in the vessels of the brain, with a peculiar mottling throughout the whole medullary substance, which became still more conspicuous when a slice was cut off, and allowed to become dry. The lungs bore marks of old inflammation, and the mucous lining of the bronchi was red, thickened, and in places even granulated.

Several cases follow in which cerebral congestion arose from emphysema of the lungs. In these cases the clear hollow sound emitted by the chest on percussion, and the faint murmur of respiration which was present at the same time, clearly indicated the nature of the disease. The head affection, however, presents nothing peculiar, under such circumstances, to distinguish it from others dependent upon imperfect action of the lungs. In the two cases which follow, great congestion in one, and actual extravasation of blood on the surface of the brain in another, were dependent upon whooping-cough. Yet farther instances are given of congestion of the brain, brought on by pulmonary obstruction; but the illustrations we have given will be sufficient on this head. We observe that Dr. Bright attributes the symptoms to the joint operation of mechanical congestion and a chemical change in the blood.

Pressure from Serous Effusion, independent of Inflammation.

This constitutes an entirely distinct pathological condition from any of the preceding, and the first case adduced is one in which the cause must be regarded as entirely mechanical. A woman was hanged, and the brain examined forty-eight hours after her death. Drops of fluid blood presented themselves on the surface of the dura mater and on the inner surface of the bone, which had a mottled appearance from effused blood. There was no coagulum in any of the sinuses; there was "decided, but not great" serous effusion under the arachnoid; the medullary substance of the brain was of a dusky colour, and full of bloody spots; the ventricles contained some serous effusion; the vessels at the base of the cranium by no means injected;

the arteries, indeed, were empty. In this case the appearance of cerebral congestion was not nearly so great as in some of the pulmonary affections above detailed, and not nearly so well marked as in the cases of death by hanging published some years ago by Dr. Monro, of Edinburgh, and by the late Dr. Kelly, of Leith.

Four cases are next detailed in which persons had become insensible from breathing the fumes of burning coals. They had all well-marked symptoms of cerebral congestion, but two of them recovered. In the two who died, slight serous effusion was found beneath the arachnoid, and there was some, but not considerable, appearance of increased vascular turgescence. In the case which follows next in order, effusion had taken place into the ventricles and beneath the arachnoid, from vascular congestion dependent on diseased heart. In another the same effect resulted from bronchitis; and in a third, which was attended with apoplectic symptoms from serous effusion, the kidneys were found granulated. In this case the urine was coagulable. The same appearance—namely, effusion of serum beneath the arachnoid, also presented itself in several cases of anasarca, where the kidneys were granulated, and the urine coagulable.

In case 118, a weak exsanguine man, with tuberculated lungs, had effusion under the arachnoid; in case 119, the same occurred in a case of emaciation, with bilious vomiting; and in three cases of diabetes, the only instances in which Dr. Bright has examined the brain in that disease, serous effusion was discovered. The author concludes this section with the following remarks upon the cases which it contains.

"In the foregoing cases of serous effusion we have an opportunity of tracing several of the different circumstances under which this morbid affection takes place within the cranium, independently of inflammatory action. These circumstances, for the most part, are immediately and obviously connected with a state of congestion, from causes which either act to a certain degree mechanically, or produce a state of debility which favours the irregular accumulation of blood. In the first case, direct mechanical pressure was applied; and if, as we suppose, the effusion depended upon the congestion arising

from the ligature, the effect must have been produced very speedily. This, however, accords with what we have reason to believe occurs occasionally in disease, where, from the rapid appearance of symptoms, effusion seems to have taken place within a very few hours before death. Some facts have been recorded by Dr. Kellie which shew that causes producing cerebral congestion are calculated to bring on very speedily serous effusion: he has related the cases of two individuals who were found dead from a night's exposure to cold, where serous effusion had taken place to a considerable extent, both in the ventricles and beneath the arachnoid. The well-known effect of cold in producing a state of unconquerable drowsiness, is satisfactorily explained, if any such elucidation were requisite, by these two cases; for excessive venous congestion was found accompanying this effusion, and it is this state of congestion, no doubt, which brings on the lethargic slumber, and terminates in effusion, or not, according probably to the previous condition of the sufferer.

"The five cases in which symptoms of cerebral congestion depended on inhaling fumes from coal and from the brick-kiln, afford further examples of the same tendency to serous effusion when the circulation through the brain is obstructed; for in the two cases where death unfortunately gave us an opportunity of investigating the state of the brain, serous effusion had actually taken place; in most of the others there is reason to believe that the morbid affection proceeded no further than the state of congestion, though it is not impossible, from the slow recovery in Case CVII., that effusion had actually taken place to a small extent.

"We have likewise, amongst the cases of serous effusion, an example in which the congestion depended on the circulation being obstructed in the heart, a circumstance which is by no means uncommon; there are others in which bronchitis and phthisis, by permitting the natural flow of blood through the lungs, have produced such a condition in the vessels of the brain as has led to serous effusion: frequently, indeed, as we have before seen, these same causes of obstruction produce only venous congestion, a difference which depends upon the previous condition of the body. In some cases simple debility

is perhaps capable of producing this effect, of which phthisis and diabetes seem occasionally to afford examples; though in both of these, other causes are brought into action, besides debility, in a manner likely to favour the effusion. We have sometimes less questionable examples of this cause of serous effusion, afforded in hæmorrhagic diseases and in the exsanguine constitutions of anasarcaous patients, though in a great majority of these cases decided affections of the kidney, or of some other important organ, as the spleen, serve to throw doubt upon the extent to which debility should be considered the prevailing cause of the effusion."

Pressure from Effusion of Blood within the Cranium.

A very interesting and instructive collection of cases follows. They may be divided into those in which the effusion had taken place on the surface, and those in which it had occurred in the substance of the brain.

A diminutive lad, aged nineteen, was visited on the 30th of November; he was lying on the bed insensible, with the breathing slightly stertorous. He had complained of being unwell; and while on the close-stool suddenly exclaimed, "Oh, my head," at the same time laying his head on the bed. In this state he had continued till the time of being seen by the narrator, (Mr. Streeter). The pulse was jerking, and the skin moderately warm. A vein was opened, but ere two ounces had flowed he became cold, and the pulse fluttering; the arm was instantly bound up. Some brandy, and a little compound spirit of ammonia, were administered; cloths, dipped in warm water, were applied to the pit of the stomach; and as the head felt hot, cold cloths were applied suddenly, and with a jerk, (as a substitute for cold affusion, and to avoid wetting the bed) to the temples. As no abatement of the insensibility took place, two cupping-glasses were applied to the back of the neck, but without previous scarification. Still no apparent improvement manifesting itself, one scarification was made, and about an ounce of blood abstracted—watching its effect on the pulse, which becoming depressed, the depletion was immediately discontinued. After a few hours he regained his sensibility, and was able to speak, though languidly.

He was blistered on the back of the neck, and purged; and he appeared gradually improving till the 8th of December, when he had a return of pain in the head; during the night he became insensible, and died at eight next morning. Dissection exhibited at least eight ounces of blood on the left hemisphere of the brain, and this was found to have proceeded from the bursting of an aneurism, about the size of a small bean, where the anterior and middle lobes join: it was connected with one of the smaller ramifications of the middle cerebral artery.

A lady, advanced in life, suffered a sudden attack of apoplexy in May 1827, but which was so entirely relieved by bleeding that in a few days she was free from any symptom of it except slight headache. In February 1828, having dressed as usual, she fell down on walking into her drawing-room. She was bled, and recovered a little, so as to complain of excessive pain by screams and moans, but she could not speak, and died in about half an hour. No trace of the former attack was found on examining the head. Blood was effused deep between the convolutions of the brain, particularly on the left side. There was some blood in the third ventricle; none in the others. The rupture was supposed to have taken place where the vena magna Galeni joins the lateral sinus; near that part the chief coagulum was situated.

A gentleman fell and received a blow on the back of the head, not sufficient to excite much attention, though it caused some external tenderness. Some days afterwards he dined out, and on the following day had a party at home. The morning after, he complained of pain in the head, became sick, and on the third morning suddenly lost the use of the left side, and could scarcely articulate. He was bled copiously, but died in the evening. On the posterior surface of the right hemisphere was a large quantity of blood. Serum was effused in other parts of the brain, and also in the spinal canal.

A gentleman, aged 72, fell down while walking, having tripped against some wooden pegs, on one of which he struck his face. His nose bled very freely, but he got up and walked home, three quarters of a mile, and called on Mr. Estlin, of Bristol (by whom the case is related), on his way. No injury of any kind, except

the bruise on the face, could be detected. He was purged, a cold lotion applied, and he was ordered to keep quiet and abstain from wine, &c. He continued, however, to walk about, as well as to eat and drink as usual. Mr. Estlin then adds—

“On the 11th of May (twelve days from the fall) he was met by his daughter, walking *from* his own house; and on being asked whither he was going, said he was proceeding *homeward*,—and shewed other marks of loss of judgment. I saw him soon after his return, and found him in a confused state of mind, with some incoherence. He occasionally fell off into a sort of doze; and asked if I had been sent for, and had returned any answer? at the time I was sitting by him. He frequently took up a book and appeared to read; he complained of his head, but did not express any sense of severe pain. He walked with difficulty up to bed (two stories): I bled him, and purged him freely; the bowels were much loaded, and were torpid. On the following day some paralysis of the limbs appeared, and rather violent spasms of the face, particularly of the muscles of the lower jaw and neck, coming on in paroxysms, and lasting for a minute: his speech became very inarticulate, and he dozed a great deal. The symptoms daily increased. He was blistered, leeches, &c. When asked if in pain, he always said, “No;” and seemed, when thoroughly awake, conscious of what was said to him. His pulse was not quick or unsteady, and the pupils were not in any respect dilated. He died on the 18th of May.”

On sawing through the cranium, a copious bleeding took place on the right and anterior part of the skull. When the calvarium had been removed, much coagulated blood was discovered beneath the dura mater, which extended to the base of the middle lobe, and invested the origin of the fifth pair of nerves. No injury of any part of the bone could be detected. The ethmoid was particularly examined. The source of the hæmorrhage could not be made out. Mr. Estlin thinks that the effusion was gradual.

A tall, large-made man, aged 46, of rather intemperate habits, and of gouty diathesis, had had at different times, some years before his death, fits of giddiness, pain in the head, and other cere-

bral symptoms, together with a peculiar fluttering sensation about the heart. About six months before his admission, he had a fit of an epileptic character, which occasionally returned; and he appeared, about two years previously, to have had a paralytic affection of one side of the face. His pulse was seldom above 30, and sometimes fell to 22. He had been in the hospital for gout during some months in the autumn of 1828, when he had frequent fits of palpitation, with confusion in the head. In May, 1829, he was again admitted, labouring under a similar condition. One of the most remarkable symptoms above alluded to continued—viz. the slowness of his pulse, which now averaged 32, while his respiration was so much as 28. He again left the hospital on the 1st of September, returned next day in his usual state for some mixture which he was in the habit of taking, and on the 3d, while in the act of taking a glass of spirits, he uttered an exclamation, fell, and died without a struggle. The following remarkable appearances were found in the head, which, we ought to remark, was not examined till five days after death.

“On raising the anterior lobes from the skull a very marked black appearance was observed on the inside of the dura mater, covering the bone on each side of the crista galli: it was evidently a carbonaceous deposit in the layer of arachnoid lining the dura mater, and was made up of a number of black points: this black colour increased greatly in intensity on all that part of the dura mater which lines the petrous portion of the temporal and the lower part of the parietal bones, and proceeded quite back to the tentorium, and was observed likewise on the dura mater of the occipital bone, almost to the foramen magnum; and although chiefly on the right side, passed over slightly to the left. There was also a very small patch of the same black deposit, shaded off on all sides, under the left anterior lobe; the intensity of this colour varied, but was greatest near the petrous portion of the right temporal bone: a very slight stain of the same dark carbonaceous colour was traced on some parts of the surface of the brain, corresponding to the darker parts of the dura mater. This grey appearance was evidently not in the substance of the dura mater, but almost confined to the arachnoid which lines it, and could in some parts be peeled off, leaving the membrane nearly natural. The surface of the dura mater, next to the bone, was of a natural colour and appearance,

“The heart was remarkably large, appearing to distend the pericardium; it was at least twice the natural size, and the parietes throughout very feeble and thin; but all the valves were healthy, and the heart contained no fibrin or coagulum, and very little blood. There was one small patch of disease near the arch of the aorta, but the large vessels were in general healthy, though they were much stained with blood.

“The other viscera appeared healthy, but had in some degree lost their usual characteristic colours by the changes of approaching decomposition; unfortunately the kidneys were not examined.”

The author conjectures that the heart was the primary source of disease, and that some spasmodic affection of that organ, connected with the gouty diathesis, had been the immediate cause of the sudden dissolution. The circumstances, he remarks, were not unlike those which occurred in the case of the late distinguished Dr. Marcet. *He*, too, had recently recovered from an irregular attack of gout; *his* death was instantaneous, and no obvious organic lesion, sufficient to account for the event, was found on post-mortem examination. In both, the body ran with great rapidity into decomposition. The peculiar appearance of the dura mater is certainly very interesting; it probably depended upon the injury which took place at the time of the paralytic attack, three years and a half before death, and had consisted originally in an effusion of blood upon the arachnoid at the part; and probably the pressure thus produced was the immediate cause of the extreme slowness of the pulse—a phenomenon which, in a less degree, is frequently witnessed in cerebral diseases attended with pressure.

MEDICAL GAZETTE.

Saturday, November 5, 1831.

“Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso.”—CICERO.

PRECAUTIONS AGAINST CHOLERA.

WE entered upon the consideration of cholera now some months ago with a decided impression, derived from the

opinions of the Indian writers, that the disease was not contagious, and this sentiment has been indirectly hinted at in various preceding articles. No long time elapsed, however, ere we began to doubt the accuracy of this opinion, and latterly a farther acquaintance with the history of the disease, and a careful attention to its progress, and to the phenomena it has displayed in Europe, have led us to abandon our preconceived notions, and to yield our conviction in favour of contagion. The grounds on which this inference has been founded, we shall detail in another number; but as we have no object but to discover the truth, and as our feelings are not interested on either side, so we have given, and shall continue to give, all the information which reaches us, whether it make for or against the opinions at which we have at length arrived; and we earnestly call upon those who have adopted opposite views to follow our example in this respect,—an adjuration which the ex-parte statements of certain recent writers have called forth, though various circumstances lead us to fear that it will be unavailing;—the end in view with some, apparently being, not to discover which is right, but to maintain their own side, be it right or wrong.

If cholera be contagious, then there are obvious means of excluding it; we do not say that these will prove effectual, but that the nature, end, and operation of the precautionary measures, are apparent. If, on the other hand, it be not contagious, then there is no human contrivance by which its introduction can be prevented, or even impeded. Now, admitting that the mode of its propagation may be matter of reasonable doubt, we would ask, which is the more dangerous error—to hold that it is not contagious, and it turn out in the sequel that it is so, or to hold that it is contagious, and it prove to be other-

wise? There cannot be a shadow of doubt with regard to any thing so palpable. It cannot for a moment be denied, even by the staunchest anti-contagionist, that the direct tendency of teaching the public that a disease is not contagious, is to make them disregard those precautions which they would otherwise take against it; and thus, if the doctrine of non-contagion be erroneous, to multiply an hundred-fold the extent of the calamity. Let those, then, who ridicule the idea of contagion, beware, that the time do not come when the victims of their rashness shall denounce them as the deceivers that betrayed them into danger, and heap coals of fire upon their heads, by reminding the public of the exertions which they made to thwart the authorities in their reasonable endeavour to repel the invader, ere yet he had effected a landing on our shores.

To the quarantine now in operation we last week adverted, and we again recur, as affording the only hope which remains of excluding the disease from England; and if the regulations could be rendered as complete in practice as they are in theory, our hopes would be by no means faint; but with so extensive a coast to act upon, with so many temptations, and so many opportunities afforded by smuggling, of evading the sanitary precautions, we fear that much reliance is not to be placed on their efficiency. While, however, a chance of success remains, it behoves the press to co-operate with the government in carrying those measures into effect. True, they are injurious to trade, but what of that? the profits of the merchant must give place to the safety of the public: true, they are detrimental to the revenue; but surely it were better, if need be, to levy a tax upon the purses of liege subjects than upon their lives. Besides, the period of doubt cannot last long: if the disease come, why then

farewell to farther quarantine, at least by sea : if it be kept out, then the measureless benefit of its exclusion will reconcile the most prejudiced and discontented to the temporary inconvenience.

Regarding the progressive approach of cholera to the west of Europe as rendering it extremely probable that this country will be subjected to its visitation, we are gratified to find that the inhabitants are preparing themselves in earnest for its reception. Local Boards and Committees are everywhere being formed; the work of precaution is in progress, and a general purification has begun. That it is so, is to be attributed to the recent manifesto issued by the Board of Health, and circulated by the government in every town and village throughout the land. Oh, for the consistency of certain among our contemporaries, who doubtless, if their modesty permitted, could easily tell us in whose hands the care of the public safety might have been much better placed than where it is! First they raised a cry against the Board of Health, because they did too little, and in this perhaps there was some shew of justice; now the complaint is, that they have done too much. Their first directions were too general, their last are too particular; their first announcement was not sufficiently impressive to arrest attention, their last is calculated "to fright the isle from its propriety." True, the document circulated by the Privy Council is of a nature to excite alarm, but was it not high time that the people should be roused to a sense of their danger? Would it have been kinder and more benevolent to have let them sit with their hands before them till the enemy was upon them, rather than warn them to be prepared that they might repel the aggressor? Would these whining deprecators of the measures demanded by common prudence—would they have us, like the ostrich of the desert, run our heads into the bush,

and shut our eyes against the pursuer till we became his prey?

In such a case as this, and with a people wrapt up in the busy pursuits of life, an appeal to their fears was the only method by which they could be roused from their habitual indifference; and we rejoice to find that it has not been made in vain. The tone of mild precaution was disregarded; but the more vivid picture of approaching danger has arrested attention, and the whole coast is on the *qui vive*. It is true, that more of apprehension mixes in the exertions at present making than may possibly be warranted; but those who have paid any attention to the workings of the public mind, must have observed how soon it becomes habituated to existing circumstances, and resumes its composure under events which at first were viewed with dread. It is incomparably better that the people should look the evil in the face—the worst of the panic will be over before the disease actually comes, and its presence will not create an hundredth part of the consternation it would otherwise have done.

Entertaining these sentiments, we earnestly call upon our readers to co-operate, to the utmost of their power, with the local authorities in carrying into effect the recommendations published by order of the Privy Council*. It is very true that some of the suggestions for the isolation of the sick appear to us of doubtful expediency; but all the preliminary measures have our fullest concurrence, and the more completely they are carried into effect, the less hazard there is of the ulterior precautions ever being required. Every man may do something towards improving ventilation, enjoining cleanliness, and

* We have not deemed it necessary to insert the Regulations issued by the Privy Council for the prevention of Cholera, as they have already been so very extensively circulated.

urging the benefits of temperance. A desire to avoid susceptibility of the disease is one of the strongest motives by which to influence men's minds; already, indeed, has this salutary principle begun the work of reformation; and it is engendering habits of regularity, which the unaided precepts of prudence or morality have never succeeded in effecting. In France they have too much sense to keep the people in ignorance, lest they should be frightened; and it is ordered, that every house in Paris be visited by persons competent to judge of its condition as to ventilation, sewers, and other circumstances of a similar nature, so that not a family in that great metropolis but have the probable approach of the disease forced upon their attention; and we anticipate as the result, that all which human sagacity can devise will be put in operation to prepare for its reception. Why should it not be the same with us?—why are not the civil authorities enjoined to enforce the necessary precautions? Let us be prepared, if the disease should unhappily reach us, to meet the emergency with calm and well-directed efforts; and above all, let us not be betrayed into a false security by those who make light of a disease in which they have no experience. Those who talk the boldest while the enemy is not within sight, are generally the first to play the craven when he comes. Are they who exclaim that the alarm is false aware that it is ascertained that not fewer than an hundred thousand persons have been cut off in Hungary, and eighty thousand in Galatia, where the principles of non-contagion have prevailed, and where the people have waited for the disease in passive inactivity? Should the confident predictions with which the press has been teeming, that the disease will either not come, or come robbed of its terrors, prove fallacious, we should then have to encounter the

most fearful of all calamities in such a case—that of a sudden and general panic, by which the energies of the public mind would be paralyzed at the very moment when coolness and self-possession would be most required.

Since writing the above, our attention has been directed to some observations of Dr. James Johnson, recently published in the Times; we have also seen another letter from that gentleman in the Herald, into which it had been copied from the Courier; and we perceive that we are to be favoured, through the same medium, with a succession of papers of a similar nature. This somewhat singular mode of giving publicity to his opinions, the worthy doctor assures us, does not proceed either from “vanity or self-interest,” and that he has no end in view but “the good of his country.” That he should anticipate such a result from his epistles puts at rest the idea of “vanity” having any share in his motives. With regard to the absolute absence of “self-interest” we should not have been so well convinced had not the doctor given us the above satisfactory assurance; but for this, it is just possible, that having his name and address kept before the public in successive numbers of a daily paper in extensive circulation, and accompanied by the laudatory comments of a friendly editor, might have been regarded by ill-natured persons as having some slight weight in leading him to adopt a proceeding so unusual with respectable members of our profession. We subjoin an extract from the Courier, that our readers may judge of the tone in which the passages we allude to are written:—

“Dr. Johnson is, from his long acquaintance with the disease in India, and from his high standing here as a physician, a better authority, perhaps,

than any other in this country; and it is not very creditable to the Board of Health to have put forth their opinions publicly, without having consulted that gentleman.'

We cannot for a moment suppose that Dr. Johnson had any thing to do with the insertion of this, or other equally absurd paragraphs; but we do think that most men would have avoided affording them even the indirect countenance they receive from his immediately becoming a contributor to the paper in which they appeared.

EXTRACTS FROM JOURNALS,

Foreign and Domestic.

ON THE IDENTITY OF SMALL-POX AND COW-POX, AND ON A MODE OF INTRODUCING THE VACCINE PUSTULE IN THE COW AT PLEASURE.

DR. SONDERLAND, of Bermen, the author of the paper which we shall here translate almost without abridgment, if his experiments be correct, has at length succeeded in establishing what physicians have long laboured to discover—a satisfactory and simple explanation of the protective power of cow-pox against small-pox; and has announced, we will venture to say, the most important discovery which has been made in the pathology of these diseases since vaccination was first introduced—by shewing that they are modifications of one another, and that cow-pox in the cow is simply small-pox in man, and may be produced in that animal at will by the variolous contagion. Of the authenticity of his facts we do not pretend to judge. All we can say is, that the author, if we judge from the language of Hufeland towards him, is a respectable practitioner, and a public medical officer.

"The simplest and surest mode," says he, "of producing cow-pox in the cow, and thus proving indisputably the identity between the contagion of cow-pox and that of human small-pox, is to follow the procedure here laid down.

"Take a woollen bedcover which has lain on the bed of a small-pox patient who has died during the suppurating stage, or is suffering from the disease in a considerable degree, and is lying in a small, imperfectly ventilated apartment; and when it is well penetrated by the contagion, roll it up immediately after death or on the fourteenth day of the disease, wrap it in a linen cloth, and then spread it for twenty-four hours on the back of a quey in such a manner that it cannot be thrown off by the animal. Then place it for twenty-four hours on the back of each of three other queys, and afterwards hang it in such a manner in their stall that its exhalations may rise upwards and be inhaled by them. In a few days the animals will fall sick and be seized with fever; and on the fourth or fifth day the udders and other parts covered with hard skin will present an eruption of pustules, which assume the well-known appearance of cow-pox and become filled with lymph. This lymph, which exactly resembles the lymph of genuine cow-pox, if used for inoculating the human subject, will induce the vaccine or protective poek. The only precaution which it is necessary to observe is that the person about to be inoculated shall not be exposed in any manner to the contagious effluvia of the cow-house either directly or through the intervention of the experimentalist's clothes, otherwise he may have natural small-pox.

"A bedcover impregnated with the variolous contagion, if firmly rolled up and wrapped in linen, and afterwards in paper, and then properly packed in a bucket, will retain the contagion for at least two years, so as to infect a cow with cow-pox, provided it be kept in a cool, shady place, where the temperature does not fall under 32 or above 52 degrees.

"My present occupations prevent me at this particular period from giving a full and scientific exposition of the consequences which must follow from this discovery; but I may state them shortly in the aphoristic form.

1. "This discovery is new; for, although many have suspected the identity of small-pox in man and cow-pox in the cow, and have in consequence performed inoculation with the matter of both, yet no one has previously ascer-

tained the possibility of transmitting the contagion to the cow in the gaseous form, so as to decide the question beyond all doubt.

2. "The desire of physicians and governments to discover cow-pox in cows, in order to revive the vaccine lymph, is more than fulfilled by the discovery of a simple method of engendering cow-pox in the cow at will.

3. "Jenner's discovery of the protective power of vaccination hitherto imperfect, is now perfected, because the hitherto unknown nature and origin of cow-pox are laid open.

4. "All previous uncertainty regarding the quality of vaccine matter, its degeneration, the loss of its protective property, and the like, must now cease, because we have obtained a clear insight into the nature of cow-pox and can lay down a substantial theory of its operation.

5. "This discovery must tend to widen the boundaries of physiology, pathology, and therapeutics, since it shews how the subtle contagion of small-pox, so hostile to the nervous system of man, may be conveyed in the aeriform state from him to the cow, excite in that animal a similar disease, but in doing so be changed by the special constitution of this class of animals into a permanent contagion of a different kind.

6. "An instructive lesson may be drawn from this discovery how the poison of diseases in the gaseous form may be communicated to the lower animals, and according to the difference in their constitution engender diversified products, which may be then used as protective means against the diseases from which they originated. Such, for example, may be subsequently proved of scarlet fever, measles, yellow fever, and plague.

7. "It is now clear why, in recent times, cow-pox has been seldom or never seen in the cow. For the cow-pox of the cow arises merely from infection by the variolous exhalations from men recently affected with small-pox, and coming in contact with the cow. As epidemics of small-pox have been rare during the last thirty years, cows could seldom be exposed to infection, and have therefore seldom exhibited the disease."—*Edin. Med. Journ. and Journal des Praktischen Heilkunde*, Januar. 1831.

BRONCHOTOMY IN THE HORSE.

The number of the Veterinarian for November contains four cases in which the trachea was opened, with the effect of saving the lives of the animals: these were all instances in which suffocation was impending from inflammation about the larynx. The following is the description of the operation as performed in the first case:—"An incision was made, about four inches in length, through the integument and sternothyroid muscle; a portion of one of the rings of the trachea was excised, large enough to admit one's finger; a thread was passed through the muscle and integument on each side, and fastened to the mane, to prevent the aperture from closing." The cases were treated by Mr. Toombs, of Great Barrington.

WESTMINSTER MEDICAL SOCIETY.

Saturday, Oct. 29th.

The Cholera.

A REPORT having been circulated through the newspapers, that the all-engrossing subject of cholera was to be discussed here this evening, there was a very full attendance of members and visitors.

DR. SIGMOND in the Chair.

Whether the "order of the day" had been correctly reported or not, we cannot tell; but certain it is, that much time was lost in solemn silence after the president had announced that the business might now begin, until at length

DR. GRANVILLE volunteered to open the discussion. He expressed himself highly dissatisfied with the regulations of the Board of Health, which he thought were both useless and severe. On other occasions he had proved himself a stout advocate of contagion—as all would remember, on the Plague question; but he would now take credit to himself for maintaining as strenuously the opposite doctrine with regard to Cholera: on this question he was a steady non-contagionist. There was one great character of a contagious disease absent in cholera, there was *no eruption*. Nor was the spreading of the disease like that which would result from infection or contact: in St. Petersburg, for instance, where the utmost care had been taken to exclude the malady, *one* case was at length officially

announced, and in less than four days after there were between 400 and 500. How could this possibly have resulted from contagion, unless the sick had actually run out into the street, and touched and infected every one they met? The same conclusion was also to be deduced from the history of the disease, as it appeared in other places. He regretted that we had not sufficiently exact accounts of the pathology of cholera: of the matter or fluid discharged from the stomach, there was but little information: it was only occasionally, and almost by accident, he (Dr. G.) could gather that it was sour to the taste; from which he was naturally induced to infer the presence of muriatic acid, especially as the other symptoms would warrant the supposition, that the patients suffered as if poisoned with that acid. The learned Doctor having indulged, in the course of his speech, in a vein of merriment upon the published regulations of the Board of Health, and upon the history of the disease generally, as given by the contagionists, apologized, in conclusion, for having treated with levity a subject of such awful interest: he did it merely, he said, to give animation to his arguments.

Dr. O'SHAUGHNESSY contended that there was no ground for supposing that patients in cholera died as if poisoned with muriatic acid, or that there was evidence of any excess of that acid in the contents of their stomachs. And as to the absence of an eruption, he would instance hooping-cough, a decidedly contagious complaint, which was also unattended with an eruption. On the subject of cholera, Dr. O'S. expressed himself to be an advocate for its contagiousness: he instanced its making its appearance in the track of human intercourse at the greatest heights and lowest depths; and the recent information which had reached us with regard to the propagation of the disease among the pilgrims at Mecca, and thence spreading into Egypt, seemed to be decisive.

Dr. GRANVILLE, in explanation, maintained that pertussis was not contagious; and attributed the pestilential cholera among the pilgrims to their numbers, last season, and their long-continued prostrations over broiling sands, at 34° of Reaumur.

Dr. GREGORY thought that the contagious cholera was a *new disease*, sent by Providence to visit the earth, as the small pox was in the seventh century.

The cholera, too, would probably abide with us, like the small-pox, appearing from time to time, after its first ravages, in a virulent form. The cholera had originated in India in 1817, and it still continued to exist in that country, and most likely would never be eradicated. The same, no doubt, would be its destiny in Europe. With regard to quarantine, he feared no measures would be sufficiently rigorous to exclude the disease: nothing short of an impenetrable wall built all round the country could effect such an object.

Dr. JOHNSON called the attention of the Society to the difficulty of coming at exact information with regard to the mode in which the disease was propagated: the evidences to be found in books were suited to the writers' preconceived opinions. He took the latest book published on cholera—that by Mr. Kennedy—and what did he there find? The author professes to give a selection from the excellent Indian Reports, and, out of some hundreds, thirty-seven are given. Now it is a well-known fact, that the great majority of Indian practitioners are non-contagionists; yet, what has Mr. Kennedy done?—of his select thirty-seven, all but two are contagionists. As to cholera being a *nova pestis*, as Dr. Gregory would have us believe, he (Dr. J.) could not subscribe to any such doctrine. He had *seen* the disease himself, in India, twenty-seven years ago, and Mr. Curtis had had experience of it twenty years earlier. Nay, he was not sure but that Sydenham had witnessed it, in the peculiar cholera of 1669, as he says nothing about the presence of bile in the discharges of the stomach, in his account of it. Precautions for the exclusion of cholera are useless: even if the impenetrable wall of Dr. Gregory were built as high as the sky, the disease will come among us *if it chooses*.

Dr. STEWART made some remarks on the probable influence of certain vicissitudes which had taken place in the weather within the last year or two. Severe cold had been experienced in southern climates, and extreme mildness in the northern latitudes.

Dr. JOHNSON (after a pause in the debate) rose to express his opinion that the propagation of cholera can only be accounted for by the supposition of exhalations from the earth: there is something going on beneath the earth's surface that we know nothing about.

MR. KING sagaciously inquired, "whether any member of the Board of Health was present?"—and upon being answered in the negative, rejoined, in a solemn tone, "Then that is to be noted!" He then inquired, with equal solemnity, "Whether there was any evidence, or could any gentleman inform him whether there was any such thing as cholera at all in the world?"—(laughter.) He was then proceeding to panegyrisse Dr. Barry, and to propose that the discussion on cholera should not be resumed till that gentleman's return from Petersburg, when

MR. CHINNOCK moved an adjournment of the discussion to Saturday next (this evening); which was carried by general consent.

CLINICAL LECTURES,

Delivered in the Dispensary of the London University,

BY

DR. ANTHONY TODD THOMSON.

LECTURE III.

October 27, 1831.

Rheumatism—Remittent Fever—Epilepsy.

GENTLEMEN,—Since our last lecture one of the cases of Rheumatism, that of Pennifather, has been discharged cured, and two others are likely to be so in a few days. In all of these cases the severe symptoms yielded to the use of the Calomel, Opium, and Colchicum, and little remained but slight indications of pain, preceded by rigor in the evening, for which the Sulphate of Quinia was ordered, and speedily removed them. In the case to which I alluded in my last lecture—that of Cadwell, the swelling of the wrists, and the pain excited by touching them, had greatly increased; but I pointed out to you the cause of this, in the intemperate habits of the patient, who smelt strongly of gin, and who acknowledged that she had indulged in a few glasses of that cordial, to comfort the weak state of her nerves under her sufferings. I take this opportunity, gentlemen, of pointing out to you, that it is only in persons who indulge in the abuse of stimulants that I have ever seen any thing like the co-existence of Gout and Rheumatism, forming that mixed disease which has been named Rheumatic Gout; and, although I am aware that Rheumatism makes its attack on the most temperate and regular habits, yet I can assure you that it is infinitely more difficult to cure it when the use of wine or spirits is continued during the treatment of the disease, than when these

are altogether set aside. Indeed, I know scarcely any disease in which attention to diet and regimen is so essential as in Rheumatism.

The employment of Bark as a remedy in Rheumatism was in use as early as the time of Morton, who, although a bad authority in matters of theory, yet was an honest physician; and his word can be implicitly relied upon, in reference to the result of any mode of practice. His observations were afterwards confirmed by the experience of Sir Edward Huhn, Sir John Pringle, and Dr. Fothergill; but it was not until the clinical experiments instituted by Dr. Haygarth, on the effects of this remedy in acute Rheumatism, that its value was fairly appreciated. Dr. Haygarth's practice was, first to reduce the febrile violence of the attack by antimonials, and then to throw in the Bark. He found that in no case did any mischievous effects follow its employment; and when manifest relief was not obtained at first, it was generally afterwards ensured by suspending the use of the medicine, and again having recourse to antimonials, until sufficient evacuations were procured. In such cases, the Bark was not again employed until the inflammatory symptoms were abated. The perusal of Dr. Haygarth's work, and an entire confidence in his details, induced me to give his plan a fair trial; and, with little variation, I have continued to follow it ever since. I have, certainly, seen the Bark do harm in acute Rheumatism; but then the precautions necessary to be observed previous to its administration, have not been duly observed. The substitution of the Sulphate of Quinia for the Bark has only added to our power over acute Rheumatism: sometimes, I have found it to be requisite to combine aromatics with the Sulphate; in which case every indication has been answered by the essential oil of Calamus Aromaticus rubbed up with Sugar, as an Oleo Saccharum, and added to the Sulphate. I was induced to prefer this aromatic to any other, from having observed the powerful influence of the root of *Acorus Calamus* in curing Agues, when used in the form of infusion, as a vehicle for the administration of the Bark. In Sussex the powder of this *Acorus* is used alone as a remedy for the cure of Agues. When Bark has failed to fulfil the intentions of the prescriber, Arsenic has been occasionally employed, after the attacks of the rheumatic pains have become decidedly intermittent. My experience of its efficacy does not authorise me to speak confidently in its favour.

With respect to the use of topical remedies, you will soon perceive that I very rarely order them. I have several times seen recessions of the complaint follow the employment of Rubefacients; and as, in such cases, we have no method of preventing the disease from falling upon a vital organ, it is better either to reject them altogether, or to employ them with the

utmost circumspection. The only topical remedy which I have freely employed is the application of leeches to the pained part, followed by either emollient poultices, or the pediluvium, when the lower extremities are the seat of pain. The comfort of the patient is thus much improved; but topical bloodletting, however performed, does not contribute to the cure of the general disease.

From what I have already said respecting the bad effects of intemperance in retarding the cure of Rheumatism, it is scarcely necessary to force upon your attentions the propriety of prescribing a Milk Diet. Sydenham very properly interdicted the use of animal food and of all fermented liquors. Indeed, in every disease, if you reflect that remedies are intended to make a certain impression on the habit, you need not be informed, that the less this is interfered with, or interrupted by the administration of stimulant food, the more likely are the remedies to prove beneficial.

Before closing my remarks upon these cases of Rheumatism, you may expect that I should say something of Chronic Rheumatism, some cases of which are upon the books of this Dispensary; but, in my opinion, *Acute* and *Chronic* Rheumatism are the same disease, merely modified by duration; equally active as respects the symptoms; and, in fact, requiring the same plan of treatment. In private practice you will find this truth too frequently presenting itself to humble the vanity of the inexperienced practitioner: you will find patients labouring under Rheumatism after having suffered for ten or twelve months, who will inform you, that their pains are as severe as they were at the commencement of the attack; you will find the same hard, tense beat of the pulse; the same buffy coat on the blood; and the same pink deposit in the urine. Under these circumstances, however, the means that would have insured success, if employed in the early period of the disease, will not now succeed: and it is only under the administration of alterative doses of Calomel and Colonium, in direct combination with Sulphate of Quinia, or some other tonic, such, for instance, as the Buckbean, *Menyanthes trifoliata*, that we can contemplate a salutary issue.

In long protracted cases, when the pains are confined to particular joints, I have seen much benefit derived from a plan imitative of the Douch Baths at Aix-les-Bains. On reading an account of these baths, which are of a temperature from 116° to 142° Fah., I was induced to form an opinion that the principle of their action was percussion in conjunction with a high temperature. The Douching apartments, which are caves of the rock, have the hot water conducted into them through channels, that terminate in tin tubes of about two inches in diameter. A large stream of water falling from a consi-

derable height is thus directed upon the affected part, whilst the patient is immersed in an atmosphere of warm vapour. The sensation experienced resembles that of a severe cudgelling; but this is followed by decided relief, and the repetition of the Douching, at intervals of two or three days, generally produces a permanent cure. To imitate these baths, I set my patient in a chair, at the side of which is placed a bucket or large vessel capable of containing three or four gallons of boiling water, and envelop patient and all with a blanket, pinned close round his neck. In a few minutes he is bathed in a copious perspiration, and in this state he is directed to apply percussion to the pained joints by means of an elastic ball, made of cork covered with kid leather, and fixed on a handle of cane or whalebone, about a foot in length. I have seen the most decided benefit follow this practice; and one case, of seven years' standing, after the daily employment of this *dry douching*, if such an expression be allowable, my patient was beaten into excellent health, and threw aside a crutch, which he had for years been forced to use.

As a prophylactic, after an attack of Rheumatism has been allayed, you will find nothing so serviceable as the daily use of the Shower Bath. If the patient have been much reduced, and the re-action is likely to be feeble, the bath should not be taken until the system has been fortified with breakfast; but when the vigour of the habit is restored, it may be used immediately after rising in the morning.

Remittent Fever.

I have now to direct your attention, gentlemen, to the two cases of Remittent Fever which have lately been before you; the subject of one of them, Henry Schmidt, has been discharged cured; William Coates, that of the other, which is of a less decided character, has just been admitted. Schmidt is a German, 26 years of age, of a delicate frame of body, with black straight hair, hazel eyes, and a pale complexion. He is by trade a cabinet-maker. He informed you, that for some weeks past he has felt unwell, without being able to specify any complaint, except that his feelings differed from their usual state. About six days before his application to the Dispensary, he was attacked by a rigor, which was not followed by much increased heat of body, and by no sweating. It left him, however, extremely languid, and ever since he has been one day better and another worse; but on no one day has there been a complete intermission of symptoms. He has generally felt better in the morning; his uneasiness, accompanied with alternate heats and chills, gradually stealing on in the afternoon, and not declining until about midnight; but although these states

were of daily recurrence, yet they were evidently more severe every other day. On the day of his admission, 18th October, his complaints were, loss of appetite and all relish for food; he stated that he suffered from a constant, although slight nausea; the tongue was much coated and dry; the bowels were confined; the urine natural; and the pulse, which was weak and compressible, beat 100. He had constant severe headache; the skin was clammy, the extremities were rather cold, and he mentioned that he felt occasionally ready to sink, and could not command his ideas. He slept heavily, but awoke unrefreshed. He was ordered a pill containing gr. vj. of Calomel, to be followed by a brisk Cathartic, and a mixture composed of four grains of Tartar emetic, in eight fluid ounces of Camphor mixture; three table spoonfuls to be taken every fifth hour. The pill and draught produced two stools only; but the tongue was cleaner, and the reportsays, "this morning (October 20th) he has perspired freely, which has brought down his pulse to 88. The Antimonial has not produced sickness; on the contrary, he feels less nausea, and his headache is gone." The pill, purging dose, and mixture, were continued. On the 22d he felt much better in every respect, and his countenance had lost the anxious expression which so strikingly marked it at his admission. The bowels were still torpid; but the skin was natural, the tongue much cleaner, and the urine natural. He was now ordered a mixture, consisting of ʒj. of Sulphate of Magnesia, six fluid ounces of Infusion of Gentian, and one fluid drachm of diluted Sulphuric Acid, the fourth part of which was to be taken three times a day. Under the use of this mild aperient, a farinaceous diet, sub-acid drink, and rest, he continued improving until this day (the 27th) when he was discharged cured.

Although this case, gentlemen, may be regarded as a mild specimen of the usual Autumnal Remittent of this climate, yet it presents some points for remark; and merits your consideration as a form of disease which is likely to come frequently under your care. The first circumstance in the case to which I would particularly recal your attention, is the peculiar physiognomy of the patient, which on his admission was so characteristic of general suffering, and of disease affecting the nervous system. The countenance, which was pale, sallow, and pinched, wore a look of great anxiety; whilst the eye was indicative of a disturbed state of the mind. The contrast between this expression and the lively, open countenance which the patient now wears, presents a theme for reflection too striking to have escaped your observation, and affords me also an opportunity of pointing out to you the great aids which the attentive study of the physiognomy of diseases affords us, both in diagnosis and in forming a prognostic. The second remark respects the evidence which you may draw

from the symptoms of this case—the alternate heats and chills, the nausea, the muscular tremors and sinking, and the wavering state of mind—are evidence in support of the opinion that much of what constitutes fever is referrible to the state of the nervous system; in opposition to that theory which would refer all fevers to inflammation of one part or another of the body. In a fever, such as that now before us, there are no symptoms which would authorize us to fix upon one part of the body more than another as the seat of the disease. That the vascular system shares in the general derangement of the nervous, is obvious; and it is the conjoint affection of both which constitutes the disease.

On inquiry, I could trace this fever only to exposure to cold acting upon a weakened frame, predisposed to morbid impressions from depression of spirits, arising from want of employment and its necessary consequence—bad food, and a scanty supply of it. The poor man, who you know is a foreigner, had been for some weeks unemployed. There was a general state of languor prevailing; the functions were performed feebly, and torpor existing in some of the viscera, which, by throwing, as it were, the circulation off its balance, produced that irritable condition of the system which is so favourable to the production of fever. Thence vicissitudes in the weather, such as have lately occurred, succeeding to a warm summer, are likely to produce effects on the body which, in any other condition, it would resist.

With respect to the treatment pursued in this case, it was obvious that, with a compressible pulse at 100; with very little cerebral excitement, and without any symptom indicative of local inflammation, this case did not demand the abstraction of blood. The other case—that of William Coates—to which I have referred, presents evident symptoms of Pulmonary Inflammation, and, consequently, eighteen ounces of blood were taken from the arm with unequivocal advantage, and he bore the loss well. In Schmidt's case, the aspect of the countenance, the rigors and coldness of the extremities, indicating intestinal irritation, pointed out clearly the necessity of purging,—the only question was the kind of purgative to be employed; and, in ordering the administration of Tartar emetic, it was my object, not merely to evacuate the bowels, but also the stomach. I was fully aware that the severe pain of the head, which so closely resembled Meningitis that it would have authorized, in some minds, cupping or the application of leeches, depended solely on intestinal irritation; and, if the accuracy of the opinion may be judged of by the result, it was fully confirmed: besides, in evacuating freely the stomach and lower intestines, the habit, I hoped, would be prepared for the use of Sulphate of Quinia, should it be necessary in the decline of the disease. This, however, has not been re-

quisite, the action of the Calomel and purgative on the liver having emptied the hepatic ducts, and improved the biliary secretion, and that the Antimony having restored the suspended action of the capillaries. Nothing further was, therefore, required to accelerate the return of health than to keep up a moderately soluble state of bowels, and gently promote the tone of the digestive organs; an intention which has been fully accomplished by the means prescribed.

Epilepsy.

Before concluding this lecture, I will merely direct your attention to the case of *Epilepsy* admitted on Thursday, and defer any comments on it until we shall have time to estimate correctly the value of the treatment which is now in progress.

Arthur Leni, æt. 37, a green-grocer, of a sanguine complexion, complains of what he terms fainting fits, which attack him once or twice a day, and throw him down in the streets, whilst pursuing his occupation. On inquiring into the previous history of the case, it appears that he had a severe Epileptic fit at fourteen years of age, but it was not succeeded by another until seven years afterwards, when he suffered a second attack, and this was followed by fits recurring once or twice in every subsequent year, until two years since, at which time they increased in frequency, and now he is scarcely during any day free from a repetition of the attack. The fit, he says, comes upon him unawares, throws him down, and he becomes insensible; but he is informed that he struggles much, and he has lately bit his tongue: he continues in the fit for a quarter of an hour, after which it is followed by profound sleep. On awaking he can scarcely distinguish the objects around him, and this loss of vision continues for some hours. He complains of severe and constant headache; there is no intolerance of light during the day, but he experiences something like it in candlelight: the conjunctiva is slightly inflamed. There is *timuitus aurium*, like the constant ringing of bells; the tongue is furred; the pulse 92, small, and irritable; the bowels are open, and the urine is high-coloured.

He was ordered to be cupped behind the ears, and ℥viij. of blood only to be taken; eight grains of Calomel, and three of Extract of Henbane, were desired to be taken at bed-time, and followed by a brisk cathartic on the following morning. On the 29th, two days afterwards, he attended again at the Dispensary, and stated that he had had no fit since the cupping; but, although he admits that he is better, yet, he complains still greatly of his head, and of the *timuitus aurium*; and adds, that he feels also a sensation, as if there was wind in the head. He feels now and then faint; his appetite is still impaired, and the tongue furred; the

pulse is 84, and feeble; the urine is high-coloured. He has been ordered twelve pills, each containing one-fourth of a grain of Calomel, one grain and three-fourths of Camphor, and a grain and a half of Extract of Henbane: two to be taken every eighth hour: and in the intervals a purgative, containing 3j. of wine of the seeds of Colchicum.

The most singular incident in this history is the long period which elapsed between the first and the second attack; and the rapid progress which the disease has lately displayed without any obvious cause. This disease is one which may arise from visceral irritation, and in that event is curable; but whether the present case be of that kind can only be determined by attentively watching its progress.

NOTE FROM MR. S. COOPER TO MR. COULSON.

To the Editor of the London Medical Gazette.

SIR,

I WILL thank you to insert the following note to Mr. Coulson in your next number, if perfectly convenient to you—I am, sir,

Your obedient servant,

SAMUEL COOPER.

Dear Sir,—I beg to correct a mistake into which you have fallen, by stating in your lecture published in the *Medical Gazette*, that I had said nothing in my Dictionary, of the treatment of the patient previous to lithotomy. The account rather surprised me, and, at first, I imagined that the omission must really have been the fact; but, on referring to the two last editions, I observe that the subject has not been neglected, as may be seen by referring to page 803 of the 5th edition, and page 828 of the 6th or last.

With respect to the minute descriptions of the various modes of lithotomy, some of which are now obsolete, I regret that you do not appear to like them; but I and many other members of the profession feel an interest, and know that there is much utility, in the historical views of what has formerly been done in surgery. Were we always to pay due attention to them, we should not frequently suppose various schemes and practices to be quite new, merely because they are revived.

I remain, dear sir,

Yours very truly,

S. COOPER.

Great Russell-Street,

Oct. 29, 1831.

Wm. Coulson, Esq.

BOOK RECEIVED FOR REVIEW.

Elements of Anatomy, General, Special, and Comparative. From the *Encyclopædia Britannica*, Seventh Edition. By David Craigie, M.D. With Fourteen Plates.

W. WILSON, Printer, 57, Skinner-Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, NOVEMBER 12, 1831.

LECTURES

ON

THE THEORY AND PRACTICE OF
MEDICINE ;

Delivered at the London University,

By DR. ELLIOTSON.

LECTURE VI.

Symptomatology (continued)—Indications afforded by the Pulse—by Examination of the different Regions of the Body—by the Excretions—Therapeutics.

I MENTIONED in the last lecture, gentlemen, that two parts of the body give us more information respecting, not their local affections, but the state of other parts, than those parts themselves, and that these were the face or the head altogether, and the hand.

I went over the chief symptoms which appear in the head, including the face, mouth, eyes, tongue—in fact, every thing above the neck ; and the chief symptoms which were indicated by the hand : and at the conclusion of the lecture I was speaking of the information obtained by the pulse, which is observed more easily in the hand than elsewhere ; not that the hand itself, or the wrist, has any peculiar quality of giving us information respecting the state of the circulation, although it is a great indication respecting other matters, but so it happens that the artery, (and it is a curious coincidence,) is to be felt better there than at any other part, and there we derive our chief information respecting the state of the circulation. The doctrine of the pulse is called *Sphygmica*.

I mentioned certain cautions which are necessary to be observed in noticing the pulse ; namely, to examine the pulse of each wrist, and in some cases to examine it also

at the heart. I mentioned likewise that it was necessary to take into consideration the age of the individual, the sex, the climate, the season ; and it is likewise necessary to ascertain the immediate circumstances in which the patient is placed—whether he is in a room of high temperature or not—whether he is near the fire or at a distance from it ; and also to ascertain what he has taken previously, because food accelerates the pulse, and the more stimulating its quality, of course the greater is the effect. You will observe different pulses at different hours of the day. It is necessary to take these matters into account, for if you were to find the pulse quicker than at your previous visit, and not to inquire whether the patient had taken food or not, or what other circumstance had occurred, you would be led into error. This is often a circumstance of great consequence ; for if you find the acceleration of the pulse accounted for by the patient having taken food, your prognosis remains as favourable as before. This has always been observed ; and you find in Celsus, what I dare say you all know, particular injunctions to medical practitioners not to feel the pulse immediately on going into the chamber of the sick, but to wait some time, to enter into conversation, and allow the flurry of the patient at the sight of his medical attendant to subside ; for it continually happens, that the pulse will augment from seeing the medical attendant, more especially for the first time. You will find the advice given by Celsus on this head to be in a high degree worthy of attention. It may appear trivial, but I do assure you it is of the utmost importance, and you will be led into a great error, especially in a public institution, if you were to put your hand on the patient's wrist at once ; it is better to ask some questions, and to remain some time before you feel the pulse.

However useful the information to be obtained from the pulse, if you neglect all these particulars, or if you take them into consideration and do not at the same time com-

pare the other symptoms with the state of the pulse, the pulse will lead you into very great error. The pulse itself, it is to be observed, affords only one symptom, and that may not be at all in unison with the real state of the patient. The pulse would sometimes lead you to suppose that there is the greatest danger, when there is none; and, on the other hand, it would sometimes lead you into security, when the patient is in the most imminent danger. However, this is not peculiar to the pulse; there is scarcely a symptom, which, if taken alone, would not lead you to draw a false conclusion. It is necessary, in examining any case, to take the whole collection of symptoms together—to set one against the other: sometimes you find them all harmonize, and at other times you find more or less confusion, but it is from the balance of the whole that you form your diagnosis and prognosis. The pulse has been called, in the most ancient times, *res fallacissima*, the most false of all symptoms; but the truth is, all symptoms are delusive, if taken separately: it is the collection which enables us to form our judgment.

I mentioned the chief varieties of pulses—the varieties which I think sufficient for all practical purposes, and which are the only ones that I have observed. You find in books an extreme *minutia* on this point, and I bink a great deal of absurd trifling. It is the Chinese, I understand, who have made the most minute observations on this subject: they describe all sorts of pulse, such as, I dare say, no one ever observed, and such as no one ever will.

Now as to these two parts of the body—the face, or rather the head altogether, and the hand—our observations upon them are chiefly made by sight and by touch; however, there is another sense that may be employed with respect to the head, and that is the sense of smell. The breath that proceeds from the mouth has sometimes peculiar odours, which, if we do not take pains to notice, we are compelled to observe.

The head and the hand, of course, give all the symptoms common to other parts of the body, when they themselves are affected. When either of these parts is inflamed, the various local symptoms present themselves. It is not, however, of local symptoms that I have been speaking, but symptoms indicating the state of the other parts of the system in general.

To proceed, however, downwards: the neck affords a certain degree of information, but not much. A long thin neck is frequently taken, in conjunction with other symptoms, as a sign of phthisis; whereas, a short thick neck indicates a disposition to fulness of the head and chest. The pulse in the neck is also an indication of different diseases. In many affections of the head, the carotids will throb violently, so as to be

seen at a distance; and, in many affections of the chest, the same circumstance is observed. Here, again, you see the necessity of attending to more symptoms than one. When you see a great throbbing of the carotids, you cannot tell whether it arises from an affection of the chest or of the head, for they both give rise to it. The pulsation of the jugulars is likewise frequently an indication of thoracic disease; a difficulty in the transmission of the blood through the chest will give rise to a great pulsation of the jugulars. I need not say that the tenderness of the neck indicates various diseases. By observing tenderness in that situation, you may frequently ascertain the existence of disease in the larynx, when otherwise you might be doubtful as to the nature of the affection.

With respect to the chest, it affords us a vast source of information; its sides and its shape indicate the general constitution. Phthical patients generally have a narrow and flat chest: the chest is contracted in various directions—shallow and flat in the front, and narrow at the sides. If a person be disposed to apoplexy, from mere plethora and congestion, and in gouty persons, the chest is disposed to be circular. Frequently the chest is deformed, and frequently it acquires an enlargement in various parts, from disease in the pleura. When a collection of water or pus, particularly of pus, takes place in the pleura, it is not uncommon to find the sides of the chest become enlarged. It is, therefore, frequently of great importance in thoracic diseases to strip the patient, and look at his chest: you may frequently discover by the eye a slight deviation in its two sides, which you cannot observe by the touch. A deviation so slight, that it would escape you if you merely resorted to measurement, is frequently perceptible to the eye. Of course, the touch will give you considerable information with respect to tenderness; and also with respect to the state of respiration, and the state of the heart, by the thrill which you experience. In some cases of difficulty of respiration, you find under respiration a thrill; and in certain obstructions of the heart itself, you find, on pressing over a part of the heart, a similar circumstance takes place, corresponding with the action of the heart. In some cases of aneurism of the aorta, and of diseases of the heart, you find by the hand a strong pulsation.

But although the sight and the touch give you this information, there is a third sense which imparts especial aid with respect to diseases of the chest, and that is hearing. The function of the lungs differs from the function of many other parts in this circumstance—it is performed with a noise, and so, likewise, is the function of the heart. The function of the brain and of the liver goes on without noise; but it is peculiar to the func-

tion of the lungs and of the heart that a certain sound should take place. If this noise take place in health, in sound physiology, you must expect that when the function of these parts is disturbed, the physiological sound will become pathological, that the sound will be altered; and such actually is the case. If this be the case, it is our duty to attend to it; if it be not the case, let those who say so prove that they are correct. We declare it is so, and we ask those who entertain a contrary opinion to listen for themselves.

As to the lungs, you will sometimes find the healthy sound of respiration cease altogether. A lung becomes impervious at a certain part, the air will not go through it, and this may arise from an obstruction to the course of the blood into the lung; but it may arise from the lung itself becoming consolidated, or from external pressure of the lung, the dropsical pleura preventing the lung from expanding. However, you may ascertain by this that respiration is not going on in that spot. Besides an absolute want of sound in certain diseases of the chest, you have in others an unnatural sound; the sound of respiration does not cease, but becomes strangely altered. You have information, also, not by listening only, but by striking. If you strike the chest, the nature of the functions of the lungs in health causes a hollow sound to be heard: the lungs are filled with air, and a hollow sound is necessarily produced. If something take the place of air—if the lungs become consolidated, or filled with fluid, or surrounded by it—it stands to reason that if you then strike that part of the lung, the air being absent, you will have a dull sound. This actually is the case, these things are physical necessities, they occur simply from the common law of physics; they must occur, and if they do occur we ought to observe them.

However, nothing would be more absurd than to attend to these auricular symptoms solely. The fault of those who merely employ the ear in diseases of the chest is just as great as the fault of those who do not employ it at all. There is a sufficient number of other symptoms frequently to point out the nature of the affection without employing the ear, but whether that is the case or not, it is as much our duty to attend to other symptoms as to those that we learn from the ear. You will, by looking at a patient while he is breathing, frequently find what is the state of respiration. You find that, like the pulse, it is more frequent than natural, or not so frequent as natural; we may have the *respiratio frequens* and *respiratio rara*, corresponding with the *pulsus frequens* and *pulsus rarus*. Now and then we find respiration performed suddenly, the inspiration and the expiration are sudden—and that is

called *celer*; and the opposite state, where the chest expands slowly, is called *respiratio tarda*, corresponding with the *pulsus celer* and *pulsus tardus*. These things may be learned simply by the sight. The respiration undergoes exactly such alterations as the pulse; it may be regular or irregular, it may be slow or quick, sudden or gradual. It may be deep or exceedingly shallow, little air being taken in at a time, and, on the other hand, you may have a full respiration. It may be full and deep, corresponding with a full and strong pulse. I do not mean to say that the function of the lungs and the pulse take place synchronously; but, as you have in the pulse quickness and slowness, fulness and smallness, jirking and sluggishness, so in respiration we have frequency and slowness, shallowness and depth, suddenness and sluggishness, irregularity, and so on.

When respiration takes place with difficulty, it is called, in medical language, *dyspnœa*: and if it be so difficult that the patient cannot breathe unless he sit upright, then it is called *orthopnœa*. *Orthopnœa* is merely an intense *dyspnœa*.

Independently, however, of the frequency of respiration or its slowness, you may hear particular sounds. You may hear a wheezing sound, which is called *sibilous*, or you may sometimes hear it performed with a snorting sound, which is called *sonorous*. Even without putting the ear to the chest, or employing a piece of wood as an intervening substance, it may sometimes be heard when standing at a little distance.

With respect to the *voice*, that affords you also much information. The voice is frequently suppressed, faint, harsh, and shrill; and these various changes will indicate great debility, or they will indicate particular diseases of the lungs themselves, or of the larynx, or of the air passages at large. From listening to the voice with the ear, in contact with the chest, or with the intervention of a piece of wood, or any similar substance, you will frequently ascertain that the voice does not sound as it sounds in general; so that you are able to say that an excavation exists in the lungs.

From a *cough*, too, with the naked ear we learn much, and the varieties generally enumerated are slight, severe, harsh, crowing, hooping, shrieking, tearing and hacking, loose and dry. There is an infinite variety with respect to coughs. Slight, severe, crowing, and hooping, I need say nothing about. You occasionally hear a cough tearing the patient to pieces, and it frequently indicates more disposition to spasm than actual inflammation. Now and then I have heard a cough which has been followed regularly by a shriek, so as to be absolutely alarming. At the end of every set of expirations, I lately heard one deep inspiration, not attended by a hoop,

as the whooping-cough, but by a shriek, as though the individual were about to have a dagger plunged into her.

In regard to the *heart*, you have great information by examining the chest itself; not only by the touch do you ascertain the force of the heart, but by listening you hear various alterations in its sound. Here again it is very necessary not to attend to any one symptom, or to the occurrence of any one sound, but to attend to the whole set of symptoms. Instead of the usual double sound which you hear in a healthy person, when the heart is affected in a certain way you hear a sound as if a pair of bellows were blowing, sometimes as if a file were in action, sometimes as if a substance were being rasped. If you attend to these only you may be led into error; but you frequently, at the same time, observe great difficulty of breathing on the slightest motion; you observe a swelling of the legs and a deficiency of urine, and you cannot but believe that there is organic disease. In other cases you only hear the sound, and then you would not be justified in saying there was organic disease, unless you listened time after time and always found the same occurrence. In observing these sounds of the heart, the greatest care is necessary not to form a judgment at your first visit. You may, by attending to the general symptoms, also see a sufficient proof of organic disease, but frequently there are no other circumstances present. These auricular sounds may arise from temporary causes, and you will not hear them two successive days. This shows that these symptoms, like those of the pulse, and almost every other, are never to be taken alone, but only in conjunction with other symptoms, and only when they have been dwelt upon carefully and repeatedly.

With respect to the *abdomen*—to descend still farther—the chief symptoms in that region are to be observed by the sight and the touch. The hearing gives you no great information respecting the abdomen, except in the circumstance of striking it, if it be enlarged. If you strike it, especially with the intervention of some little substance, either the finger or a piece of ivory, you learn whether the tumor arise from liquids or solids, by its emitting a dead sound as if you struck the thigh; whereas, if it arise from mere air, you find it sound hollow, like a drum. By going over the abdomen carefully, you may ascertain whether there is enlargement of any organ; and you may ascertain the shape of an enlarged liver with the greatest accuracy, the dullness occurring where you strike over the enlarged organ, and the hollow sound where the abdomen is in its natural state. This is the only point in which the ear is of much assistance. The ear is of the greatest use in diseases of the chest; but in the abdomen

you must more particularly have recourse to sight and touch.

Frequently, if you expose a patient in the case of abdominal disease, you may discover an alteration of figure which the hand cannot at once detect. Nothing can be more absurd than for a practitioner to prescribe at once for a patient who comes to his house for diseases of the chest and of the abdomen. It is very well to prescribe in that way for diseases of the skin, the head, and other affections; but with respect to diseases of the chest and the abdomen, if, in many cases, you do not listen, or have the patients more or less undressed, it is impossible to form a correct opinion. You may form a good guess, and you may be right; but you will not be able to form such an opinion as a philosophic mind would always wish.

In regard to the abdomen, by the sight you may discover enlargement, either local or general; but it is by the touch certainly that you acquire the greatest information altogether. You may feel an enlargement of various parts; you may feel the induration of different viscera; and, if there be fluid, you may ascertain its existence by the occurrence of fluctuation. But one great use of employing the hand to the abdomen is to ascertain the existence of inflammation; inflammations of the abdomen are detected easily in general—from the circumstance of the abdomen having no bony coats—by the hand pressing upon it; and it is always necessary, in acute diseases, carefully to examine the abdomen by means of the hand. In fever, and various affections of the alimentary canal particularly, if you do not carefully go over the abdomen with the hand, you may allow inflammation to escape you, which, if neglected, may prove dangerous. In fever, at any rate, it is right every day or two to feel the abdomen carefully, beginning at the epigastrium, which is the part most frequently affected, and going more or less all over it; but in every case where the abdomen is affected, you find it of the greatest importance to make these minute observations. It is nonsense to say that a physician is to use his pen and a surgeon his hand. Every physician must be more or less a surgeon, and every surgeon must be more or less a physician. It is as impossible for a physician to do without the use of his hand in the case of the abdomen, as it is for a surgeon in the case of fractures; and it is not creditable to any physician to pride himself upon not making these mechanical examinations, as I believe they are contemptuously called.

With respect to the *excretions* of the body, the sight and the smell give us the chief information. It is by the sight that we judge chiefly of the character both of the feces and the urine; the smell, however, gives certain

information in these particulars, as it also does with regard to the excretion of sweat. The sweat in many diseases is exceedingly sour, and in others it has peculiar smells, of which I shall hereafter speak. However, we sometimes are more minute in our observations respecting the urine; at any rate, it is necessary not to depend merely upon the sight and upon the smell, but to employ chemical means for ascertaining its quality. It is frequently requisite to ascertain whether the urine is acid, or neutral, or alkaline; it is right frequently to see whether it contains albumen or not; and in other cases it is necessary to ascertain whether it contains sugar. The most minute observations are sometimes necessary; and, of course, it is only with respect to the excretions that chemical minuteness is at all of service.

As to one sense, that of taste, I believe it can only be employed with regard to excretions, and I presume but with one of them—the urine. Some go so far in professional enthusiasm as to wish us to taste every thing; but it is not usual except in the case of the urine, and then, for the most part, it is better to get the patient to taste for himself. However, this is not *absolutely* necessary with regard to the urine, because by evaporation, and by weighing the urine, you can easily ascertain whether it contains sugar or not. I never yet in my life tasted it, but I have no objection, in diabetes, to taste the saccharine extract, for that is not urine.

Having gone through the chief of the organs of the body, I may say that the *whole surface* together gives such information as the head, but in a very inferior degree; we gain certain information, but it is undoubtedly far less than we gain from simply inspecting the face. From examining the whole body we learn whether there is a degree of fulness or emaciation; by observing its plumpness we can tell whether the patient is in condition or not. We observe likewise the dryness or the moisture of the skin, and the firmness of the flesh. We observe whether it is swollen or not by dropsy. It is to the surface of the body that we employ the sense of smell, in regard to the excretion of sweat. On the surface of the body we can ascertain the increased temperature of the patient; and for more minute observation a thermometer is employed. It is generally put into the *hand*, one of the two principal parts, or put under the tongue—a portion of the *head*. Every part of the surface of the body has its own affections; and it is there that not only the chief symptoms of cutaneous diseases—the symptoms of all its own affections, are to be found, but there often we observe affections of parts below. In inflammation, for example, of the *dura mater* from an injury, the surface immediately above frequently becomes *oedematous*. We

learn the state not only of the skin itself locally, but of parts beneath it, by looking at the skin immediately superjacent.

These are the chief observations which I have thought it necessary to make, generally speaking, on the third branch of general pathology, called *semiology*, or *symptomatology*.

The fourth division of general pathology is *therapeia*, or the general treatment and prevention of diseases.

Now in regard to most diseases nature has a tendency to get rid of them. It is ordained by providence, to a great extent, that the injurious causes to which we are exposed should have but a temporary effect, the body having the power of resistance, or they either existing temporarily, being applied but temporarily to the body, or being able to exert no more than a temporary influence. In this way it is that, generally speaking, we may say, although there are many exceptions, there is a tendency to shake off disease. This power of the body to shake off its morbid state is called by writers the *vis medicatrix nature*, or *autocrateia*. For example, if any thing too acid is taken into the mouth, a great flow of mucus and saliva is the consequence, which has a tendency to dilute the acid matter and wash it away. So with respect to acid matters taken into the stomach; the stomach has a tendency to reject them; or if they be passed through the stomach into the intestines, the intestines are excited to action, and they are got rid of. This is the general tendency of the frame.

Again, many causes of disease are only temporary. A person is exposed to a great cause of disease; the cause ceases, and, of course, in many instances the effect ceases likewise. It is not a necessary consequence, however, that the effect should cease because the cause is removed; but it very frequently thus happens. Again, there are other causes which cannot produce an influence upon the body, however long they are applied, for more than a certain period. If you take the poison of small pox, it produces a disease of only a certain duration; the patient may die, but if not, it can only last for a certain time; and when the disease has once occurred, the body, in the greater number of cases, becomes insusceptible of it again. These are the various means which nature has provided for getting rid of disease—for getting rid of noxious causes before they produce actual disease, and for getting rid of diseases themselves.

Some have so great a confidence in nature, that they leave every disease to itself, and that sort of treatment is called the *medicine of expectation*—*medicine expectante*. Such treatment does well in many cases; it is often the best in many cases, both medical and surgical, but it is totally inapplicable to a large number. No one with a violent in-

inflammation of the lungs would consent to sit down quietly in the hope that he would grow better day by day, when he knows that he may obtain immediate ease from the lancet, and get rid of the complaint. When a person has taken poison into his stomach, of course he would be mad to wait for nature to effect a cure; he would send for the physician's medicine, or for the surgeon's stomach-pump.

Now the treatment of the diseases of the human body is either preventive or curative; we either attempt to prevent disease from occurring, or we attempt to remove it; and when we cannot remove it we attempt to palliate it. The treatment of the human body in the way of preventing it becoming the subject of disease is called *prophylactic*; that which attempts to cure disease is called *curative*; and when we cannot attempt to cure a disease, still we may do a great deal in the way of palliation,—so that medicine is prophylactic, curative, and palliative. It is a great point very often to enable a person to live, while he must live, in comfort and happiness; and if he must die, it is a great point to enable him to die easily. In many cases we have to adopt both curative and palliative treatment together—that is to say, we aim at curing the disease, but we adopt palliative measures all the time, for the purpose of lessening any one symptom, or particular symptoms, which it is a desirable object to get rid of, or lessen, before the cure of the disease enables us to do so. For example, in the case of inflammation of the lungs, although we may attempt to cure the disease by bleeding, it is sometimes of the greatest importance to put an early stop to a violent cough by a narcotic.

In the treatment of diseases, whether it be prophylactic, (and it can hardly be called the treatment of disease when no disease exists; but we must allow an extended use of terms,) whether our means are to prevent diseases,—prophylactic, or whether they are to cure them,—curative; or whether they are to palliate them,—palliative; they may be of two kinds—they may be *rational* or they may be *empirical*. In the treatment of many diseases—not only in their prevention—we act rationally, we proceed upon general principles, and the whole treatment is in the highest degree philosophical. We make an accurate diagnosis, in the first instance; we then see what is the wrong state of the system, and we employ such means as are evidently calculated to remove that state. But in certain diseases, and in certain varieties of disease, we are obliged to act empirically—to act in a certain way without knowing why we are likely to be successful.

Now this empirical mode of practice is of two kinds—that is, sometimes we have a specific remedy for a disease without knowing why, and, in other cases, it is impossible to know the exact variety and cha-

acter of the disease, and we fire a shot at random that can do no harm, but which may do good. The treatment of inflammation is in the highest degree rational, the treatment of various spasms is the same, and the treatment of continued fever, too, I must think altogether rational. The treatment, however, of ague, of itch, and of syphilis, is empirical; for it is impossible to know why a few grains of sulphate of quinine cure ague, why a few grains of sulphur will cure the itch, and why syphilis will cease sooner if you exhibit mercury than if you do not. No one would *a priori* imagine that any of these remedies could have such a peculiar property; there is nothing in the nature of the disease, or in the nature of the remedy, that would lead us to any such conclusions. We also are obliged to be empirical frequently where we cannot ascertain the proximate cause of a disease. For instance, epilepsy arises, in cases innumerable, from an unknown cause situated in the head, or in distant parts. If we can ascertain that it arises from irritation, our treatment is rational, we remove this; if we can ascertain that it arises from an inflammatory state of the system, our treatment is rational, we remove this; but sometimes we cannot conceive that it arises from either of these sources, and then we use certain remedies that are known occasionally to do good; not that they are a specific against epilepsy, as bark is against ague, or sulphur against itch, but because they continually do good in the disease. They are not specifics, for they often will not cure one case out of many, and in many they are totally inapplicable, but still they frequently do cure the affection. I allude to copper, zinc, nitrate of silver, &c.

The mode of treatment which is pointed out from the circumstances of the case, is called the *indication*; and things which the nature of the case forbid us to do, are called *contra-indications*. To discover the indications which are necessary in the treatment of a disease, we must make a careful diagnosis of the nature of the case; then we must make an equally careful diagnosis as to the variety of the particular disease; and then we ascertain the stage of the affection, the exact strength of the patient, and the incidental circumstances—for example, we ascertain whether the place in which he lives at all accounts for his disease, or whether the season of the year is such as is likely to have affected him. We must likewise observe whether there is any peculiar constitution of the period. Sydenham used to mention the character of epidemics as the constitution of the year. Continued fever has sometimes been attended by so much tendency to debility, that evacuations were in the highest degree improper, and the patients did much better when they were allowed to take merely

simple food, with scarcely any other treatment. This has been the case during several months lately. If the patient were left alone, or treated with what in truth is no treatment at all, he has in most cases done well. If the least evacuation were attempted, the disposition of the disease was to great prostration of strength and rapid sinking. It is necessary, therefore, to know the constitution of the period; and, but for this, I certainly should have destroyed many patients by treating them as I had been accustomed to do; however, I was warned by the experience of others, and by careful observation of what was going on.

It is necessary also to know what are the contra-indications—whether there is any peculiarity in the patient. Some patients will not bear a remedy which is very appropriate to a disease; the disease may appear evidently to point out the necessity of a certain mode of treatment, but a peculiarity in the individual may render it exceedingly improper. This peculiarity sometimes relates not only to particular medicines, but to particular articles of food; and sometimes there may be some other disease present which forbids the employment of a remedy: for example, if a person have a hernia which cannot be well kept up, and you wish to give an emetic, you would try to do without it, lest you should force the intestines down too violently. You therefore see that you are not to be satisfied when you have given a name to a disease. It is a great point to make a good general diagnosis, but that will not do alone; you must make a more accurate diagnosis still; you must ascertain its minute variety, individual peculiarities, and all the other circumstances that I have just now mentioned.

In regard to the things indicated, they are divided generally into two classes—*regimen* and *pharmaceutical* means; the former consisting of injunctions in regard to the temperature of the place, to food, to exercise, and rest, and every thing of that description; and the latter respecting drugs. In prescribing medicines, it is necessary to recollect that, however excellent they may be, they are frequently rendered abortive by our want of attention to something else. There can be no doubt of the use of a large number of remedies, both drugs of the *Pharmacopœia*, and other remedial means; but I have seen them fail, not through any fault in themselves, but through something else that was not attended to when they were employed: for example, if mercury be given to arrest a violent acute inflammation, you cannot expect it to have that effect if you neglect to bleed the patient; and if, in addition to giving mercury, you do bleed him, you cannot expect to succeed unless you are particular with regard to diet. So, in diseases of the skin, there are many remedies which have a

particular operation which we cannot explain; they are not exactly specifics, but they do great good; yet the skin, in the midst of these diseases, may be in an inflammatory state, and if you do not bleed and enjoin low diet you cannot remove the affection. As no one symptom can be depended upon, so it is with remedies; and when you prescribe a remedy for a patient, you must take into consideration all the other points on which it is necessary to give directions.

Notwithstanding, however, the most scrupulous attention to all the circumstances I have just enumerated, you will often be disappointed in the cure of a disease; but you are not to be dismayed on that account. Our profession is capable of effecting the greatest good, chiefly in the way of preventing disease. By improvements with respect to cleanliness, with respect to air, with respect to food, and every thing else of that description, diseases are now scarcely known which formerly prevailed to a great extent; and we can do absolute good to a large amount in curing disease when it has occurred. A great many diseases are inflammatory; and over inflammation, by means of bleeding and other remedies, we have the greatest power. When we cannot eradicate a disease, still it is of great importance, and it is also a great blessing, to be able to mitigate pain; and it is to be remembered, that although we fail in the cure of many affections, yet, if we make the attempt, by degrees more knowledge will be attained, and those who follow us will be able to do what we cannot.

ON THE TREATMENT OF CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

SHOULD you deem the following hasty remarks worthy of insertion in your valuable publication, they are at your service.

E. STANLEY, M.D.

Brighton, Nov. 2, 1831.

In the treatment of cholera, it appears to me that sufficient stress has not been laid on the suppression of urine, and state of the urinary system, as shewn by dissections. If we consider the baneful effects caused by the constituents of urine being thrown back on the system, we shall, I think, be enabled in a great measure to account for some of the worst symptoms of the disease. Again, those remedies which at times have proved successful, such as

diluents, calomel with opium (a combination which, by preventing the former from running off by the bowels, may have operated on the urinary system,) hot baths and emetics, (by either of which a vicarious outlet by the skin was established,) &c. may have owed their efficacy to their directly or indirectly obviating that suppression. Besides, a return of the secretion and evacuation of urine being one of the most favourable prognostics, I should direct my immediate attention to a vigorous employment of diuretics:—oil of turpentine, in doses of 3j.—ij. repeated at short intervals, perhaps in combination with other essential oils, tinct. canthar. and other active medicines of that class; hot fomentations, blisters, or boiling water, to the perinæum; embrocations of oil of turpentine and cajeput, &c. to the spine, loins, and pubes, with a turpentine clyster, might, as far as practicable, be employed in conjunction with means calculated to cause a determination to the surface, (such as vapour or hot bath, &c.); and general bleeding, if not evidently counterindicated, or, as the immediate abstraction of blood from the *venæ portæ* system would seem desirable, leeches to the anus and right hypochondrium, or cupping to the latter. In some cases, especially where there is little or no vomiting, I should feel inclined to try the powerfully relaxant and revellent effects of tartarized antimony, in large doses, at shorter or longer intervals. From the colour of the blood, I am induced to believe the inhalation of oxygen would prove beneficial, and the depression of the vital powers might, perhaps, be relieved by the inhalation of nitrous oxide gas. As one of the remote causes of cholera may be a change in the electrical qualities of the atmosphere, electricity or galvanism, particularly the latter, might prove valuable auxiliaries. For the purposes of fumigation, the muriate of soda and sulphuric acid are, perhaps, the most convenient articles.

With respect to the question, whether cholera is contagious or not, I think it difficult in the extreme to come to a decisive conclusion; perhaps, it is idle to attempt it. The effluvia on which contagion seems to depend, probably vary in their quality and tendency to reproduce the disease, as much as symptoms do in number and degree; and again,

the susceptibility of those exposed to contagion, may be diminished or increased by numerous circumstances. Facts have almost indubitably shewn the disease, in many instances, to have been propagated by contagion; other more recent facts have proved that numbers have been exposed to it without becoming infected. This ought to convince the unbiassed, that the only conclusion we can arrive at is, that cholera may be either contagious or not, according to circumstances. Hence, while we ought not to neglect measures of precaution, we should at the same time use every means in our power to divest this fatal scourge of some of its terrors, and consequently to render individuals less susceptible of it, by dwelling on the fact, that many have escaped infection under very unfavourable circumstances, as well as on the probability, that if it should reach our shores at all, it may in this latitude appear in a milder form, both as to its contagious and other characters.

NON-MEDICAL TREATMENT OF CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

As all facts relating to the treatment of cholera are useful, whether derived from medical sources or otherwise, I send you the remedies resorted to by the Jews of Wiesnitz, who suffered less during the existence of the disease there, than other sects. My information is from a private letter to me, from one of the principal bankers and merchants of Frankfort S. M. and I give you the quotation *literatim*:—

“ One pint of strong Spirits of Wine;
 One pint of good White Wine Vinegar;
 One ounce of powdered Camphor;
 One ounce of flour of Mustard, or bruised Mustard Seeds;
 Half an ounce of ground Pepper;
 A full teaspoonful of bruised Garlic;
 Half an ounce of powdered Cantharides;
 Mix well in a closed bottle, and expose it to the sun, or some equal warmth, for 12 hours, frequently shaking it.

“ Let the patient be immediately put to bed, under warmed blankets (cover-

lids,) and let his hands and feet be rubbed powerfully and uninterruptedly with the above lotion, after it has been made warm. During this operation, let the patient take a glass of strong drink, composed of two parts of camomile flowers, and one part of balm of mint," (I suppose, of the infusion.) "Persevere in this course, and at the end of fifteen minutes, at the utmost, he will break out into a profuse perspiration,—the patient's head and body being kept well covered beneath the bed clothes. The patient must be kept in this state between two and three hours, and must not fall asleep. After this, remove the extra covering from the bed, and he will fall into a slumber, which may last from six to eight hours, and be accompanied by a gentle perspiration. When he awakes, he will feel weak, but the disease will have entirely left him, and he will require but rest and moderate diet to restore him.

"After rubbing, the patient must not have a single finger above the clothes,—the slightest chill would cause his death.

"When the cramps in the stomach come on, we apply very hot dry bandages of bran and ashes to the pit of the stomach, and, when necessary, a bladder of hot water to the region of the navel.

"The great point is, to produce strong perspiration, and to restore the circulation, which is drawn from the surface of the body, and thrown with frightful virulence on inward parts."

Note.—I have learnt, also, that these intelligent people used camphor extensively about their persons and apartments long before, and during the raging of the disease about them.

I send you this non-medical communication without comment; the medical reader will know how to appreciate it, and the general reader will derive advantage from its perusal.

I have, &c.

JOHN S. GASKOIN.

Clarges-Street, Nov. 5th, 1831.

REMARKS

ON

INJURY OF THE LENS.

By R. T. HUNT,

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As the cases of injury of the lens, related by Mr. Mackenzie, comprise facts which are peculiarly illustrative of the advantages of extraction, in such injuries, I trust the following observations will not be considered entirely inapplicable.

The extraction of the lens when dislocated from its capsule, and lying in the anterior chamber, is a practice which has generally been advocated by those who have particularly attended to ophthalmic surgery, and indeed extraction of the cataract seems to have been first attempted in consequence of Petit's extracting an opaque lens, which had become so displaced as to occupy the anterior chamber. This plan of treatment appears, however, to have been confined to those cases in which dislocation of the lens and its consequences were the only injuries the eye laboured under. I am not aware that any writer had noticed the utility of early extraction in injuries of the eye, until the appearance of Mr. Barton's paper on that subject, in the *Medical Gazette* of March 20th, 1830. Since that period, a considerable number of complicated injuries of this organ have been thus treated at the Manchester Eye Institution, and the general result is such as will at least entitle this method to further consideration. It may be right to mention, that nine of these cases were published in the 4th No. of the *North of England Medical and Surgical Journal*, in a "Report of the advantages of extracting the Lens in such injuries."

When a considerable period has elapsed after the accident, previously to the examination of the patient, there is much difficulty in determining which of the diseased changes in other parts have been produced by injury simultaneous with that of the lens, and which are to be considered as consequences of the lens' displacement. For instance, a blow on the eye dislocates the lens

without immediately affecting the retina; this latter structure, however, gradually suffers, in consequence of the lens remaining dislodged, and amaurosis is the result. In another case, the blow which dislocates the lens causes concussion of the retina, and instant amaurosis. Suppose they are examined six weeks afterwards, when the injured eyes of both patients have become amaurotic; how can an observer then distinguish one case from the other? But let the lens be extracted, in such an instance as the former, as early after the accident as is possible; this will certainly prevent that injury of the retina which the displacement of the lens from its natural situation is stated as having produced. I have stated that amaurosis may occur, not only as an accompaniment of dislocated lens, but also as a consequence of such an injury. This is rendered more than probable by the frequent super-vening of this disease upon the operation for depression, in which the lens is dislodged with much less injury to other structures than when its displacement is caused by a blow. After the operation, patients frequently possess good vision for a time, which subsequently deteriorates, the case terminating in amaurosis. When, therefore, greater violence causes the dislocation of the lens, it is not assuming too much to state, that there will be at least an equal risk of the retina becoming affected. The lens was extracted in three of Mr. Mackenzie's cases; but although its removal appears, in every instance, to have relieved the pain and irritation which had previously existed, yet very little improvement of vision resulted, owing probably to the length of time which had elapsed before this practice was resorted to; for as the lens produced such severe symptoms, after the eye had been so long accustomed to its new position, it is evidently reasonable to conclude, that an equal, if not greater, degree of irritation must have existed soon after the occurrence of the injury. The author recommends extraction, not only in cases of dislocation of the lens, where other parts are uninjured except by its displacement, but also where other structures (the cornea, for instance) are implicated in the injury; but this recommendation is not clearly stated; it is merely implied in the treatment adopted, particularly in Case III., and in the observation, that "The lens

was extracted by Mr. Rainy with the same striking relief which extraction generally affords in such cases." Upon carefully examining Mr. Mackenzie's *Practical Treatise on the Diseases of the Eye*, in which no such observation occurs, I conclude that the practice has been adopted since his publication of that useful work; and it would be a great addition to the evidence, of the advantages of extraction in such instances, already before the profession, if one, who has long possessed and still retains such ample opportunities for the observation of diseases and injuries of the eye, would make public his favourable report of such treatment.

Whilst writing the foregoing observations, a very unusual case of dislocation of the lens came under my notice, viz. that which Mr. Mackenzie calls the sixth variety of this injury.

Thomas Siddal, a weaver, ætat. 66, received a severe blow on the eye on October 18th, which caused much swelling of the eyelids, &c. After this had subsided, he found his vision injured, and applied at the Institution on the 24th. There is a semi-transparent tumor at the upper part of the eyeball, of a roundish form, about the size of a large bean, apparently produced by some body lying between the conjunctiva and globe, immediately behind the ciliary ligament. The cornea is perfectly free from injury; the iris detached from the ciliary ligament on the temporal side, and drawn upwards towards the swelling already noticed, so as to distort the pupil, which is rendered still more irregular by effused blood adhering to the lacerated iris. There is a slight degree of intolerance of light, and considerable vascularity of the sclerótica and conjunctiva; but the pain is much less severe than might be expected from the extent of injury. There is no constitutional irritation.

Considering the tumor as formed by the dislocated lens, I passed a cornea knife through the distended conjunctiva, when the lens was easily removed entire; it was perfectly transparent, and of the yellow colour usually found in advanced age. Its removal was not attended by any escape of either vitreous or aqueous humour, and the lids were kept closed by court plaster, in the usual manner.

Oct. 28th.—The progress of the case, up to the present time, is very favoura-

ble. He can now distinguish objects, and there is no appearance of opacity in the pupil; but when the extent of injury and the age of the individual are considered, it would not be judicious to expect that even the present degree of vision will remain. I shall reserve a more accurate report of the case until I am enabled to state the condition of the eye when a considerable period has elapsed since the occurrence of the accident.

OBSERVATIONS

ON THE

DEAF AND DUMB.

BY JOHN HARRISON CURTIS, Esq.

THE condition of the deaf and dumb has of late become a subject of more than usual interest. An opinion having long prevailed that such afflictions are of rare occurrence, the public sympathy has been powerfully excited by the correct statement promulgated respecting the comparatively large proportion of individuals labouring under partial or total privation of hearing, and of speech, in the united kingdom. For the reception and maintenance of sufferers, so strongly entitled to the compassionate aid of the benevolent, many institutions have been founded, but they have all proved inadequate to prepare the deaf and dumb for general intercourse with society. Another disadvantage attending them is, the regulation by which no applicant is admitted before the age of nine years, for it not unfrequently happens* that four, five, and even seven children in one family are so afflicted as to possess equal claims on these specific charities.

Having been led to consider the great number of sufferers, in comparison with the establishments provided for their reception, I ventured to propose a plan, which was first suggested by me in 1817, to the Governors of the London Deaf and Dumb Asylum. On that occasion I recommended, as a measure of primary importance, a minute examination of all children born deaf and dumb, with the proviso, that for every child

presented for admission, there should be produced the report of a surgeon upon its case, as being curable or otherwise, and I advised that none but incurable cases should be admitted. Under this regulation, it appeared to me that the worst cases would be most certain of reception; and that in all curable cases an opportunity would be given for trying such methods as might appear best calculated to afford relief, if not effect a perfect cure.

From the abstract of a report made by M. Husson to the Royal Academy of Medicine in Paris, on the method adopted by M. Itard for the cure of the deaf and dumb, it appears that M. Itard has presented to the minister of the interior *three memorials*; the first relating to the various methods hitherto employed for the cure of congenital deafness, including those pursued by the author himself during a long course of practice; the second, containing a detail of experimental treatment adopted in nearly two hundred cases, with the view of determining the advantages and disadvantages of injections through the eustachian tube into the internal ear, an expedient apparently recommended to public confidence in a recent report of the institution; and the third, adducing arguments against the last-mentioned process, and proposing one which M. Itard considers as exclusively entitled to preference, being a medico-physiological method, calculated in his estimation to afford relief in very many cases of congenital deafness. To the last of these alone the present address refers, for according to M. Itard, absolute deafness is extremely rare; he admits not more than one-fifth of the cases that occur to come under that definition. Every endeavour, then, should be made, to relieve any curable defect or obstruction, before a case is abandoned as hopeless. It should be recollected, that to restore to society one who suffers from accidental causes, and to render him useful by the scientific development of his natural powers, is much more satisfactory and laudable than to lead him through the indirect and exclusive forms of an artificial and symbolical education, however expedient this may be with regard to desperate cases, for which all practicable resources have been tried, and have proved unavailing. In offering these observations, as confirmed by daily practice and experience, I have no hesi-

* Vide Report of the London Deaf and Dumb Asylum.

tation in declaring, that the more I see of these afflicting cases, the more I am convinced that much relief may be administered, and that not one should be relinquished as desperate until all means have been tried to remove the defect. In very young children I have found the most frequent causes of deafness to arise from obstructions of the eustachian tube—from various affections of the membrane of the tympanum—from viscid mucus impacted in the bottom of the meatus, and, not unfrequently, from those herpetic eruptions which often afflict infants during dentition. Notwithstanding the general advancement of medicine and surgery, and the improved methods of cure for various diseases, it is to be regretted that those of the ear, though not numerous, are attended with more inconvenience than any others; and hence, until of late years, little attention has been paid to their relief, or, at least, little skill or ingenuity has been employed in multiplying and varying the expedients likely to attain that object. In considering the chief obstacles that present themselves, it is to be observed in the first place, that the structure of the ear is extremely complicated, the parts that compose it are principally internal, and the diseases that assail it chiefly concealed from view: hence, in very many cases, arise the uncertainty of diagnosis, and the difficulty of adopting a proper course of practice. In the second place is to be noticed the extraordinary prejudice which, ascribing all congenital deafness to malformation of the organ of hearing, denounces the disease as incurable, and depreciates all attempts at relief as at once nugatory and fallacious, or, at least, painfully uncertain. A third objection, which has been often urged, is the difficulty of applying remedies immediately to the parts affected. Yet means of relief may be employed without inconvenience, and often with success. Congenital deafness, and that which is observable soon after birth, frequently depends upon casual circumstances. In the ear, as in every other organ of the body, cases of defective formation may present themselves; and in these, deafness is incurable. But the obstruction of the eustachian tube, the most frequent cause of this disease in young children, is generally removed without difficulty. At the Royal Dispensary for Diseases of the Ear, as well

as in my private practice, I have had opportunities, which have not occurred to any other practitioner in the metropolis, of examining different applicants, for, perhaps it is not generally known, that the governors, in order to afford more prompt and extensive relief, have often, with the most benevolent views, ordered that all deaf and dumb patients should be admitted without recommendation; I have thus been enabled to try every mode of treatment, and none has appeared to me so efficacious, after having had the ears well syringed, (an operation rarely performed on deaf and dumb children,) as blisters and gentle emetics. The following I have generally found most successful:—

R. Antimonii Tartarizati, gr. ij.

Pulv. Ipecac. gr. xxiv.

Aquæ Distillat. ℥vj. M. fiat mistura.

In order to suit the medicine to the patient, a dessert spoonful is given every five minutes, until it operates.

To illustrate these observations, the following cases are given:—

Mary Haines, aged four years, was admitted as a patient at the Royal Dispensary. She has been deaf and dumb from her birth. She was the only one of her family afflicted with these defects, though her parents had five children besides herself. She was much emaciated, and in such a state of general debility, that I had considerable doubts whether she would survive. On examining the ears, I found a great deficiency in the ceruminous secretion. Her general health being so deranged, the first indication was to endeavour to invigorate the constitution, and this, by proper means, was happily accomplished. The usual remedies, such as blisters, injections, and other topical applications, were then employed, with the occasional use of purgatives. By the prompt and judicious adoption of these means, I am happy to state, she has acquired the inestimable faculties of speech and hearing.

Mary Anne Hagne, on recovering from a severe fever which had endangered her life, became deaf and dumb. She was brought to me when she was seven years of age; her case seemed of the most hopeless kind, and nothing but the strong importunities of her mother could have induced me to make attempts for her relief, and persevere in the treatment. After a number of ineffectual

fectual essays, and various modifications of my plan of proceeding, I had the satisfaction to restore her to hearing and speech. This case strongly exemplifies the advantages of perseverance in the treatment of these complaints, and demonstrates the necessity of repeated and prolonged trials of the various remedies, even in the most discouraging circumstances, especially when it is considered, that the faculties which we seek to restore, are of primary importance, and will qualify the patients to become incalculably more useful to themselves and to society. In regard to this child, I may also remark, that her speech was very probably improved in consequence of her being at a common school, for we are well aware how forcibly the principle of imitation acts upon children, and what great efforts they will make to perform whatever they see done by others.

Selina Hewitt was admitted a patient at the Royal Dispensary for Diseases of the Ear, afflicted with congenital deafness and dumbness. It was with some hesitation, in regard to success, that I undertook her case; but after commencing a plan of treatment similar to that adopted in the former instances, I soon observed indications of gradual amendment, and she has made such progress as to be able to hear and speak, much to her own satisfaction and to the heartfelt gratification of her friends, who awaited the result with great solicitude.

Thomas Mayo, aged two years and a half, had become deaf and dumb from accidental causes. When he was about a year old, the servant who had charge of him, having taken him out for an airing, imprudently detained him in the park too long on a very cold day, the consequences of which exposure were febrile symptoms, accompanied with sore throat. On examination, I found the eustachian tube obstructed, probably from inflammation when labouring under the sore throat; there was also a discharge from the meatus. I employed the emetic mixture, which was repeated several times, at intervals of a week. This completely removed the obstruction; the discharge was cured by means of injections, and a slight alterative course of medicine. The child is now able both to hear and speak.

James Lawlor, a boy about five years old, was sent to the Dispensary, from Ireland. His mother, who accompa-

nied him, informed me that he was born deaf and dumb. He was also blind of one eye. Finding, on examination, an obstruction in the eustachian tube, I ordered the emetic. When I saw him, three days afterwards, his mother informed me that he could distinguish loud sounds. The emetic was consequently repeated, with even greater apparent success than before; and as he continues under my care, I anticipate the most favourable results. I adduce this case as a proof that slight affections, if early attended to, may be treated by this curative process with the fairest prospect of ultimately effecting the total or partial removal of the malady. It is requisite to repeat the emetics frequently, in order to remove the collections of mucus which lodge in, and obstruct, the tubes. The period required for the success of such means, must vary in different cases, according to the degree of obstruction. No remedy is so suitable for the purpose of removing these obstructions as emetics; of this every well-informed practitioner must be convinced, when he considers the nature of their action. The operation of emetics is not, as is imagined by the vulgar, confined to the evacuating the contents of the stomach; these remedies produce great increase of the secretions of the mouth and throat, as well as of the juices of the stomach; by their action the biliary and pancreatic ducts are emulged, the secretions of the liver and pancreas are augmented, as well as those of the glands in the vicinity of the mouth and throat; stagnant fluids are put in motion; morbid congestions and accumulations in various parts of the system are removed; the excretion of mucus from the lungs is promoted; the general absorption of the system is increased; the circulation is rendered freer; the blood is diffused more equally over the system, and topical determinations are, in many instances, lessened or removed; the articular and pulmonary exhalations are very sensibly promoted; an action is also excited in the nervous system, which is often highly salutary in nervous disorders. The excitement produced rouses the sensibility of every part, without producing debility; and thus the different minute parts of the organ are enabled to recover their energy, and structural mischief that may be forming is prevented.

Soho-Square, October 20, 1831.

BOTANY OF NEW ZEALAND;

BEING A

Description of Trees, Plants, &c. indigenous to that country.

By GEORGE BENNETT, Esq. M.R.C.S. &c.

[With Engravings.]

Edwardsia Myerophylla, SALISBURY.
Sophora tetraptera . . . FORSTER.
Kowhy, or *Kongia*, of the natives of NEW ZEALAND.

Natural Family, *Leguminosæ*.

Class, *Decandria*. Order, *Monogynia*.

THIS tree is the *Kowhy*, or *Kongia*, of the natives, and attains the height of from forty to fifty feet, but it rarely attains more than five feet in circumference; it is of very irregular growth, and is seldom seen straight; it is found in good soil. The wood is hard and durable, and is used principally by the natives in the manufacture of their paddles, and instruments for digging, &c. The flowering season is in September, and the two or three subsequent months.

There are two species at New Zealand, both of which have been correctly figured in several botanical works.

CRYPTOGAMIA.

Asplenium lucidum . . . FORSTER.
Uru-urn-fenua of the natives of NEW ZEALAND.

It is difficult to assign a reason why certain trees and plants are held sacred by Pagan worshippers, as the mistletoe (*Viscum album*) among the Druids; the Tamanu (*Calophyllum inophyllum*) and Aito or Toa (*Casuarina equisetifolia*) among the Tahitians; the *Ficus religiosa*, and others among the Hindus; and the Fern now under observation among the New Zealanders. To queries on this subject, an answer is usually received of "its being the custom," probably originating in some lost tradition, or else some superstitious legend is found attached to them.

This species of *Asplenium* is a sacred plant among the New Zealanders, and is called by them *Uru-urn-fenua*; it is used by the *Tohunga*, or Priest, when

praying over a sick person; he waves (during the time he is endeavouring to avert the anger of the gods by his prayers) a frond of this fern; if it breaks it is a sinister omen, and the sick person will not be expected to recover.

When the *Tohunga* consults the spirits previous to engaging in war, he also waves a frond of this fern in his hand during the time he offers his prayers; if it breaks it is considered that the spirits are adverse to their engaging in war, and the enterprize is consequently given up.

It is also used by the natives as a badge of mourning. When a wife mourns for her husband, she sits wailing in her hut, with a frond of this fern, bound as a fillet around her head: a husband does the same when he loses his wife. They are careful not to burn, or use this plant in cooking their food.

When a Chief has his hair cut, the *Tohunga*, or Priest, is present. After the operation is performed, the Chief holds a frond of this fern in his hand, during which the Priest prays over him, frequently taking the frond and shaking it; after which it is dipped into water, and shaken over the Chief; if it breaks it is regarded as a sign that the life of the Chief will be but of short duration; and if one of the leaflets only is broken off, it is expected that one of the family will soon die; if the frond remains entire, it is an indication of success and long life.

The fronds are usually two feet in length.

Ephialis Sp.

Puredi, or *Kauwéré*, of the natives of NEW ZEALAND.

This is an unpublished species of *Ephialis*, and is figured in Parkinson's Botanical Drawings, made during Cook's first voyage, among the collection of Sir J. Banks now deposited in the British Museum. It is the *Puredi*, or *Kauwéré*, of the natives of New Zealand; seldom attains more than the height of forty feet, but in circumference is usually from ten to twelve feet. The wood is very hard, and is seldom used, as it injures the axes on attempting to cut it. The wood is of a yellowish colour, and yields a yellow colouring sap on being cut, and when washed. It is not used by the

natives, however, as a dye, or for any other purpose. The flowers are elegant, drooping, and of a pink colour. The fruit is a drupe, the seed inclosed in which has on it numerous irregular projections; the fruit is of a

bright red colour. The following drawing was made from a recent specimen at New Zealand. The fruit is delineated of the natural size. (a) The four stamina attached to the calyx; (b) the pistil.



Daerydium cupressinum, BANKS and SOLANDER.

Remu, of the natives of NEW ZEALAND.

Natural Family, *Coniferae*.

Class, *Diacia*. Order, *Monadelphina*.

This species of *Daerydium* is named *Remu* by the natives of New Zealand; it attains the height of eighty to ninety feet, but seldom exceeds fifteen feet in circumference. The timber is hard, of excellent quality either in plank or spar, and is considered the hardest timber of all the New Zealand pines. This species is not found so abundant as the other pines, growing only in particular districts. The wood in colour is an intermixture of white and red; no gum-resin exudes from it. The fruit is eaten by the natives, and was described by them as a small red berry, containing a black seed. This species of *Daerydium* is the one mentioned by Captain Cook as the spruce fir of New Zealand.

It is figured in Parkinson's Drawings, made during Cook's First Voyage, in Sir Joseph Banks's collection, now deposited in the British Museum, from which the engraving in Cook's First Voyage seems to have been made.

Phyllocladus Species.

Tánakáá Táwai, or *Toatoa*, of the natives of NEW ZEALAND.

Natural Family, *Coniferae*.

Class, *Diacia*. Order, *Monadelphina*.

This tree, the *Tánakáá Táwai*, or *Toatoa*, of the natives of New Zealand, is an unpublished species of *Phyllocladus*; it attains the height of sixty to seventy feet, but seldom exceeds fourteen or fifteen feet in circumference. The timber is heavy and hard, of good quality either in plank or spar, and of a white colour, but not equal in durability to the Kowri, and its weight renders it less valuable for spars. It sinks in water; a gum-resin exudes from it, but in very small quantity.

The bark of this tree is used by the natives of New Zealand for dyeing a red or black colour*; the mode of pre-

paration is as follows. The bark is pounded, and then placed into a vessel of cold water, into which hot stones are placed until the water boils*. After the bark has been boiled for some hours, the liquid becomes of a dark red colour: it is then left to cool, the decoction strained, and is ready for use. The Flax (*Muka*) to be dyed is then placed into this liquid; and after remaining for some time, when taken out, is found of a red colour; it is then placed into the sun to dry. This red colour is not indelible, but will stand frequent washings before it comes out. When the flax is to be dyed a black colour, after having undergone the before-mentioned process, it is placed in mud, (usually from marshes, &c.) and remains in it for the space of twelve hours, after which it is taken out, dried, and is found of a shining black colour†. The bark of this tree is generally used in the recent state, but its virtues also remain when dried. The beautiful black colour of the flax used in the manufacture of some kinds of the New Zealand mats, is procured by the above-mentioned process. The bark of this tree is sometimes stripped off by the natives, the outer part removed, and the inner bark, which is of a dark red colour, is worn round the waist as an ornament, but is not of any durability.

Podocarpus Species.

Mai, or *Matai*, of the Natives of NEW ZEALAND.

Natural Family, *Coniferae*.

Class, *Diacia*. Order, *Monadelphina*.

This tree, the *Mai* or *Matai* of the natives of New Zealand, is an unpublished species of *Podocarpus*. It attains the height of from eighty to ninety feet, and from ten to twelve feet in circumference; it is scarce in the vicinity of the sea, but is found abundant inland. The wood is of excellent quality, of a red colour, resembling cedar. One tree was cut down at Wyshaki Cove,

* This is the native mode of heating water, as they have no vessels which can be placed on the fire.

† I know not, on analyzation, of what chemical properties (but probably salts of iron are held in solution) the marsh mud may be found composed, but it will be an interesting subject for further investigation. In Ireland it is customary among the poorer people to steep the undyed woollen cloths in the water of the turf-bogs, which dyes the cloth of a dark colour.

* I believe that a black dye is wanted in this country, which, on exposure to sea air, will not lose its colour; if so it is to be found in the black procured from the bark of this tree, as the New Zealand mats dyed with it do not fade even when washed in salt water, but it rather gives a greater brilliancy to the black.

River Thames, from which I procured specimens; it measured thirty feet in height, and six feet in circumference. The torins, or New Zealand flutes, (which are tastefully carved,) are made from this wood; they are not hollowed out, but formed of two portions, which are accurately joined, tightly bound with cord made from the New Zealand flax, and the joined portion well luted.

London, Nov. 4th, 1831.

MEDICAL GAZETTE.

Saturday, November 12, 1831.

“Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso.”—CICERO.

MODE IN WHICH CHOLERA IS PROPAGATED.

THE only rational source of confidence and tranquillity under the existence of an approaching danger, is a thorough acquaintance with its nature, and a timely adoption of the measures requisite to avert its actual presence, or if this cannot be, at least to limit its extent, and controul its influence. On this ground it is that we think the public ought to be put in possession of all the most correct and useful information which can be obtained on the subject of Cholera. Now the two most important points to determine are, first, the manner in which the disease spreads, that we may know how to avoid it; and, secondly, the best mode of treating it, should our previous efforts at prevention fail.

The first of these—or the manner in which cholera propagates itself—is a consideration of vital importance to the interests of the community; and it is one with regard to which a large portion of the press has done, and is continuing to do, mischief which may prove irreparable: we allude to the efforts which are made to convince the public that the disease is not contagious. The

motive of the writers we doubt not to be good, but they err lamentably in judgment, because the evidence against their views is of the most convincing description to an unprejudiced mind, and because the direct tendency of their doctrines is to lead those who adopt them into unnecessary danger. Let it not be supposed that we are among the “terrorists,” or that we concur in the sentiments of the panic-stricken writer in the *Quarterly*, whose recommendations, if adopted, would sever every bond which links society—would cause the parent to abandon his child, and the child his parent—converting the land into a scene of distrust and desolation worse, infinitely worse, than has resulted from the disease itself even where its ravages have been the greatest. But, because others may have magnified the evil, is that any reason why we should shut our eyes, and refuse to look upon it as it really is? If small-pox were raging epidemically, should we not tell the public that the surest way to escape was to avoid the source of contagion?—precisely on the same principle, then, we urge the imperative necessity of their being made aware that cholera is propagated, and, consequently, may be avoided in a similar manner; and we pledge ourselves to prove in the sequel how small the individual risk is if proper precautions be taken, and how great the peril if they be neglected.

The first point, then, to determine is, whether it be contagious or not; and here we would premise that many of those who argue this point are wholly incompetent to give an opinion upon it at all*. But instead of interrupting our narrative by arguing with them here, we proceed to grapple with those who are of the medical profession, and who, better acquainted with such subjects, still prefer the fanciful and incon-

* See what we have said further on this subject at p. 214.

gruous explanations, which their ingenuity enables them to offer, of the phenomena regarding cholera, rather than adopt the plain straight-forward path to which undeniable facts and common sense alike conspire to lead them.

First, then, we hold that the great majority of intelligent persons, who have had opportunities of witnessing the disease, or of examining on a large scale the evidence afforded by others, are of opinion that it is propagated by contagion. This has not only been denied, but the very reverse has been asserted by the anti-contagionists to be the case; and as it is a question, not of reasoning, but of recorded facts, we shall proceed to prove that we are right in our assumption by quoting the opinions of the best authorities, as stated in various public documents.

The disease, it is known to all, whether it had previously existed or not, first attracted notice, and excited alarm, in 1817, when it broke out in the Delta of the Ganges, and thence gradually over-spread the whole of India. Now it has been said that the Anglo-Indian practitioners almost universally regarded the disease as non-contagious; and it is true that the majority, though by no means all of the regimental surgeons in India, came individually to this conclusion. It was deemed expedient, however, to appoint at each presidency a Board of the more experienced and intelligent medical officers, to examine and collate all the returns transmitted to head-quarters, and to sift the whole body of evidence upon the subject. The result was, that at Madras and Bombay the disease was unequivocally admitted to be contagious. At Calcutta, on the other hand, no report was ever drawn up by the Board collectively, but merely by one member of it, Mr. Jamieson, who gave his own individual opinion against contagion. While, however, he did this in one part of his paper, he expressed himself in another in such a

manner as to shew, either that his opinion was not perfectly made up, or that he actually admitted the doctrine of infection as existing when emanating from a considerable number of persons. The following quotations will shew that we represent this matter correctly:—

Bombay Report.—“It appears, then, incontrovertible that this disease is capable of being transported from one place to another, as in cases of ordinary contagion or infection, and also to possess the power of propagating itself by the same means that acknowledged contagions do.”

Madras Report.—After citing numerous illustrations of the infectious nature of the disease, and alluding to the objections of the anti-contagionists, it is added—“Diseases avowedly infectious, such as small-pox, measles, &c. have not at all times the power of spreading epidemically; for while it is certain that their exciting causes are never wholly extinct, it is only at particular periods that these diseases become epidemic. The same may be the case with cholera. . . . The sudden appearance and disappearance of cholera, however, unlike the progress of known infectious diseases, is not admitted as being irreconcilable with the doctrine of infection, especially if the disease be of sudden invasion, after the application of the exciting cause.”

Bengal Report.—“This much, however, may be affirmed, from a review of the whole progress of the epidemic in this quarter, that the infectious medium, in whatever it consisted, was limited within a very circumscribed circle, and was very slowly extended. If, setting aside the circumstances militating against it, we take it for granted that the infection was truly received by the centre and Hansi divisions, from the detachments above-mentioned, we must believe that the disorder, although not communicable by contact from person to person, was so from one large body to another large body; and that, when the poison got head amongst a number of men, it assumed some new quality, so as, when mixed with the atmosphere, to become infectious. What constituted this additional quality, we cannot pretend to tell.”

When the disease at length made its appearance on the south-eastern fron-

tier of Russia, medical commissioners were appointed at Orenburg, and other places, with whom its mode of propagation was a primary object of investigation, and the result was, that "the local boards, as well as the physicians, have been fully convinced that the cholera does, in fact, disseminate itself from one man to another, and by this means travels from place to place*." Still holding on its course, the cholera ere long reached Moscow, and here a considerable number of medical men adopted the anti-contagion side of the question; in consequence of which a special commission was appointed by the Emperor, and the evidence adduced before them led them to report, that they did not think the disease was transmissible *by goods*; but they said not a word at variance with the opinion previously given by the supreme Board at St. Petersburg, that "it has resulted from observation that the contagiousness really exists," and that the contagiousness of cholera is "in some instances incontestible†;" and we learn that "at a consultation of forty of the most respectable physicians of this city [St. Petersburg], thirty-eight came to the conclusion, after mature deliberation on the documents laid before them, that the disease was infectious, and only two were of an opposite opinion‡."

To Moscow also repaired Dr. Albers, a Prussian physician, who stated to his government that the disease was contagious: the same view of the subject was taken by Dr. Walker, an English physician, resident at St. Petersburg, who had been sent to Moscow;—and MM. Moreau de Jonnés and Gravier transmitted to France a similar opinion.

When the disease manifested a tendency still to spread, other European governments, alarmed at its progress, em-

ployed commissioners to examine its phenomena, and to ascertain, if possible, its mode of propagation. Austria, Sweden, and Holland, sent intelligent persons to the spot where it was raging, and all with one voice have pronounced in favour of contagion. From England, Dr. Russel and Dr. Barry were dispatched; and if the opinion of the latter, who was previously known to be a contagionist with regard to other disputed diseases, may be received with some suspicion, no one can reasonably call in question his zeal in investigating, or his fidelity in recording facts. But even if his evidence were rejected altogether, no similar objection can be brought against that of Dr. Russel. Indeed his opinion, on the very principle which might invalidate the other, becomes of greater weight when we state that he had imbibed the doctrine of non-contagion in India, but that what he witnessed in Russia has removed his preconceived notions, and that he advocates this doctrine no longer*. At Warsaw it is said that the disease was regarded as not being contagious. By whom was it so regarded? By an English surgeon (Mr. Searle), who had already committed himself on the subject, having recorded his opinion in a work published some time before he went; and by a few Quixotic young physicians from Paris, who were so ignorant of the laws of contagion as to regard inoculating themselves with the blood, or tasting

* Official documents published at St. Petersburg.

† Official Report, published at St. Petersburg.

‡ Private letter from Dr. Russel.

* At the very moment we are writing we have received wet from the press the second edition of Mr. Orton's work, decidedly the best which has yet appeared upon the subject of Cholera. Like Dr. Russel he had rejected the doctrine of contagion, but can no longer resist the evidence in its favour. The following passage is highly creditable to his candour:—"It was not without astonishment that many of the profession in India heard that the Medical Board of Bombay, in 1818, held the disease to be contagious. My feeble voice, in common with the great majority, was raised in opposition to the—as it appeared—monstrous dogma; but the march of time and events, the great accumulation of facts and gradual removal of prejudices, have wrought in my mind the same revolution they have in many others: the opinion of the contagious nature of the disease has been gradually gaining ground even in India, and seems to be the general one in Europe—*Magna est veritas et prevalet*."—Orton on Cholera, 2d edition, page 514.

the evacuations of patients labouring under cholera, as an *experimentum crucis*; when it is notorious that no inference whatever can be drawn in favour of their views from these disgusting proceedings. Of these gentlemen we may remark that six out of about twice that number who went to the Polish capital, have perished*. Certain it is that both Diebitsch and Skrznecki, the commanders of the Russian and Polish armies, were satisfied that the pestilence was contagious: and the government at Warsaw, guided by the opinions of their most intelligent men, adopted the same view, as will appear by the following extract from one of their proclamations:—

“Ever since the battle of Iguinie we have had to struggle against that contagion, which attacked us not only in the camp, but even in our capital We were already congratulating ourselves upon having deprived our enemies of one means of injuring us, and upon having rendered a fresh service to Europe, but on the 26th of May the battle of Ostrolenka brought the two armies into contact, and the cholera once more made its appearance, as well among our soldiers as among the inhabitants of Warsaw†.”

So also with regard to Berlin, from whence we have the following account by Dr. Becker—himself, alas! subsequently a victim of the malady:—

“It may finally be stated, that the manner in which the cholera has appeared and spread in Berlin, perfectly warrants the conclusion, that this disease is always produced by a peculiar human effluvium, which in its mode of generation and diffusion shews some analogy with that of the *common typhus fever of Great Britain*, but extends its deleterious effects to a limited number of persons only, who are rendered eminently susceptible by cold, intemperance, fatigue, fear and anxiety, diarrhoea, and other previous diseases‡.”

* Viz. MM. Hugon, Collé, Michaud, Barrie, Grezes de Saintour, and Le Gallois. We are unable to say whether they all died of the cholera or not.

† Circular of the Polish Government respecting the cholera morbus.

‡ Private Letter from Dr. Becker.

And lastly, with regard to this part of the question, hear what an intelligent writer says, from a totally different quarter:—

“*Valeni del Mount*.—Having observed in the public prints an opinion put forth by some physicians that the cholera morbus, even when epidemic, is not contagious, I think it right to state, that, in this country at least, it is undoubtedly communicated in the same manner as plague or scarlet fever—by contact or approximation. Thus the thirteen cases of that malady that have hitherto occurred in this place, have been all of them distinctly traced to the above source*.”

But it will probably be asked, are there no places in which the local Boards regard the disease as non-contagious? There are. Opinions to this effect have been expressed at Riga and Dantzic; but we are inclined to place little reliance upon them; first, because no sufficient grounds of this exception to the more general sentiment are stated; and, secondly, because the extent to which both these places are dependent on their trade, renders some degree of distrust almost unavoidable as to the impartiality of their decision; just in the same manner as we have seen the existence of cholera at Sunderland denied, in letters from that place, published in the daily papers. Another non-contagionist presents himself at Dantzic, in the person of Dr. Hammet, who was sent there by the English Government. With regard to the communications of this gentleman, we can only say, that we think the Board of Health would have acted more judiciously if they had published them. We presume that they have been prevented from doing so by their extreme prolixity; still the illustration which they afford of the loose manner of reasoning adopted by the non-contagionists, would indirectly have strengthened the opposite side of the question, while

* Official letter from Mr. Blatte, the English consul, to Lord Cowley.

their suppression may lead those who have not seen them to imagine that they have been withheld on account of evidence against contagion—which, in fact, they do *not* contain.

We have thus shewn, then, the utter groundlessness of the common assertion, that they alone think cholera contagious who have not seen it, and that, on the contrary, the immense majority—not of illiterate persons, but of those capable of forming a judgment on the subject—have espoused the side we advocate. Nor is it a trifling addition to the probability of this view being correct, that it has been unanimously adopted by the Board appointed in London, numbering as it does among its members some of the most eminent of our civil practitioners, as well as those at the head of the medical departments of the army and navy.

But in coming to the conclusion at which we have arrived, that cholera is a contagious disease, we have not been influenced by these authorities farther than as an inducement to examine with attention, at the same time with candour, the evidence which had led so many to an inference, which, we are free to confess, was at variance with our views with regard to certain other diseases, which by many are regarded as contagious; and the following are the circumstances which have decided our opinion:—

There are three modes in which disease may pervade a district—it may be endemic, epidemic, or contagious. Now if we apply the characters of the two former to cholera, they will not be found sufficient to explain its phenomena; or rather, they will appear manifestly to be inconsistent with them.

The extent to which cholera has spread, and the diversity of the localities which it has occupied, at once set at rest the idea of its being what is usually understood by endemic; and there remains only that we should choose between the

two latter. Diseases which are epidemic, without being contagious, break out in distant places either simultaneously, or travel with a rapidity, and in a manner, that defies all calculation; cholera travels slowly and progressively. Epidemics march on the winds, and cannot be arrested; the progress of cholera has repeatedly been stopped. Epidemics generally prevail in tracts of country analogous as regards humidity, temperature, soil, elevation, or some other obvious similitude; cholera has exerted its dominion alike in the marshy jungles of Hindostan and the arid plains of Persia—the burning sands of Arabia, and the snow-bound provinces of Moscow. Epidemics, unaided by contagion, prevail for a time and disappear; cholera, like small-pox, scarlatina, and other undeniably contagious maladies, has never wholly left any country it once has visited; it still lingers in Bengal, where it commenced, and, in fact, wherever it has laid its envenomed hand, though it may occasionally relax its grasp, it still retains its hold. These considerations render it, *prima facie*, improbable that cholera should be merely epidemic. Let us now see what evidence of a direct kind there is of its being contagious.

Before doing this, however, we must request our readers to keep in mind, that the evidence of any disease being contagious is derived from a consideration of many facts, with regard to the circumstances attending its extension from person to person, or from place to place; and that, though perfectly demonstrative to men of ordinary reasoning powers, and whose minds are unprejudiced, it never is of that overwhelming and intuitive nature which forces conviction by a direct impression upon the senses. So much otherwise, that Sydenham, who witnessed the ravages of small-pox on a great scale, seems to have entertained no suspicion

that it was contagious; yet no reasonable man of the present day doubts that it is so. But though this be the mode of its propagation, and though it be thus *communicable*, it does not follow that it will in every instance be thus *communicated*. It is a remarkable fact, with regard to all the animal poisons, that some peculiar state of the individual is required in order to bring him under the influence of the disease: thus it is recorded by Dr. I. Hunter, that 21 persons were bitten by a rabid dog, and only one became hydrophobic. Does any one suppose that, because eleven escaped the effects of the poison, therefore the twelfth, who took the disease, did so from some other cause, and not from the bite? Yet this is precisely the argument of the non-contagionist with regard to cholera: the numberless cases in which the disease has directly followed exposure to some one having it are set aside, and some other explanation is offered as to the origin of the disease, because, in certain other cases, persons have been exposed to the same cause with impunity. How, then, is it with regard to complaints which universal opinion has now acknowledged to be *transmissible*—are they always necessarily *transmitted*? Take measles: probably there are none of our readers, who have had but a moderate share of experience, who have not met with this kind of case: measles breaks out in a family—it affects A, B, and C, successively, and, proving mild, the parents are anxious that all the children should have it; and, accordingly, a fourth child, D, is exposed to it by being allowed to eat, play, and even to sleep with the others; yet D refuses to take it, and passes the ordeal unaffected. Does any one thence suppose that measles is not infectious? On the contrary, we merely say that D was not at that time susceptible of the disease, and we know that, on another occasion, this same child may become affected with

measles, from a degree of exposure not nearly so great as that which had previously been resisted. So also, with regard to small-pox, an immense proportion of mankind are incapable, from the protecting influence of vaccination, of having this disease communicated to them; but does any one thence doubt as to the mode of its transmission to those who do receive it? Our object is to impress upon the minds of those interested in this question, that the fact of a disease being transmissible by infection, and so communicated to those who have it, by no means implies that it must of necessity be transmitted to all who are exposed to it. There is no instance of any disease, however infectious, in which this is the case; and therefore the argument against the transmissibility of cholera from man to man, founded on the notorious fact that many men are constantly exposed to it with impunity, is a complete *non sequitur*—absolutely illogical; and which, if admitted, would prove, by parity of reasoning, that neither small-pox, nor scarlatina, nor measles, nor any other disease, was either infectious or contagious. The kind of evidence, therefore, by which any disease can be shewn to be infectious, is not that every, or nearly every one exposed to it becomes affected, but that, on the one hand, its appearance in individuals, or in places, so frequently follows approximation to those who have it—or on the other hand, the exemption from its attacks, while it rages around, is so frequently preceded by a careful avoidance of the presence of those affected, as to render it impossible for us to give any rational explanation of the phenomena, without looking upon exposure to the presence and the subsequent occurrence of the disease, or the exclusion of its presence and the continued exemption from the disease, as standing in the relation of cause and effect.

Now, keeping in mind that the infec-

tious nature of a disease is to be deduced chiefly from the multiplication of probabilities, rather than from proofs approaching to mathematical demonstration, the first point to which we would direct attention is the mode in which cholera travels. We have already said, that epidemics overrun whole countries, either travelling with the wind, or at least with equal rapidity; but cholera journeys more leisurely. It proceeds from place to place successively; and, being at any given spot, it never jumps over any great intervening space to another similar locality, but, be the adjacent district moist or dry, high or low, healthy or unhealthy, if only there be men in it, and these have any intercourse with their fellows where it then is, the disease is sure to visit them. On the great scale it moves along the principal roads and navigable rivers, first affecting the towns and villages by the wayside, and subsequently extending to those more remotely situated. "Change of air and climate has apparently no influence on the progress of cholera*." "When once established in a marching regiment, it continues its course in spite of change of position, food, and other circumstances†." In short, it travels as man travels, stops where he stops, and proceeds again at the time, and in the direction, in which he resumes his journey. Moving along with the caravans of the east, it was approaching the city of Teheran, in Persia, when the Shah resolved to shut his gates against it. The travellers were obliged to make a circuit, in order to avoid the place; and thus did this sagacious Persian protect his domain from the visitation of this scourge for no less a period than eight years, though it raged on all sides around him; an example which shews what might have been accomplished had any precautions been

taken in India to arrest its progress; but unfortunately none whatever were adopted. The instance of Teheran is, we believe, the first in which the quarantine was fairly tried;—it was commenced in 1821, and it was not till 1829, owing to some remissness, probably engendered by this long exemption, that the disease at length gained admission. The Russians, too, arrested it on its first irruption at Astrachan, but allowed it to spread on its reappearance in 1830; and, indeed, after this no effectual attempt seems to have been made to stop it on the eastern border of Russia. Not only were travellers allowed to proceed, but "they who could flee the city fled, and as the malady was not considered contagious, servants, labourers, Tartars and Russians, were permitted to rush into the country*." From this time it branched off in various directions, according to the routes of the different fugitives; and there are some circumstances connected with its course which strikingly illustrate its disregard of every thing except the path followed by men. From Astrachan it proceeded up the Volga, but without touching the Don, till a cossack, who had been sent from the station named Katchalinskara on this river, to buy provisions at Doubooka on the Volga, died of the cholera on the 7th of August, after his return home. From that time the malady spread successively through the different Cossack villages along the Don†, and arrived at Azof in October. We request our readers to attend to this circumstance. The cholera travelled up the Volga a certain distance—was carried across to the Don, where it was till then unknown; and while on the former river it continued its course upwards, running nearly north-west; on the latter it

* Professor Lichenstein.
† Madras Report.

* Letter from a clergyman at Saratov. Quarterly Review, November.
† Crichton, &c.

held its way down (having first joined this river near its source), running in a direction nearly south-east. Thus during the month of September 1830, cholera was marching on two nearly parallel lines, but in opposite directions—on the one from south to north, and on the other from north to south—an occurrence easy of explanation, if we admit that it was carried by the persons travelling in these directions, but totally inexplicable on any other principle.

Again, in every instance where cholera invades a new country, it does so on a frontier next some district already infected; or, if it attack an island, it does so at some sea-port. Thus in Ceylon it first broke out at Jaffnapatam and Columbo, in Sumatra at Acheen, in Java at Batavia, in the Isle of France at Port Louis, in Bourbon at Port St. Denis, in *Britain at Sunderland*; and, in fact, the rule is universal; nor can any rational explanation be offered of the circumstance except that of the disease being imported. Persons on shore, in the neighbourhood of vessels having the disease, never take it unless some intercourse has taken place with the crew; and the converse holds equally true, that vessels arriving off an infected place remain free till they have had some communication with the shore. The illustrations of both these facts are too numerous to admit of any doubt. By sea then, or by land, we still find cholera accompanying man; and is not this a strong presumptive proof that it is carried by man?

But are there no exemptions from its so general influence—have no favoured places escaped its visitation? Yes, there are such exemptions—there are such places; and their history proves that there is one method, and only one, by which it may be turned from our doors. Exclude men, and you exclude this pestilence. To one example of this (that of Teheran) we

have already alluded; but the point is too important to be left without farther proof. Let any unprejudiced man read those which follow, and we think he will see in them powerful evidence in favour of our argument:—

“Many gardens and farms in the neighbourhood of Astrachan remained exempt from the epidemic, having broken off all intercourse with the diseased districts. In many villages, too, where similar measures of security were taken, the issue was equally fortunate, although the cholera raged all around them; for example, in the lordships of Smirnov, Beketov, and Prince Dolgoruki, in Sarepta, eight miles from Zaritzin, and some other places*.”

“The Moravian colony on the right bank of the Wolga, and several German colonies in the government of Saratov, around which the disease was violent, adopted the system of exclusion, and were also unhurt. At Caramala-Gubeewa some Russian peasants, living together, scarcely a hundred yards from the village, shut up their hamlet on the first report of the disease having appeared in their vicinity, and by enforcing a strict quarantine during the prevalence of the epidemic, remained in health†.”

“Mr. de Lesseps, the Consul of France at Aleppo, when the cholera approached that city in 1822, retired, in company with all who wished to be of his party, to a garden at some distance from the city. His asylum was inclosed with walls, and was surrounded by a large fossé; there were only two doors, one for entrance, the other for going out. As long as the malady lasted, he admitted nothing from out of doors without submitting it to the precautions observed in lazarettoes. His colony comprised *two hundred* persons, and consisted not only of Franks more or less acclimatised, but also of several natives. *Not a single individual contracted the disease*; while, at the very same time, within the city, four thousand beings perished in the space of eighteen days‡.”

“The cordons around Zarcozelo and Peterhoff were removed last week. We immediately visited these places, and

* Hawkins.

† Ib.

‡ Ib.

saw, for the first time, Sir William Crichton and Sir James Leighton. Both these gentlemen separately and positively asserted, repeated the assertion, and permitted us to note it, that no case had occurred within the sacred precincts of either cordon since their establishment, though the circle of demarcation was completely surrounded with the disease, and though the enclosure around Zarcozelo contained from 8000 to 10,000 souls*."

"Ever since the epidemic, the thoroughfare to Moscow has been through these little villages, which are separated by about half a verst, and contain about 200 souls. Yet, though at the village of Yshora, two versts further on, there have been several cases and deaths, and though there were so many at Colpina, there has not been a single case amongst the colonists up to this date [Aug. 8.]... The authorities say, that no precautions of any kind were taken; but it is remarked by Dr. Bowmann, that travellers to and from Moscow never halt at the colony; Yshora on one side, and Colpina on the other, being so much better resting places†."

"The large establishment composing the Academy of Military Cadets, at Moscow, was preserved by a similar plan from the scourge which was so active on all sides of it‡."

"There were 150 pupils on the officers' side (Cadets at Cronstadt), which is kept perfectly distinct from the school, for petty officers and sailors. The gates were shut on the 19th June, and as strict a quarantine as possible maintained to the 6th August (O.S.) No case amongst the pupils, who are from nine to twenty years of age§."

But if the effects of rigidly avoiding those labouring under the disease be thus remarkable, the manner in which it spreads when once introduced is not less striking. This can only be shewn by examples; and first, as to places.

"The cholera was brought to Astra-

can by ships, and it has spread itself over Russia from Astracan by the emigration of the inhabitants, principally those of the lower orders. This is the chief cause of its propagation in Russia; it has never shewn itself in any place except where it has been brought by travellers, who came from infected places. *We have not a single instance of a town, or of a village, which, without communication with houses or persons affected, has contracted the disorder.* Several places surrounded by the disease have preserved themselves from it by a rigid insulation*."

"From every thing we have been able to learn as to the progress of cholera morbus in the north of Europe—from its first appearance in the towns and villages of this country having been generally, if not always, preceded by the arrival of persons, or vessels, or both, from infected places—from the manner in which the disease has now broken out in this city [St. Petersburg], we see no other mode of accounting for its sudden appearance here than by concluding, that the harks from places on the Wolga, where the disease prevails, have brought something with them, which, disseminated in this atmosphere, has been the immediate cause of the eruption of cholera, which has just occurred‡."

"The first cases of cholera in Berlin occurred among the skippers on the boats lying on the river Spree, which flows through the town, and in houses in the immediate neighbourhood of the river. The disease has prevailed to a considerable extent in all those streets which lie along the navigated branch of the river, and whose inhabitants at the same time live in frequent intercourse with the skippers and fishermen. On the fourth and fifth days cases appeared in other parts of the city, and, in many instances, they were those of individuals who were known to have had intercourse with cholera patients, or at least with the boats lying on the river, and with the streets first infected‡."

Nor is the evidence at all less convincing if we direct our attention to in-

* Drs. Russel and Barry.

† Ib.

‡ Crichton.

§ Drs. Russel and Barry.

* Extract of a letter from Dr. Rehman, Physician at St. Petersburg, communicated to the Academy of Medicine, at Paris, by Dr. Marc.

† Drs. Russel and Barry.

‡ Private letter from Dr. Becker, of Berlin.

dividual examples of the mode of propagation. In the above illustrations, and in those which follow, we have preferred selecting instances which occurred in Europe, because, what is nearest us, both as to time and place, naturally makes the greatest impression on our minds.

"City Prison.—From the moment that the disease was proclaimed, the strictest precautions were adopted; no person was admitted without medical examination; rooms were set apart for a cholera hospital, and persons of both sexes appointed to attend the cholera cases, should any occur. Dr. Bish, who resides within the walls of the jail, and who, it must be observed, *was* an anti-contagionist, as acknowledged by himself, showed us a plan of the prison, illustrating the introduction of the disease amongst the prisoners, led us round the whole building (July 30), and communicated to us the following information from his journal, which had been most accurately kept:—“A woman had been sent out some weeks before to be treated for a syphilitic complaint*, in a public hospital. Her husband was also in confinement at the time, in a different part of the building, but remained. The woman was returned to jail, on the 23d June, O. S., with a diarrhoea upon her. She saw and embraced her husband for a moment, as she passed on to be placed in the room of observation. In a few hours she was seized with true cholera, and died that night. This was the very first case. The next persons attacked in the prison were three women in the same room with the former, one of whom had rubbed the deceased. These three died all within three days after the first. The next prisoner attacked was the husband of No. 1; he lived in a separate part of the jail. After this man, others in his room, all numbered on the plan, and registered in Dr. Bish's journal. In short, of twenty-seven attacked (fifteen dead), there is but one to whom communication cannot be traced. He was confined for a capital offence, and had less liberty than the others. There were about four hundred prisoners and attendants: the former are all well kept,

and treated with great indulgence. *None of the noble class, who are lodged in a separate part of the building, were attacked*.*”

Pensa.—“ ‘In a village of the government of Pensa, where this medical officer was sent in consequence of the breaking out of the cholera, to trace its origin and to afford medical aid, he learnt the following circumstances, which are attested by all the village authorities, and of which we are promised an authenticated copy, signed by himself:—The son of a villager, who was coachman to a nobleman, at fifty versts distance, died of cholera; the father went to the place to collect the effects of the son, and brought home with him his clothes, which he put on and wore a day or two after his arrival at his native village. He was shortly thereafter seized with cholera, and died of it: three women, who had watched him in sickness, and washed his body after death, were also seized and died of the disease: the doctor arrived in time to see the fourth case, and, finding that it spread on that side of the village, he had the common street barricaded on the side where the disease had not reached, and interdicted all communication to the two sides of the village, even for the purpose of going to church. In that side in which the disease first broke out, upwards of 100 cases of cholera occurred,—of whom 45 died, but the disease did not appear on the other side of the barricade†.’ ”

“ ‘The Navarino corvette, Captain Nachinoff, 200 men. She had been placed two miles to the eastward of Cronstadt, during the epidemic, to question and examine all craft from St. Petersburg. She had eleven severe cases of cholera, of whom eight died. Her first and second cases occurred on the 26th June, O.S. These two men belonged to the boat that examined the vessels coming from St. Petersburg, on board many of which they had been. The next men who fell ill were of those who carried the two first cases to the hospital in town‡.

Colpina.—The first patient was a

* “It is a rule, that this disease shall not be treated in the prison hospital.”

* Drs. Russel and Barry.

† Private letter from Dr. Russel.

‡ Dr. Russel and Barry.

non-commissioned officer of a ship's crew, who had arrived from St. Petersburg a few days before. No. 2 was an invalided workman; this man was noted for his bad conduct, and had been ill for at least a day before he was brought to the hospital. No. 1 had been in company with him, and had been drunk the day previous to his being taken ill. The woman, No. 3, had nursed No. 2 while at home; so that there appears a strong case of communication of the disease, which cannot be traced further*."

"A physician (Dr. Calow), who had attended cholera patients, being at the time in bad health, and labouring under diarrhoea, died of cholera; on the day following, his landlord (Mr. Steibelt), died; a day afterwards, two children of the landlord died, and the servant-maid was taken ill of cholera, and recovered. The landlord's wife had been removed to quarantine. There have been no more cases of cholera in this street or its neighbourhood†."

Instances of this kind might be quoted without number; but if what we have given do not convince our readers that the disease is transmitted from one person to another, probably no multiplication of the same kind of evidence would avail.

There is, however, one point connected with this part of the question which it may be necessary to set right before we proceed farther. It has been said, that if cholera were contagious, a larger proportion of medical men than of others ought to take it. Hear what happened at St. Petersburg:—

"Fifteen hospital physicians had been attacked, and six had died of cholera up to the morning of the 13th [July] instant, N. S., out of 264 medical practitioners of all descriptions, who were in St. Petersburg at the breaking out of the present epidemic‡."

Was it by chance, we ask, that the first fifteen medical men affected with cholera were hospital physicians §? But again—

"Though we have not yet obtained official returns of the number, we are satisfied, from the statements we have personally received in the numerous hospitals we have visited, that the proportionate number of attendants, of all descriptions, on the sick, who have been taken ill with cholera, is fully greater than that of the medical men*."

Besides—

"There were 70 attendants of all classes employed about the cholera hospital of the Foundling Hospital, of whom 15 were attacked; two prescribing physicians, one attacked. Dr. Duppe [the resident physician] was seized on the 22d [June, O.S.] He had seen no sick before the first case of the *garde malade* already mentioned†."

Such are the chief considerations which, after the most scrupulous investigation, and in opposition to preconceived opinions, have convinced us that cholera is contagious. Several very important questions connected with the circumstances which facilitate or retard its propagation remain untouched; but we have already trespassed too long on the patience of our readers, and must leave further observations for another opportunity. We shall therefore add but one sentence more. The first step towards arresting a disease consists in detecting its mode of spreading—if Cholera does so by contagion, as we hold to be the case, the means of checking it are obvious; if it depend on any thing in the air around, or in the earth beneath us, then no human means will avail aught in arresting it;—therefore our view, as it is the more rational, is also the more safe and consolatory; and let it be kept in mind that, even if we be wrong, still no danger is incurred to the public safety by adopting our opinions; but if the anti-contagionists be wrong, and their principles followed, the peril is imminent, the evil incalculable, and the

* Private letter from General Wilson.

† Private letter from Dr. Becker.

‡ Drs. Russel and Barry.

§ Ten other medical men became affected in a few days after.

* Drs. Russel and Barry.

† She was taken ill on the 19th.

extension and permanent residence of the malady in this country, its naturalization among us, will be the inevitable result; just as the same ignorance, or disregard, of the mode in which the disease is transmitted, led to its overrunning the whole of India, in which country, at the end of fourteen years, it still continues to abide.

[See Postscript, page 214.]

COMPARATIVE MORTALITY OF CHOLERA.

THE mortality of cholera has been very different in different places. According to an account given in the Russian State Gazette, the number cut off by the disease out of every 1000 inhabitants, during the first 48 days of its visit, has been:—at Lemberg, 51; Mitau, 34; Riga, 31; Posen, $16\frac{1}{2}$; Petersburg, $12\frac{1}{2}$; Königsburgh, $11\frac{1}{2}$; Elbing, $9\frac{1}{2}$; Dantzic, $8\frac{1}{2}$; Stettin, $5\frac{1}{2}$; Berlin, 4.

CHOLERA AT BERLIN.

IF it be recollected that the capital of Prussia contains 225,000 inhabitants, besides a strong garrison, the ravages of cholera in that city will not seem to be so very frightful. From the 31st of August to the 10th of October, the daily number of persons attacked was from thirty-five to forty; and the whole number during that period amounted to 1100, of whom 233 were cured, and 665 died—the rest still remaining under cure. Of the garrison, no more than ten soldiers were attacked, but eight of them died. The physicians and hospital attendants have been generally safe.—*French Paper.*

PREMATURE INTERMENT IN CHOLERA.

THE bodies of persons, apparently dead of cholera, have been in some instances observed to move. M. Londe, President of the late Warsaw Commission, has expressed his belief that many have been buried alive in the complaint.

SINGULARITIES.

AMONG the correspondence read at a late sitting of the Academy of Sciences,

in Paris, were the following singular items:—1. A cure for the cholera, by a M. Dutertre, who offers to lose his head at the Place de Grève if his remedy be not found superior to all that has ever yet been tried; 2dly, a letter from M. Garnier, announcing his discovery of the quadrature of the circle; 3dly, a letter, accompanied by a pretended *head of a flying dragon*, which M. Cuvier at once recognized to be the *head of a pig*; and, 4thly, a copy of verses on a new remedy for cholera!

ANALYSES & NOTICES OF BOOKS.

“L'Auteur se tue à allonger ce que le lecteur se tue à abréger.”—D'ALEMBERT.

Reports of Medical Cases. By DR. BRIGHT. Price 9*l.* 9*s.*

[Continued from page 158.]

PRESSURE OF THE BRAIN resumed.

THE next case is remarkable for the unusual rapidity with which it proved fatal. Mr. N—, aged 57, of spare habit and anxious disposition, enjoyed good health up to July 1st, 1830. He had been much fatigued, and went to bed at half-past ten, having taken a moderate supper. About half-past one he awoke sick, but seemed to be relieved by vomiting. His wife, however, was alarmed by his mode of breathing, and rang for assistance, when he was found in a deep apoplectic seizure; and before medical assistance could be procured he was dead—his decease occurring within twenty minutes from the commencement of his attack. The lateral ventricles were found distended with a large quantity of semifluid blood, which raised up the velum interpositum from the thalami. From this membrane the hæmorrhage seemed to have taken place, as it was rent at one part. The corpora striata seemed to be broken down in their substance to the extent of a fifth of an inch, and beyond this depth spots of ecchymosis were perceptible. The commissura mollis was torn, and the blood had descended into the third and fourth ventricles: the entire quantity might be above three ounces. The action of the heart had always been very forcible, but that organ appeared healthy in its texture, with the exception of a slight de-

posit in the aortic and mitral valves, but not to such an extent as apparently to interfere with their function. The coronary arteries were much enlarged.

In the preceding case the death took place with unusual rapidity for apoplexy, though still it was not so instantaneous as in rupture of the heart, or great vessels.

In the case which follows, death took place in an hour and a quarter from effusion of blood near the corpus striatum, tearing the texture of the brain and bursting through the arachnoid. The hæmorrhage appeared to have been copious and unrestrained, owing to the softened condition of the brain at the point where it occurred. Numerous other cases are given of apoplexy dependent upon effusion of blood, but in none did the fatal termination take place so soon, and in some it did not occur for several weeks. These cases were, in general, instances attended with some disease of the vessels. In two instances, where the patients had suffered repeated attacks of hemiplegia, affecting articulation and deglutition, and proving fatal at the end of several months, disease was found at the posterior part of the corpora striata. In another there was hemiplegia of the left side, and the cerebral lesion was not on the opposite but on the same side. As the case is short, we shall insert it.

Hemiplegia on the left side, with Cerebral injury on the same side.

"John Walker, aged 33, was admitted into Guy's Hospital October 14th, 1829, labouring under a severe attack of bronchitis; his countenance purple, his breathing very difficult, and his legs œdematous. He had considerable hesitation in his speech, owing to a paralytic affection of the mouth and tongue; and his left hand and arm were weak and obviously paralytic, though not to such an extent as to be useless. On inquiring from his relatives, we learnt that the whole left side was paralysed about a year ago, that the affection was sudden and for some time severe, but that he gradually recovered the use of his limbs to the present time. He survived only three days after his admission, and his death was quite sudden, so that he had spoken to the nurse within a few minutes of his dissolution.

"*Setio Cadaveris.*—The dura mater was perfectly natural. The arachnoid and pia mater shewed a good deal of vascularity, but of purple blood, arising from congestion;

very slight effusion beneath these membranes, which were stript easily from the convolutions, except on the lateral and outer part of the left hemisphere, where it was found impossible to detach them without lacerating the brain, to which they were glued by a thin opaque yellow flake of deposit, like fibrin: immediately below this adhesion, and extending to the middle of the corpus striatum, was an opaque portion of the brain considerably harder than the brain itself, plainly shewing in its centre that it was the scar of an apoplectic cyst, surrounded by hardened parietes. The cavity in this was scarcely so large as half a small French bean, containing a little softened curd-like substance; but the derangement from the injury evidently extended from the middle of the corpus striatum, the structure of which was much disturbed, quite to the surface of the middle lobe.

"The whole of the remaining parts of the brain, and more particularly those of the right side, were most carefully examined, but without finding the slightest trace of disease. The medulla oblongata was so cut into, that we did not ascertain whether there were any peculiarity in the decussation of the fibres."

There is every reason to believe, with Dr. Bright, that the diseased portion of brain above described was constituted by the remains of the apoplectic cyst, which had undergone a kind of cicatrization. A case in which the lesion and paralysis were on the same side, is recorded by Morgagni, Lib. I. Epist. 2. § 16.

The author next introduces some cases, illustrative of the effects of pressure acting suddenly, yet not proving immediately fatal. It is upon cases of this sort, which fortunately are not infrequent, that all our hopes must be founded of being able to prolong life, and often to secure even a pleasurable existence after the occurrence of severe attacks and of very unfavourable symptoms. Sooner or later, however, if their subsequent history be traced, it will be found that the disease has recurred in most instances, and ultimately proved fatal.

A woman, aged 33, had experienced, during some weeks, headache, vertigo, and occasionally dimness of sight, for which she was bled, and blistered on the neck, with very little relief. Upon awaking one morning, she found she could not move her left leg and arm as well as usual, and, in short, she proved to be afflicted with all the symptoms of

partial hemiplegia. She suffered much from pains in the side affected, particularly in the joints of the leg and arm. Pulse 108; tongue covered with white fur, moist, and slightly drawn to the left side. She attributed her illness to exposure to cold and wet. She got better in the hospital, but her partial paralysis was by no means cured when she was discharged. Those pains in the affected limbs, sometimes (and with correctness) ascribed to rheumatism, were also observed in two other cases; they appear, however, to be rather connected with the affection of the nerves on which the paralysis depends.

A man was taken to Guy's Hospital, who, while rowing the day before, was suddenly seized with insensibility, and, on recovering himself, found that he had lost the power of his right side, and was unable to articulate. He had been labouring under severe cough and dyspnoea for six weeks, and there can be little doubt but that this pulmonary disorder laid the foundation for the effusion of blood in the brain, which was immediately induced by the exertion he was making at the moment. By cupping in the neck, keeping the bowels open, and by the use of a combination of squill, grey oxide of mercury, and a little opium, he was enabled to go home in a few weeks much relieved.

Another instance of paralysis from cerebral congestion, arising from obstructed circulation in the lungs, but augmented by habitual constipation, is given by Dr. Bright, in the case of a woman who had been long accustomed to a most severe asthmatic affection, which used very often to prevent her lying down at night. She was also subject to occasional pain in the head, giddiness, and sickness. While suckling a child, some six weeks before she came to the hospital, she suddenly heard of an accident having happened to her son, upon which she fell into a sort of hysteric fit. This was followed by a loss of power and sensibility in the left thigh, and the arm of the same side was similarly affected. A few days after, the right leg became numb and powerless, so that she could no longer support her weight. By the use of the aloes and myrrh pill, and attention to her bowels, which were habitually confined, she was so much improved that, in about ten days, she could rise from

her bed and walk some distance; and she went on improving to such a degree that, though not completely restored, she was allowed to leave the hospital, at her own request.

We shall just give an abstract of one case more before we proceed to the author's general inferences. A thin, middle-sized man, 46 years of age, whose occupation was to work the steam-engine of Barclay's brewery, was admitted into the hospital, affected with hemiplegia. For some time previous he had suffered from occasional headache and vertigo; but it was not above two hours before his admission, that, as he was walking, he felt his right foot become very heavy, and then his right hand; his articulation also became very indistinct. Sensibility was very obscure on the whole of the affected side; indeed it might be traced accurately to the middle line of the body and head, including the ear and cheek. The mouth was drawn to the left side; the speech indistinct; the pupils acted naturally when light was brought to them; his intellects quite undisturbed; bowels costive; head more free from pain than for a long time previous to the attack. He was bled and purged freely, cold applications put to the nape of the neck, blistered, &c. Under this mode of treatment he gradually improved, and at the end of ten weeks left the hospital pretty firm on his feet, and with some power of his arm. It was ascertained, however, that some weeks after his return home he had another fit of apoplexy, in which he died. The history of this case, as Dr. Bright observes, is not very mysterious: there was here clearly long irregular distribution of blood, probably accompanied by diseased vessels; extensive sanguineous effusion into the left hemisphere, perhaps injuring the posterior part of the corpus striatum. This effusion having ceased, the process of reparation went slowly on; some fresh excitement induced increased flow of blood; the injured vessels again gave way; the effusion became more extensive, and the blood probably burst its way into the ventricles; but the exact circumstances of the patient's death are unknown. "Had he been contented," says Dr. B. "to remain quiet for some months longer, it is very possible that the injury would have been more completely heal-

ed, and that, with care, he might have lived many years; but his arm would never have been perfectly restored."

General Observations on Apoplexy.

With regard to apoplectic attacks generally, it will be observed, and the observation is borne out by a copious induction from the cases adduced by our author, that, in the first place, neither age nor sex affords security: a majority, however, of those attacked have been males, and these chiefly above the age of forty, and many above sixty years of age. The spare and tall, as well as the short and robust, have been subject to the disease, but not in equal proportions; the plethoric frame certainly seems to predispose to that irregular action of the arterial system, and to that tendency to congestion, on which apoplexy and paralysis so essentially depend. Intemperate and irregular habits of living have a large share in preparing the way for an attack; and accidental blows appear frequently to act as strong predisposing causes. Anxiety, mental exertion, and sedentary habits, likewise decidedly predispose. Many severe attacks occur without any previous symptom sufficient to excite alarm; but much more frequently some subordinate seizures have been experienced—momentary loss of consciousness, giddiness, slight defects in the sensibility of parts, frequent irregular or convulsive actions; and at other times sensations of beating within the head, or peculiar noises, or defective vision. On inquiring into the previous history, we not unfrequently find that strong evidence exists of disease in the heart and large vessels, or of great obstruction in the lungs, or of such derangement in the kidneys as is calculated to interfere very essentially with the healthy discharge of other organs.

"When the decided attack has taken place, it varies greatly in extent and severity: sometimes it is attended with violent pain in the head; sometimes there is not the slightest pain; sometimes it assumes the form of complete apoplexy, the annihilation of all consciousness, the extinction of all sensation, the loss of all voluntary motion:—for a few minutes, or a few hours, the retarded pulse bespeaks the difficulty with which the heart and arteries maintain, by their involuntary actions, the labour of circulation, and the ster-

torous sound or impeded breathing betrays the inactive condition of those muscular parts through which the involuntary powers are called upon to force the air in the process of respiration; and these soon prove insufficient to maintain life. At other times the apoplectic condition, though well marked, gradually subsides; or frequently the state of insensibility continues for a few minutes only; while in some attacks the consciousness is never destroyed. But though the apoplectic state should not exist, or though the consciousness should have returned, yet if effusion of blood have taken place, paralysis will generally remain. The extent of this paralysis will vary almost indefinitely; it frequently affects both the motion and the sensation of the same part; sometimes, however, the motion, and sometimes the sensation, suffers in the greatest degree; and occasionally the sensation of one part and the motion of another are more strikingly influenced. Hemiplegia is by far the most common form which paralysis assumes from effusion of blood within the cranium. I have, indeed, never met with a decided instance of paraplegia from this cause: occasionally one leg or one arm will be affected, without the other limb on the same side suffering materially; but those forms of paralysis, which occupy almost exclusively the two upper or the two lower extremities, very rarely result from the sudden effusion of blood in the brain. Cases occur where paralysis of the two lower extremities has appeared to depend on other disease or injury in the brain; but of these we should always be somewhat sceptical, from the obvious sources of error to which they are liable; amongst which, the unobserved or the unsought diseases of the spinal cord and its membranes are the most to be suspected. Defective articulation and deglutition, either alone or as attendants upon hemiplegia, are likewise common results of apoplectic seizures. Occasionally great pain is experienced in the affected limbs, while at other times a sense of numbness alone is felt. The powers of the mind generally suffer in some degree, but this varies greatly:—sometimes the mind evinces great irritability, and sometimes a childish tendency to excitement, and a trifling turn quite inconsistent with the former disposition, or with the present situa-

tion of the sufferer:—at other times the patient falls into a dull state of imbecility; while cases occur in which the affection of the mind is so slight, that it is only by close examination it can be detected.”

Treatment.—The most important of all remedies in apoplexy, is the judicious employment of bleeding. It is clearly indicated for the removal of congestion in the brain, or with a view of checking the hæmorrhage, or to reduce the general plethoric condition of the system. When apoplexy simply depends upon congestion in the brain, a large and free abstraction of blood almost immediately effects a cure of the disease. When rupture and effusion have actually taken place, and the slow and labouring pulse shews to what a degree the vigour of the nervous system has been depressed, it is still our duty to bleed, and to bleed largely. Small bleedings, however, will rather do mischief, by increasing the chance of hæmorrhage as the force of the circulation is increased. After bleeding, the pulse must be watched, and if it increase in rapidity, still maintaining considerable force, we must bleed again; but if we find that after two free bleedings, in which we have abstracted forty or fifty ounces of blood, no decided benefit is derived, we must remember how much it behoves us to husband the small remaining stock of vital energy, which, owing to the patient's habits of life perhaps, had been, even previous to this abstraction, greatly diminished. Cupping is to be employed only after venesection has been fairly tried; experience leads us to think favourably of the practice, and perhaps by applying the glasses either to the temples or behind the ears, we may more immediately, and often very expeditiously, draw blood from arterial branches of considerable size with the most decided benefit. How far the application of cold to the head is generally useful, admits of some doubt, as a means of checking hæmorrhage when it is taking place from parts which have strong sympathy with the skin, or are almost continuous with it, we have the strongest proofs of its efficacy. Epistaxis and uterine hæmorrhages are good examples of this; though in apoplexy the circumstances are not precisely analogous. There is much danger, too, it ought to be observed, in a long-continued application of cold to the head in apoplexy, when there is reason to

apprehend a tendency to delay in the circulation, or probable congestion. Where congestion exists, the sudden application of cold is most desirable—and sometimes even when hæmorrhage is taking place within the substance of the brain; but it would undoubtedly be hazardous, as Dr. Bright very properly observes, when employed with this view; for it might throw the impulse from the external to the internal circulation, and thus rather increase than diminish the extravasation of blood.

We give the remainder of Dr. Bright's treatment in his own words, without abridgment:—

“Purgatives are of great importance in every stage of these diseases. It very frequently happens that the immediate cause of the attack is the neglected state of the bowels, and nothing tends more to favour cerebral congestion than a loaded condition of the alimentary canal. When the patient lies under the first impression of the attack, he is often quite unable to swallow medicines, in which case the oil of croton is the most active and appropriate remedy of this class. We should be careful not to administer calomel before the powers of deglutition are sufficient to ensure its being swallowed. I once saw most serious consequences result from this; for having put five grains of calomel on the tongue, and attempted to wash it down with a cathartic draught, the calomel, instead of passing into the stomach, remained, moved about by the tongue, and produced in a few hours the most alarming ptyalism, in which the tongue was forced out of the month, and it was necessary to scarify it deeply before it could be returned within the teeth. If, however, the patient can swallow well, a dose of calomel with extract of colocyath, followed by castor oil, is a very proper purgative. Cathartic injections may also be used with much advantage. In some cases, where the paralysis has been less complete, where paraplegia has occurred, or where there has been a combination of hysteric irritation with the paralytic affection, purging has sometimes been the chief means of cure.

“Diuretics are likewise of great utility; indeed the importance of attending to the action of the kidneys may easily be inferred from the perusal of the foregoing cases, in which the connexion between the apoplectic state and their defective action has been so

frequently traced: and those who have seen much of diseases cannot be ignorant, that even the undue retention of the urine in the bladder is liable to produce great cerebral disturbance; a fact which is proved continually during the prostration of fever, and in a still more striking manner by the state of coma in which strictures of the urethra and retention of urine often terminate.

"Blisters to the nape of the neck and the counter irritation produced by the liniment of tartarized antimony, or the continued discharge from a seton, often promote the cure when the first severity of the disease is passed away, and when we have reason to suppose that morbid action is interfering with the healing processes of the system.

"The time, however, soon arrives when it is necessary to give tone and even to stimulate the torpid nerves, which appear in a certain degree, even after they may be considered as organically capable of resuming their functions, to continue inactive from habit; and this is very much the case in the subordinate attacks of paralysis, where no very extensive organic lesion has occurred. It is at this period that the balsamic preparations are of utility, and that the mineral tonics are employed with advantage: the sulphate of zinc and the arsenical solution act occasionally very well. The *nux vomica* is a remedy which has long been employed during the state of nervous torpor which attends the tedious convalescence after paralytic attacks, and of late the more active principle of that drug, the strychnia, has been substituted: it tends to excite an action in the nerves, which, though not in itself a healthy action, may counteract the morbid state to which they are reduced. In a case of local paralysis, I have applied this powerful remedy in doses of the eighth, the quarter, or half a grain, to a blistered surface, with the effect of producing spasmodic action through the paralysed muscle, and I have sometimes administered it internally with advantage. But cases of hemiplegia from the rupture of vessels are not those in which this remedy holds out the greatest prospect of success, though with caution it may be employed in the advanced stages of convalescence, with safety at least, and sometimes with benefit. Exercise of the affected parts, and friction, are of use; and, by degrees, moderate

and cheerful occupation of mind, avoiding excess and anxiety, will rather promote than retard recovery."

Warning to the British Public against the alarming approach of the Indian Cholera. By SIR GILBERT BLANE, Bart. F.R.S.

THE benevolence which dictated the publication of this little tract will, we have no doubt, be generally appreciated. The venerable baronet, filled with just apprehension for the safety of his fellow-countrymen, lifts his pen for the most praiseworthy of all purposes, and imparts his advice with the earnestness of a dying injunction. He endeavours to impress upon them the horrible nature of the malady with which they are threatened; gives a short sketch of its history; distinctly expresses his belief of its contagious nature; and concludes with some plain practical suggestions for its treatment when it is among them. As we heartily concur with Sir Gilbert, in thinking this to be one of the most alarming visitations with which England has had to contend for many years, and that too much pains cannot be taken to impress this opinion upon the public mind, accompanying that impression with salutary advice, we will not stay to find fault with some minor points which struck us in the perusal, but recommend the "Warning" generally, to the notice of all.

Cholera, its Nature, Cause, Treatment, and Prevention, clearly and concisely explained. By CHARLES SEARLE, Esq. of the Hon. East India Company's Madras Establishment.

WITHOUT attending to the rather uncomplimentary reflection on medical critics, thrown out by Mr. Searle in his Preface, we proceed to notice the substance of his pamphlet as practically as he could wish, premising merely, that, concise and clear as his little work, on the whole, undoubtedly is, it could be rendered still much more so by leaving out the third and fourth chapters, which profess to treat of the "cause of cholera," and of the "operation of the cause in producing the phenomena, &c." We have not patience for dwell-

ling on those pages of a writer professing practical, which treat *de non apparentibus vel non existentibus*; suffice it, for the benefit of those who may not see the work, to mention what the author holds to be the "one essential agent, and immediate cause of the disease;" which is, "*a mephitic vapour or miasm, of the nature of, if not identical with, malaria, in the common acceptation of the word, which being received into the system by respiration, contaminates the blood with its poisonous agency, and, in common with the other poisons of a sedative class, of which it is one, operates immediately and specifically upon the capillary vessels, in depreciating and arresting their functions; and hence, as they enter into the composition and structure of every organ and part throughout the system, and as their function is nutrition, secretion, calorification, and, in short, all that appertains to the life and health of the part and system in general, it necessarily follows that the depreciation of their action is attended with debility of all the functions, &c. &c.*" We fear this will not pass for a good specimen of the author's ability in getting up a useful popular pamphlet; we therefore turn to something really more valuable, or at least less questionable and obscure.

Mr. Searle divides the symptoms of cholera (the cholera, as it appeared in Poland, we presume—the pamphlet is dated from Warsaw) into three stages. In the first, or that of invasion, the patient feels giddiness, and disturbance of the stomach and bowels; then vomiting comes on, with evacuations from the bowels of a sero-mucous or barley-water-like appearance; prostration of strength, twitches, pain, numbness and cramp in the extremities; sunken dusky countenance, ringing in the ears, cold skin, and feeble pulse. In the second stage, or that of excitement, inward burning heat at the pit of the stomach, attended with great thirst; pulse sharp and frequent; head and trunk somewhat more warm; tonic spasms gradually involving the muscles of the belly and chest. The third stage, or that of collapse—which often follows the first stage, without the intervention of the second, or that of excitement—is characterized by the cessation of the spasms; livid extremities; cold clammy skin; countenance death-like; coma and dyspnoea—with death ensuing in ten or twelve, but

generally within eight-and-forty hours from the period of invasion. There is considerable variety, however, in the symptoms, as they appear in different individuals and under different circumstances: the spasms are sometimes not distinguishable; sometimes a mortal coldness comes on from the beginning, and the patient dies without a struggle.

We have, on a former occasion, given some of Mr. Searle's peculiar opinions with regard to the exhibition of *common salt* in this disease: we may now add, on this head, that he continues to recommend that practice most strenuously, as the best beginning of the treatment.

"As a deranged state of the stomach is, I am of opinion, no unfrequent exciting or predisposing cause of the attack, or if not, as indigestion invariably succeeds to it, as a general rule, I think it always advisable to commence the treatment by evacuating the stomach, whether the patient has vomited before or not. With this intention let the patient drink freely of warm water, or (which I am of opinion is better, as it operates almost instantly, and more perfectly accomplishes the purpose, and is always available, and from its stimulant and mild aperient qualities is useful also with these intentions)—a *large table spoonful of common culinary salt*, (muriate of soda) dissolved in half a pint of hot water, and drank as warm as it well can be: should it fail in producing the desired effect, which however is but seldom the case, a second dose may be repeated five minutes afterwards. Prior to the emetic, I think it however advisable, whenever the state of the patient admits of it, particularly if there be pain in the head, or oppression about the præcordia, that the patient be bled from a small orifice, in the recumbent posture, to a moderate extent, as this tends immediately to the relief of these symptoms, which are dependent upon accumulation of blood in these parts; and the emetic following the bleeding to the restoration of an equilibrium of the circulation."

We need not follow the author in the other particulars of his practice in cholera, which are those generally recommended by most writers, and already detailed with sufficient minuteness in our pages. Calomel he prescribes in large doses, because, says he, "in effect, in this disease large doses are but equal

to small ones under ordinary circumstances, from the diminished absorbing power of the stomach and susceptibility which assists to its influence."

Twelve grains, he thinks, ought to be the first dose, "which being simply placed upon the tongue, should be washed into the stomach by a cordial; and a very suitable one is a dessert or table spoonful of brandy, with double the quantity of hot water; and the same dose of calomel should be repeated every hour or two, according to the urgency of the case, and commensurate with the improvement of the patient in smaller doses, should it be continued, till bilious stools and urine are restored, when it may be given in conjunction, or alternately with some mild aperients, till healthy secretions are established."

In his directions with regard to articles of diet and sustenance craved by the patients in cholera, we fear Mr. Searle is too indulgent in consulting so freely the wishes of the sufferers, notwithstanding that he pleads the observation of twenty years practice of medicine to bear him out in the proceeding; but we highly approve of the greater part of his treatment, and more particularly of the sensible remarks which he offers for the benefit of those who are wise enough to take timely precaution. We need scarcely add, that he advises temperance and moderation above all things; and the following hint ought not to be neglected:—

"In cholera times, it is particularly advised that you should not leave home in the morning with an empty stomach; a cup of coffee, with or without a tea-spoonful of brandy in it, and a biscuit, is indispensable, if too early for breakfast. And a light supper, with a little spirits and water, is no bad thing before going to bed, which should be early."

We bar the supper, however; the cup of coffee and the biscuit, as a stay before breakfast, may do very well.

The pamphlet concludes with (we think very unnecessarily) a reiteration of the author's continued adherence to his opinion that the disease is not contagious. We take leave, with regard to this point, to refer him to our leading article in the present number.

Practical Remarks on the Disease called Cholera, which now exists on the Continent of Europe. By JOHN GOSS, M.R.C.S. and late Assistant-Surgeon in the Hon. East India Company's Service, Bombay Establishment.

On Cholera Morbus. By MEDICUS.

IN the first of these productions, the only thing we discover worthy of notice, is the author's announcement of the strong acidity of the contents of the stomach when ejected in cholera. "I was sent for," says Mr. Goss, "to an old native woman, who was labouring under all the symptoms of the second stage of the disease. I applied carbonate of soda to the fluid she vomited, which made it effervesce most powerfully; I therefore administered about a drachm of the soda, with ten or fifteen grains of the carbonate of ammonia." The woman recovered, and the author, of course, thinks himself justified in giving the soda plan the strongest recommendation. But the means on which he would place his whole reliance are these—bloodletting to the fullest extent that the system will bear—constant frictions, with a camphor cantharides and turpentine liniment—a mustard poultice to the abdomen—soda and ammonia as above mentioned—and, finally, calomel and jalap. Some absurd arguments are ventured in favour of the non-contagionists.

The brochure of *Medicus* is a catch-penny—ill written and ill got up. In that part where the writer proposes his treatment, he says, he *should not be disposed to cavil with the universal hot bran cataplasm of the Russians!*—though perhaps a vapour bath might be a *more elegant* substitute; nor does he think that in cholera *the bowels ought to be neglected*, for which reason he prescribes opium; and "for the safe administration of that drug," combines two grains of it with "a scruple of calomel, washed down with a camphor draught."

Remarks on the Cholera Morbus; containing a Description of the Disease, its Symptoms, Causes, and Treatment, &c. By H. YOUNG, M.D. formerly of the H. E. I. C. Medical Service in Bengal.

At the present moment our readers will naturally expect us to take notice of all

most works on the engrossing subject of cholera. The one whose title we now give presents no particular claims to their attention. The history and symptoms are described with tolerable perspicuity, but these are now familiar to all, and there is nothing in the details before us of any novelty. The author supports the doctrine of contagion, and recommends the same general methods of treatment as the rest of his Indian brethren.

THE LATE DR. BECKER'S ACCOUNT OF THE CHOLERA AT BERLIN.

Copy of a Report of Dr. Becker, of Berlin, to Mr. Chad, his Majesty's Minister in that capital.

Oct. 12, 1831.

THE comparatively small number of persons attacked with cholera in the city of Berlin, during the first month of the prevalence of this disease, seems to be owing chiefly to the following causes:—

1. The manner in which Berlin is built, compared with other populous cities. It has few crowded and narrow streets, and a great proportion of the lowest classes of the population inhabit the outskirts of the town, where the streets are large and distant from one another.

2. The attention which has before and since the appearance of the disease been directed to the necessity of *avoiding cold and intemperance*. The advice given on this subject by the civic authorities, as well as by the medical men, which, as everybody can read here, must have reached all classes of the population, has been practically assisted by food and clothing distributed among the poorest inhabitants. The almost complete immunity of the garrison of Berlin from cholera is chiefly to be ascribed to the pains taken to give the soldiers additional warm clothing, and to force upon them a healthy diet.

3. The measures taken to prevent the spreading of the contagion. The method adopted is, when a case of cholera occurs in a family, to put those who have had intercourse with the patient, as well as the patient himself, under a sort of quarantine: this lasts for five days after the patient has died, recovered, or been removed to an hospital. It

is left to the choice of the friends whether they will pass this time in their habitation, or go to houses appropriated to the purpose of receiving them (*contumaz, anstalten*). During the time the rooms in which the cholera has prevailed are cleaned, and 'disinfected' with chlorine*.

All these measures are directed by the local committees, of which there are 61 in the city and suburbs, consisting of respectable inhabitants, medical men, and a police-officer, and having in their service men who devote themselves to nursing the sick, and preventing intercourse with other persons. These servants of the committee are generally lodged in particular houses, where they are always to be found. It cannot be said, however, that these regulations are, in all instances, rigidly adhered to; they are often neglected, owing to the prejudices of the patients and their families, who conceal the disease; the remissness or connivance of the medical men, who do not wish to cause inconvenience to themselves and their patients; and the inactivity of some of the committee.

Experience has shewn that the spreading of the cholera has been most efficaciously prevented when the patients have been *immediately removed to an hospital*, and where those who had lived in the same room with them also left the infected house, and returned to it only after it was cleaned and aired. This measure having been adopted wherever circumstances admitted of it, may, in fact, be regarded as the chief cause of the very moderate extent to which the cholera has till now prevailed.

In reply to the other queries put, the following answers may be given:—

1st. What description of persons has most suffered from the disease at Berlin?

The great majority of persons attacked with cholera in Berlin consists of those who, on the one hand, are exposed to the usual causes of disease, viz. cold, fatigue, and particularly intemperance in food and drink, or are labouring under previous disease, particularly diarrhoea; and, on the other hand, are, by their business, or by chance, brought into intercourse, direct or indirect, with those already attacked by the disease,

* Of the superior efficacy of this substance, however, there is no evidence.

and with their dead bodies. Of course the greatest number of patients occur in the lower orders, who form the bulk of the population; but there have not been few instances among persons in easy circumstances; in them also, previous disease, intemperance, fatigue, or anxiety of mind, have generally, although not uniformly, preceded the disease.

2dly. Whether the disease was confined, for any length of time after its appearance, to one street or one district of the city?

The first cases of cholera in Berlin occurred among the skippers on the boats lying on the river *Spree*, which flows through the town, and in houses in the immediate neighbourhood of the river. The disease has prevailed to a considerable extent in all those streets which lie along the navigated branch of the river, and whose inhabitants at the same time live in frequent intercourse with the skippers and fishermen. On the fourth and fifth days cases appeared in other parts of the city, and, in many instances, they were those of individuals who were known to have had intercourse with cholera patients, or at least with the boats lying on the river, and with the streets first infected. In the part of the city chiefly inhabited by people in easy circumstances (Friedrichstadt) such cases generally remained solitary, and the disease did not spread in the streets where it had thus appeared; in those districts, on the other hand, which are peopled by the labouring classes (in the centre of the town, and in the eastern suburbs), the disease once having occupied one house of a street, was observed to attack other houses successively. Three weeks after its commencement the cholera had spread in all directions, without any regard to situation, high or low, damp or dry, or to exposure northern or southern, eastern or western, but occurring in solitary instances only in the Friedrichstadt, whilst it found numerous victims in four or five other quarters remote from one another, and inhabited by the poor.

3dly. Whether, when the disease attacked one member of a family, the other individuals in the same family suffered from it?

This is the case so frequently, that it may almost be considered as the rule, and the contrary as the exception, unless the patient be removed from his

family. It is not possible to give any numeric returns of the recurrence of the disease in families, but the following is a statement of its re-appearance in the same houses where it had shewn itself:—

From August 29th to September 26th, there have been reported cases of cholera in Berlin, 770. During that period, a second case has happened in the same house where one case had been reported:—After one day, 65 times; two days, 34 do.; three days, 23 do.; four days, 16 do.; five days, 21 do.; six days, 7 do.; seven days, 3 do.; eight days, 2 do.; nine days, 0 do.

In order to illustrate this point, it may not be superfluous to mention some instances of the recurrence of cholera in the same families and buildings:—

1. A physician (Dr. Callow), who had attended cholera patients, being at the time in bad health, and labouring under diarrhœa, died of cholera; on the day following, his landlord (Mr. Steibelt) died; a day afterwards, two children of the landlord died, and the servant-maid was taken ill of cholera, and recovered. The landlord's wife had been removed to quarantine. There have been no more cases of cholera in this street and its neighbourhood.

2. A family, living on the river side, consisting of husband and wife, four children, and a servant-maid, were all successively attacked with cholera; only the husband and one child survived.

3. In a house (Alte Jakobstrasse, 66,) the following cases of illness have occurred:—1. Sept. 7th, W. M. 27 years of age, seized by cholera; recovered.—2. 8th, Miss M. 32 years of age, with vomiting and purging; recovered after twelve hours.—3. 11th, A boy of two years and eight months, son of a cutler, cholera; died on the 12th, after eleven hours' illness.—4. eod. A journeyman of the cutler, vomiting and purging; recovered.—5. 13th, The child of a tailor, aged two years and nine months; died of cholera after nine hours' illness.—6. 15th, An apprentice of the cutler, vomiting and purging; recovered.—7. eod. The man who had been in attendance on No. 5 took the cholera, and was sent to the hospital.—8. 16th, Another apprentice of the cutler, vomiting and purging; recovered.

4. The *workhouse* (arbeitshaus), a large building occupied by a numerous

poor population, had a cholera patient, on Sept. 3d, *five* days after the appearance of the disease in the city: the following cases successively occurred in this building:—Sept. 3d, 2 cases; 5th, 2 do.; 11th, 1 do.; 15th, 1 do.; 16th, 5 do.; 17th, 7 do.; 18th, 6 do.; 19th, 9 do.; 20th, 4 do.; 21st, 6 do.; 22d, 2 do.; 23d, 6 do.; 24th, 3 do.; 25th, 2 do.; 26th, 4 do.—60 cases.

Immediately beside the workhouse there is another building (Familienhaus), inhabited by a great number of very poor families, who there find employment of various kinds: here the disease did not show itself till September 8th, *five days after the first case in the workhouse*: from that day to the 26th Sept. 27 cases occurred in this house.

It may finally be stated, that the manner in which the cholera has appeared and spread in Berlin, perfectly warrants the conclusion, that this disease is always produced by a peculiar human effluvia, which in its mode of generation and effusion shows some analogy with that of the *common typhus fever of Great Britain*, but extends its deleterious effects to a limited number of persons only, who are rendered eminently susceptible by cold, intemperance, fatigue, fear and anxiety, diarrhoea, and other previous diseases.

(Signed) F. W. BECKER, M.D.

Berlin, Oct. 5, 1831.

PARISIAN BOARD OF HEALTH.

Extract from the Instructions given by the Central Commission of Health, in Paris, to the Minor Commissions of Districts and Parishes (d'arrondissement et de quartier).

In the event of the arrival of this awful epidemic, it will behove all good citizens to combine their efforts with those of government to purify the city as perfectly as the localities will permit.

The commissions are composed of medical men, persons of professional distinction, so selected as that each may be employed in that line for which his talents and his experience adapt him. To these are added persons unconnected with the profession, but whose standing in society is most respectable, and who have more time and local knowledge to

assist and direct the proceedings of their colleagues. The civil power has received orders to second the business of the commissions.

The parish commissioners are to visit *every house* in their parish, and point out to the inhabitants whatever they find amiss, warning them of their danger and of the propriety of setting all to rights before the police interfere. In these visits it will be particularly necessary to ascertain the state of the privies, leads, gutters, and channels into which the foul water of the house flows; also the wells and cesspools; and to look to the stables, that their floors have the proper inclination and allow the fluids to run off from the dunghoops which are often suffered to accumulate.

Establishments—such as schools, nurseries, *maisons de santé*, and places inhabited by dog-keepers, pig-feeders, rabbit-dealers, pigeon-fanciers, who seldom remove the offal; also the dwellings of rag-men, graziers, stable-keepers, bathmen, tanners, gut manufacturers; and, in short, all workshops which become nuisances by bad smells and want of cleanliness—will have need to be examined with the greatest care.

The commissioners will also see that the public streets in their respective parishes are properly paved and in good order, well swept, with the channels running free and cleanly washed; that the reservoirs of the fountains are in good repair, and sufficiently numerous; that the places of public convenience are well kept, and as many as need be; and whether there should not be new *urinoirs* established in certain situations.

The reports of the parish commissioners are to be made to the commissioners of districts, which last are to communicate with the central commission, and also to give their personal attendance in visiting those places from which they have not received satisfactory reports. The district commissioners are also to call upon the principal shopkeepers and artisans, and to solicit their exertions among their class for the more perfect purifying of the city; to insist upon the necessity of neatness in their houses and dress, and of temperance, the want of which produces such aggravation in the progress of an epidemic.

The business of the central commission is to discuss and digest the reports received from the district commission-

ers; to form the grand point of re-union of every thing concerned in these works; and to act immediately in connexion with government.

Members of the central commission engaged in drawing up this document, MM. Marc, J. Pelletier, Girard, Darcet, and Lucien Delamoriere.

CLINICAL LECTURES,

Delivered in the Dispensary of the London University,

BY

DR. ANTHONY TODD THOMSON.

LECTURE IV.

November 4, 1831.

Acute Hydrocephalus—Epilepsy.

GENTLEMEN,—The principal object of this lecture is, to direct your attention to the very interesting and highly-instructive case of acute Hydrocephalus, which was admitted on the 27th of last month, and which has terminated fatally—the too frequent result of cases of this formidable disease. As the little patient was necessarily confined to bed, and visited at home, a few of you only have had an opportunity of observing the progress of the malady; I will therefore enter a little more minutely into the history of the case than would be otherwise necessary.

George Harker, three and a half years of age, a fine intelligent boy, with light hair and blue eyes, was stated to be too ill to be brought to the Dispensary; he was, therefore, visited at his father's house. On inquiring into the history of the case, we were informed that, a year and a half ago, this child fell down an area, and struck the back part of his head with violence; he lay for two hours in a state of insensibility, with the eyes fixed. Since the occurrence of that accident he has been less playful than children of his age usually are; occasionally subject to attacks of dizziness: and, more than once, has suddenly fallen down. Six weeks since, when in the country, he caught a severe cold, was brought to town, and soon afterwards placed under the care of a medical gentleman, who lanced the gums under the impression that his complaints arose from dentition. On Friday, the 21st of October, he was seized with a violent pain of the head, which continued without intermission. The cough and difficulty of respiration, which were brought on by the cold, still remained, but only in a slight degree, having diminished from the time that the pain of the head came on. The bowels were relaxed, and the stools dark-green and offensive. Such was the account given by the mother.

At the time of taking the report the boy was lying on his back, the head a little inclined to one side, and the eyes closed: he expressed great intolerance of light. His mother told us that he cried for many hours together, sometimes uttering a loud and prolonged scream. He was very restless, especially at night, rolling his head on the pillow, and sometimes jumping up in bed. He was perfectly sensible, recognizing every one of his family; and put out his tongue when ordered to do so. There were frequent flushings of the cheeks, followed by dampness of the skin. The pulse was 116, full and soft; the skin was dry; the temperature moderate; the tongue little furred; the urine scanty, high coloured, and offensive to the smell. He refused food. On examining the head, it appeared larger than in children of a similar age, measuring $21\frac{1}{2}$ inches round, and 12 inches across the head, from ear to ear, in the direction of the coronal suture. Both parents are healthy, and not subject to complaints of the head. Two other of their children, however, died with symptoms closely resembling those of this boy; but neither of them had met with any accident. Ten Leeches were ordered to be applied behind the ears; and a powder, consisting of six grains of Calomel and eight of Jalap, was directed to be taken after the bleeding, and the head to be kept moist with a cold evaporating lotion.

On the second visit we found that four Leeches only had been applied; but the mother assured us that much blood had flowed after they dropped off. The same degree of restlessness, the rolling of the head on the pillow, and the screaming, continued; the pulse was 120, thin, and hard; and the stools had assumed the slimy, glazed appearance, which is so pathognomonic of acute Hydrocephalus. The pupils were not dilated; the tongue was still but little furred, and moist. Four grains of Calomel and three of Colchicum, were ordered to be taken every eight hours, and two table spoonfuls of a solution of six grains of Tartar-emetic in six fluid ounces of distilled water.

On the 1st of November the pupils were much dilated; he squinted, and complained of not seeing distinctly; there was some degree of delirium, and the pupils were occasionally turned upwards. The stools had lost their green colour and glazed appearance, and were pale and pasty; the tongue had become more furred, but was still moist; and this, you will find, is frequently the case when the brain is affected. The same plan of treatment was continued; and, as the head was hot, the application of the lotion strenuously enjoined. On the afternoon of this day convulsions supervened, and continued with slight remissions until ten o'clock on the following morning. The pulse was 148, and bounding; the pupils were still

sensible to light, but the patient appeared incapable of recognizing any one. At noon (November the 2d), the whole face was suffused with a deep scarlet blush; the eyes were protruded, the pupils much dilated, and the teeth fixed as in trismus, whilst the breathing was stertorous. The thumbs were thrown across the palms of the hands, and the two first joints of all the fingers rigidly bent inwards; the pulse was hard, bounding, and 150; the temperature high; and there was an evident tendency to opisthotonos. He had passed scarcely any urine since yesterday, and only one stool, which was dark-green and pasty, but not glazed. The head was very hot, although the lotion had been sedulously applied. The temporal artery was ordered to be opened, but, before five ounces of blood flowed, a state of collapse came on, which seemed to indicate a rapid fatal termination of the case; nevertheless the trismus did not yield. The breathing, however, lost the apoplectic character, but he became torpid, and continued so until he died. Enemas, composed of small quantities of White Wine Whey and four drops of Tincture of Opium, were directed to be administered every hour; and a large blister was applied to the nape of the neck. In seven hours afterwards he died.

Post-mortem Examination of the Body.—On laying bare the cranium, the sutures appeared more than usually consolidated in so young a child; and the adhesion between the dura mater and the bone was so great, that the skull cap was with much difficulty raised. The vessels on the surface of the brain were greatly congested; as were those also running through its substance; the medullary part displaying an infinite number of large bloody points. The whole surface of the brain was bathed with serum, and about two ounces of fluid were collected in the base of the cranium, passing down into the cavity of the spinal column. The appearances of inflammation were most obvious on the medulla oblongata, and in the sheath of the spinal marrow, particularly in the pia mater, which was beautifully injected. The quantity of fluid in the ventricles was comparatively small. The substance of the cerebellum was healthy. On opening the abdomen, the liver was found in a diseased state, studded with dark patches, which penetrated the substance of the viscus; several small tubercles, not larger than millet-seeds, were also observed in it. The spleen and the pancreas were healthy; but the mesenteric glands were enlarged, and some of them contained suppurating tubercles. The rest of the abdominal viscera were in a natural state.

Few cases, gentlemen, will come before you more fraught with important matter for remark, both in a theoretical and

practical point of view, than that the report of which I have just concluded. In tracing the origin of the disease, we should be tempted to refer it to the accident which occurred to the child eighteen months ago; but I am of opinion that that concussion impressed upon the brain a predisposition to take an inflammatory action, rather than that the inflammatory state of the meninges then commenced. It is true that the child displayed less sprightliness than usual in young children after the accident, but it is pretty evident that it was not until after he caught cold, six weeks ago, that the inflammatory action was set up; and there seems to have been a translation of diseased action from the lungs to the head, as the cough abated when the head became affected. How far the state of the brain, induced by a severe blow on the head, resembles that which may be denominated *hydrocephalic*, and which seems hereditary in some families, it is difficult to determine; but that such a state exists there can be no doubt, and there is as much propriety in talking of a *Hydrocephalic* as a *Scrofulous*, or any other diathesis: the constitutional disposition to many diseases is obvious, long before the habit is actually under the influence of the disease to which the diathesis is supposed to contribute. In this state of the brain, however it may be brought on, a diseased condition of the liver, or some other of the abdominal viscera, not unfrequently disposes the meningeal vessels to take on that action which terminates in effusion of serum, either upon the surface of the brain or into its ventricles. In our little patient's case, the primary diseased action was evidently in the lungs.

The knowledge of the fact, that, in families in which the hydrocephalic diathesis prevails, a transference of diseased action from any part of the body to the head is always likely to occur, points out the importance of early attention to even the slightest inflammatory attacks in such individuals; and more especially when the abdominal viscera are affected. In such a case as that now under consideration, I am of opinion that much security may be reasonably anticipated from establishing an issue, so as to divert the determination of blood to the head, and from constant attention to the bowels. In the present case, both the liver and the mesenteric glands were diseased; but it is probable that these were consequent on the state of the head, arising from the fall: and daily experience will teach you, gentlemen, that, in cases of cerebral derangement, conjoined, as they almost always are, with disease of the hepatic and chylotropic organs, nothing is so difficult as to determine whether the brain or the abdominal viscera be the primary seat of disease. The dizziness and falling down, which oc-

curred before our little patient caught the cold which proved the exciting cause of this affection of the brain, indicates a state of the organ similar to that which sometimes precedes attacks of Epilepsy; and if the life of the patient had been spared, he probably would have suffered from that disease.

The next observation to which I have to direct your attention, is the position of the patient on our first visit. He was lying on his back, with his eyes closed, to shade them from the painful stimulus of light. This affords me another opportunity of making a few remarks on the physiognomy of disease. When the brain is affected, the power of volition over the muscles is much diminished, sometimes wholly suspended; consequently, the position on the side, which is natural to us during rest, in a healthy state of the body, cannot be maintained, and the patient turns on his back. By observing this position, therefore, when it is attended with screaming, rolling the head on the pillow, and jumping up in bed, you may always form a pretty accurate idea of the state of the brain. These symptoms, however, mark a high degree of irritation rather than one of acute inflammation; but if the means taken to obviate this condition prove insufficient, the acute inflammatory action is rapidly set up, and runs its course to a fatal termination. It is true that the previous symptoms often so closely resemble those connected with Worms, or with Dentition, that the diagnosis becomes extremely difficult; but when we see the patient always lying on his back, and rolling the head on the pillow, we can scarcely be mistaken in pronouncing the disease to be *Acute Hydrocephalus*. In employing this name, gentlemen, I do it merely in conformity to established custom, for the *Hydrocephalus*, or *Dropsy in the Brain*, is certainly not the disease, but the consequence of the disease.

The thumbs thrown across the palm of the hands, and the clonic spasm of the flexor muscles of the two first phalanges of the fingers, with the accompanying trismus, indicate a high state of cerebral excitement, particularly on the base of the brain, involving the origin of the motor nerves within the cranium; and this the post-mortem dissection demonstrated to be the case, in a very striking manner. The crossing of the thumbs is not an unfrequent symptom of visceral irritation affecting the brain and medulla oblongata, in severe cases of dentition, and occasionally in irritation from worms: in these cases, the spasm is generally resolved by correcting the condition of the abdominal viscera by active purgatives, so as to make some impression on the brain; but when purging fails, bloodletting, followed by calomel and opium, must be resorted to. Trismus is a very uncommon termination of *Acute Hydrocephalus*, and can only be accounted for by the extension of the inflam-

mation of the pia mater to the medulla spinalis; for, in this case, there was nothing in the state of the abdominal viscera to which it could be referred. Late investigations, particularly those of the continental pathologists, have also discovered an increased vascularity of the spine in all cases of tetanus; indeed, when we consider that, besides the fixation of the jaws, the diaphragm, and abdominal muscles, the trapezius and the serratus magnus auticus are thrown into spasmodic contraction, we can only trace these effects to inflammation affecting the origins of the par vagum, the spinal accessory nerve, the phrenic and external respiratory nerves, or to those nerves affected by some other powerful irritating cause. That the former is the most probable cause, may be inferred from the permanent or clonic nature of the spasm.

The turning up of the pupils has been commonly regarded as indicating the effusion of fluid within the cranium; and, as it always appears towards the close of this formidable disease, the vulgar say "that children, with water in the head, are looking to their final home." It is said, that vulgar sayings are generally founded in fact, and this is true as far as this turning up of the eyes is the prognostic of great debility: but it is only when it occurs in our waking hours that it is so, for in sleep it is always present; and, therefore, we may all be said to be looking towards our final homes during sleep. You will read in Dr. Good's *Study of Medicine*, and other medical writings, that both the turning up of the pupil and strabismus is the result of spasmodic action, but the discoveries of Sir Charles Bell have demonstrated that this is an erroneous opinion. The turning-up of the pupil is merely an indication of great debility extended to the voluntary muscles of the eye, leaving the obliqui to act with relatively greater power, and it is by their operation that the pupil rolls upwards. The same thing, as I have already said, occurs in sleep, but it is not observed, owing to the eyelids being closed. When any circumstance occurs, however, to prevent the eyelids from closing, then it becomes visible. This is well described in a letter from Mr. Jackson, of Sheffield, which Sir Charles Bell has published in his admirable work on the *Nervous System*. The patient, owing to partial paralysis of the face, could close the lids of one eye only in sleep, whilst the other remained uncovered. "The right is closed," says Mr. Jackson, "and the upper eyelid on the left side remains as in the state of ordinary vision, whilst the inferior margin only of the cornea is visible; thus simulating the appearance on the paralytic side of a person in the act of dying." No feature, nevertheless, is so indicative of the state of the brain as the eye. In the early or inflammatory stage of *Acute*

Hydrocephalus, they are generally suffused, watery, and intolerant of light: when the state of oppression supervenes, the pupils become dilated, the eyelids are not closed even in sleep; strabismus, the rolling of the eyeball in its socket, and various contortions are observed. Sometimes in the commencement of the disease we find the pupil much contracted, with knitting of the eyebrows; but these symptoms rather indicate the existence of nervous irritation than inflammation in the membranes of the brain, although it is often mistaken for Arachnitis, into which it may decline, if not properly managed. It may be removed by a course of brisk purgatives, followed by the judicious and cautious administration of opium and light aromatics: if blood be abstracted, it should be in small quantity, and by means of leeches on the temples, or behind the ears.

The green, slimy, glazed looking stools, which are so characteristic of this disease as to be designated *Hydrocephalic*, demonstrate the powerful influence of the nervous energy on the secretion of the liver: instead of the ordinary secretion of the viscus, these green slimy stools are produced. It is of importance not to confound these stools with the dark green alvine discharges which usually follow the administration of Calomel in children; and which seem to depend on the mercurial stimulating the gall-ducts, and thus exciting the gall-bladder to pour out its contents, which, from having been pent up for some time, have become viscid or inspissated, and acquired a much darker green hue than recent hepatic bile. *Hydrocephalic* stools are distinguished from these by being very little acted upon by water, which is scarcely coloured or rendered turbid by them and by possessing little or no offensive odour. We know too little about the manner in which the liver, or any other gland, separates its secretion from the blood, to explain the nature of the change which produces *Hydrocephalic* stools; but we know that it is intimately connected with the state of the brain; and one fact of practical utility is worth a score of conjectural theories. If no change occur in this state of the alvine evacuations, a fatal prognosis may be pronounced; but the contrary does not always authorize us to venture on hazarding a favourable opinion; this is demonstrated in the case before us: the stools on the fourth day of the attack lost their *hydrocephalic* aspect, and became pale and pasty, whilst the disease was rapidly gaining ground. It was almost immediately after this change that the patient became insensible, and incapable of recognizing those about him; and it might be expected, that, as the fluid was poured out within the cranium and the inflammatory action was partly suspended, the function of the liver would become more

natural and, consequently, the bile reassume an appearance more consonant with its usual state.

The frequent fatal issue of attacks of acute *Hydrocephalus* has led to a too general opinion, that it is an incurable disease; and has, unfortunately, paralyzed those efforts that would otherwise have been exerted to trace and avert the causes of the evil. Let me warn you, gentlemen, against entertaining such opinions: every disease, which has a tendency to destroy a vital organ, becomes an incurable disease in the hands of the ignorant and the negligent; but *Hydrocephalus*, even in its advanced stage, after strabismus, blindness, and convulsions have taken place, as long as the pulse remains moderately steady, and the breathing continues natural, must not be contemplated in inactivity, as if the fatal event were inevitable.

In regarding the disease as an inflammatory state of the cerebral vessels, excited by external injuries, or more frequently by diseased states of the abdominal viscera, sympathetically affecting the brain, peculiarly predisposed by hereditary organization or accidental causes to take on diseased action, we are led to consider two distinct kinds of treatment, according to the period of the disease at the time our advice is demanded: the one *prophylactic*, the other *curative*. It falls to the lot of the practitioner to put the former in practice only when he is aware of an hereditary predisposition to *Hydrocephalus*, existing in a family, which has that implicit confidence in his opinion that would insure attention to it, even when the reasons for following it are not very evident to common observers; in exercising the second, it frequently happens that the disease has gained too great an influence before his efforts to overcome it are commenced.

As a prophylactic measure in those families in which the *Hydrocephalic* diathesis is suspected, the closest attention should be bestowed on the state of the abdominal viscera. In the slightest deviation from the healthy state, the hypochondria should be examined; if any tenderness on pressure be felt, leeches should be immediately applied; and if the stools be unnatural, Calomel, and Tartar Emetic, or James's Powder, in conjunction with Jalap, or Scammony, should be administered. But you must also bear in mind that when intestinal irritation is great, drastic purgatives frequently tend to increase it; and, therefore, instead of Calomel, combined with Jalap or Scammony, you will find the *Hydrargyrum cum Creta*, with the powder of Colchicum, better adapted to subdue irritation and promote healthy secretion. I have found the most obvious advantage to result from a long-continued course of the Wine of Aloe combined with

Alkalies. If, however, notwithstanding the openstate of the bowels, headache, or any other symptoms of morbid excitement of the cerebrum, display themselves, then the employment of more active purgatives should be resorted to; but these ought not to stand in the way of local blood letting, for it is only by subduing vascular excitement in its earliest stage and correcting the derangement of the digestive organs, that we can expect to ward off the influence of those causes, which have called into activity the morbid tendency of the habit, and to aid the powers of the constitution in restoring its healthy functions.

After the attack has really commenced, the depletion should be more active, but nevertheless limited. In the case under consideration, the number of leeches applied was inadequate to the effect, and it is probable that the account of the quantity of blood lost was exaggerated. Although aware of this, yet I was hopeful that, by purging actively with Calomel and Colchicum, with the addition of Tartar emetic in doses sufficient, by its nauseating influence, to abate arterial action, I should be enabled to diminish the general morbid irritability of the nervous system, without depressing too much the powers of life. The low state of the habit previous to the attack rendered much depletion by blood-letting a doubtful measure; and having had many opportunities of witnessing the beneficial effects of active purging in Acute Hydrocephalus, the leeches were not ordered to be repeated. It may be thought that the nature of the purgative to be employed is of little consequence; Calomel, in combination with Colchicum and an antimonial, not only stimulates the torpid bowels, and, by acting on the orifices of the gall-ducts, empties the liver, but it also aids in altering the abdominal secretions and exciting the languid absorbents; consequently it ought always to be preferred. I recollect Dr. Denman having informed me that he had ordered Elaterium in one case; the effect of which was a violent diarrhœa that continued for several days, during which the symptoms of the disease disappeared, and the child recovered. My own experience, also, induces me to place more confidence in purging than in bloodletting in Hydrocephalus. When, however, the symptoms of Apoplexy, with stertorous breathing, and the scarlet, flushed state of the face supervened, no alternative remained but opening the temporal artery: but, could I have anticipated the sudden collapse which followed, I think I should not have ordered it.

Some benefit might have possibly resulted from the substitution of Croton oil for the lancet; and as the fixed state of the jaws prevented medicines from being exhibited by the mouth, its effects would have been obtained by applying it to the surface, in combination with Opium. As the collapse

immediately followed the arteriotomy, the only chance that then remained, to avert the fatal issue, was to arouse anew and to support the powers of the habit. I cannot, gentlemen, urge too strongly the necessity of making yourselves familiar with the distinction which exists between Coma and Exhaustion; and warning you, immediately on the appearance of the latter, to throw aside Calomel, Antimonials, and every other depressing agent, and to have immediate recourse to White Wine Whey, or Brandy and Water, Ammonia, and Opiates, in adequate doses, frequently repeated, until evident symptoms of re-action display themselves. These are readily recognized by the pulse, which will become less frequent and fuller; by the warmth of the extremities returning; and, with the colour of the cheeks, the restoration of the natural expression of the countenance. As soon as this takes place, the howe's should be regulated by gentle aperients, such as Rhubarb and Magnesia, and the stimulants gradually exchanged for light nutritive diet and appropriate tonics. Were it necessary, I could detail to you several cases, in which I have snatched children, as it were, from the very jaws of death, by exchanging the depleting for the cordial plan of treatment, under which every symptom of threatened effusion has vanished, and the smile of returning health gradually returned to a countenance, which not twenty-four hours before displayed the *facies Hippocratica*—that fatal index of complete exhaustion, and, frequently, too certain harbinger of final dissolution.

It is unnecessary to make any comment on the appearances of the brain and the spinal marrow displayed in the examination of the body; they were such as satisfactorily to account for the symptoms that characterized the disease, and to add another proof, were any wanting, of the importance of morbid dissections.

Epilepsy.

I have no doubt, gentlemen, that you have observed, with satisfaction, the success of the plan of treatment adopted in the case of Leni. He has had no fit since the 27th of last month; but, nevertheless, although the headache has completely left him, yet he still complains of dizziness, and there is a constant winking of the eyes, with suffusion of these organs, and an expression of great anxiety. The pulse is 72, small, and irregular, and the tongue furred. The bowels are open, and the urine has lost its high colour. He has been ordered to continue the use of his pills, and mixture, with the addition of ℞xiv. of the Arsenical solution. I shall say nothing regarding the reasons for prescribing this tonic, until we shall have seen something of its effects.

REPORTS OF CASES OCCURRING
AT PUBLIC INSTITUTIONS.

GENERAL DISPENSARY.

Stricture of several years standing, Extravasation and Death—with Clinical Remarks.

JAMES FYNLAISON, aged 50, residing at No. 112, Aldersgate-street, was seized with shiverings, which came on once in twenty-four hours, and were so severe as to compel him to apply at the General Dispensary. The shiverings were speedily removed by the use of quinine; but Mr. Coulson was requested to see him for a difficulty which he had in passing his water. The patient said, that for twelve years past the size of the stream of urine had been diminishing, and that he had experienced at times great pain in passing his urine, and felt frequent desire to make it. These symptoms had of late very much increased. At this time he could scarcely pass any water, and what came away was passed in drops; there was great pain and a sense of scalding in the urethra, and on examination a swelling was found in the perineum. There was also great constitutional disturbance. It was very clear that some portion of the urethra was in a very diseased condition, and from the pain and scalding in the part, and swelling in the perineum, Mr. C. apprehended that ulceration of the urethra, and extravasation into the perineum, had commenced. A middle-sized catheter was introduced, without the least difficulty, as far as the membranous part of the urethra; but here a firm obstruction was met, and the instrument was instantly withdrawn. A small-sized catheter was tried, and with very little difficulty introduced into the bladder: about half a pint of dark-coloured fluid came away, and the patient felt much relieved. Mr. Coulson wished to make an incision into the perineum, but the patient would not consent. A gum elastic catheter was kept in the bladder; warm fomentations were applied to the part; an opiate was given immediately, and some aperient medicines ordered in the morning.

The patient had continued easy until the middle of the night, since which the pain had returned; the swelling had extended to the scrotum, and the prepuce was also œdematous. The necessity of giving relief by an operation was again strongly urged upon the patient and his friends, but the proposition was met by a decided negative. As the bowels had not been opened, a clyster was given; the pulse was small and quick, (120); and the countenance pale and anxious. The extravasation extended during the course of the day to the lower part of the abdomen, and on the penis there were vesicles filled with black fluid; the penis itself was semi-erect; the pulse 140, and very small; hiccup; now free from pain. In the evening he died.

The body was examined within 24 hours after death. The whole cellular tissue of the perineum, scrotum, penis, and lower part of the abdomen, were loaded with a dark-coloured fluid, having a strong urinary smell; the urethra was sound in every part except at the membranous portion, which was completely destroyed by ulceration, and the opening communicated with a cavity in the perineum of sufficient size to hold an egg; the prostate was enlarged, and the bladder inflamed.

Nov. 4th.—To-day, Mr. Coulson made the following clinical observations on the above case:—Gentlemen, this case of extravasation of urine with stricture, which occurred some time ago in this institution, shows the rapidity with which the mischief extends, and even death ensues, unless the most prompt and efficient measures be resorted to. The giving relief to the extravasated urine by free incision might have afforded the patient a chance of recovery, and ought in no case whatever to be neglected. Extravasation of urine may occur in consequence of pre-existing mischief in the urethra, (and is then frequently, though not always, preceded by retention,) or in consequence of violence, either external, as a blow, fall, &c., or internal, as the too forcible introduction of instruments. The symptoms, however, of the disease from either cause are pretty much alike: there is pain and a sense of scalding in the urethra, increased by any attempt at making water, swelling and tenderness of the perineum, which, in severe cases, generally extends to the scrotum, prepuce, and lower part of the abdomen, and the skin over the part where the urine has extravasated ultimately becomes gangrenous. When only a very small quantity of urine has escaped, a hard circumscribed swelling occurs, which terminates in an urinary abscess; the constitutional symptoms are very severe; fever of a typhoid character generally supervenes; and if the urine has not been evacuated by free incision into the part, the patient rapidly sinks: if, on the contrary, this plan of treatment has been adopted, the chances of recovery are much increased. The course which the urine takes is a point worthy of your attention, and depends on the attachment of the perineal fascia. The urine seldom makes its way down the inside of the thighs, or in the neighbourhood of the anus—a direction which, from the position of the body, you might naturally expect the urine to take. The urine, however, makes its way upwards, where the perineal fascia is loose, or has no fixed attachments, being there continuous with the fascia covering the anterior parietes of the abdomen. Laterally this fascia is connected to the rami of the ischia and pubes, and posteriorly it dips behind the transverse perineal muscles, and becomes connected with the anterior layer of

the triangular, or, as it is sometimes called, Camper's, ligament. Thus you will see that, unless the fascia is destroyed by ulceration or external violence, the urine cannot escape in these two latter directions. Sometimes the urine makes its way into the cells of the corpus spongiosum, and then the penis attains a large size, and in general becomes gangrenous; the penis also in these cases is in a state of erection. Mr. Brodie, in his valuable lectures on diseases of the urinary organs, (*vid.* Medical Gazette, vol. i. page 7,) says, that "a black spot may be sometimes seen on the glans of the penis; it is a most fatal sign, for I never knew one to recover in whom it appeared; it indicates that the urine has been effused into the cells of the corpus spongiosum." The urethra also gives way sometimes more anteriorly about an inch or so from the external orifice; in this case the prepuce will be most œdematous. I will now show you a curious case of this kind, in a man fifty-three years of age, who is at present under my care, and whose urethra has given way in this part. You perceive that when he is making water the anterior extremity of the penis, particularly the under surface, swells to a great size; the circumference of this part is seven inches. When he has emptied his bladder, he then empties this pouch, or second bladder, if I may so term it, with his fingers; it gives him little or no inconvenience.

The treatment in cases of extravasation of urine consists, first, in introducing a catheter down to the stricture, or, if you can, into the bladder—then place your patient in the same position as if you are going to operate for stone, and make a free incision in the perineum down to the ruptured portion of the urethra. If you have succeeded previously in getting a catheter into the bladder, you have nothing to do, when you have completed the incision, but to let the instrument remain: if, on the other hand, you have only succeeded in getting an instrument down to the ruptured portion, you must then, taking the point of the instrument in the urethra for your guide, endeavour, from the external wound, to pass a gum catheter into the bladder, and keep it there two or three days: if you cannot get a catheter into the bladder from the external wound, you will not have much to apprehend, as the urine will make its escape, if there be a free external opening: also make free incisions wherever the urine is extravasated. If the urethra is ruptured just behind the scrotum, this part attains a great size; and in this case you must make a very deep incision, guided by the middle line of the scrotum down to the ruptured part. I attended a case, not very long ago, with Mr. Lawrence, where the urethra had given way in this situation; we were obliged to make a free division of the parts in the manner in which I have now men-

tioned; the patient, however, died. Locally, apply warm fomentations and poultices, to promote the coming away of the sloughs, and when they have come away, apply some mild ointment to the wounds; internally, you must give some saline medicines till the sloughs have come away, and opiates, if at bed-time, and afterwards support your patients' strength with tonics, wine, and strengthening diet.

On examination after death, we in general find ulceration of the urethra to a considerable extent, the cellular tissue of the perineum, scrotum, and lower part of the abdomen loaded with urine, or in a state of gangrene. If there has been stricture of long standing, the prostate, bladder, and sometimes the kidneys, will be found diseased. Extravasation of urine from the urethra, as I said before, is frequently preceded by retention, or some such marked symptoms of disease in the canal, as to lead the practitioner, at a very early stage of the complaint, to detect its nature, and to adopt at once the proper means of relief: this, however, does not invariably happen. A case of extravasation came under my care in this institution, in 1823, where there had been but little previous disease, and at the same time no such marked symptoms as to lead me to detect the complaint. The case had been entered under the physician, but I was requested to see the patient for a pain and a scalding in the perineum. I at first suspected that there might be extravasation, and I asked him to make water, which he did in my presence, in a very good stream: at the time of making water, however, the pain was increased; he assured me also that he had never any difficulty in voiding his urine. I then thought nothing more of extravasation, but treated the complaint with the usual antiphlogistic means, as leeches to the part, and warm fomentations. The pain was followed by redness of the lower part of the abdomen, but there never was much swelling. The patient, however, rapidly sunk, and the post-mortem examination disclosed the real cause of the mischief.

There was a great deal of urine lodged in the cellular tissue of the perineum and lower part of the abdomen. There was no extensive ulceration of the urethra; the urine had escaped from three or four minute openings (of a size sufficient to admit a small silver probe) in the membranous portion of the urethra. This part of the urethra was also narrower than natural. I thought that it would be instructive to you to mention this case, for it made a deep impression on my own mind, the man's life having been lost from the nature of the complaint not being understood. I would advise you never to conceal an error in practice; if the only effect of concealment were to deprive our professional brethren of an ample source of in-

struction (for errors will occur to all), this of itself ought to form a sufficient inducement for us not to practise it. But this is not all—by keeping secret our errors in practice, we run the risk of bringing ourselves into a state to commit them without remorse, by deadening those moral feelings which the commission of errors in practice ought to excite in every honourable mind, and which, if properly cherished, form at the same time the strongest stimulus to avoid them.

THE COURIER *versus* THE MEDICAL GAZETTE.

THE Courier has thought fit to allude to the observations contained in our last number on the opinions of Dr. James Johnson: the protocols from Suffolk-Street have been interrupted, but another writer has adopted the same sentiments and the same language; and the Editorial writers have again put forth their own most dangerous doctrines on the subject of Cholera, which they assert not to be contagious, “as the Board of Health pretend.” It is of importance to the public that all points of this nature should be placed in their true light, that the extent to which those, who assume the guidance of public opinion, are entitled to confidence, may be at once determined, and on this account we feel called upon to make the observations which follow.

It is a mere mockery to urge persons to the observance of the quarantine, and in the same breath to ridicule the authority by whose advice such precaution is adopted, and to concur in the sentiments, and praise to the very echo, the reasoning of one who holds (or at least held) the disease to be non-contagious, and the danger to be imaginary: this, we say, is contradictory and inconsistent, yet it falls short of the plenitude of absurdity which some recent writers have displayed.

Thus the Editor of the Courier, about a fortnight ago, in commenting on certain letters published in his paper, says, “We insert to-day a letter with which we have been favoured from Dr. James

Johnson, one of the King’s physicians, on the subject of cholera. It is with great satisfaction that we lay before our readers the opinions of a gentleman so competent to the task which he has undertaken.” What, then, were the opinions to which the Editor thus confidently directed attention? Why these—that the precautions against cholera were perfectly useless. The following are the doctor’s words. After ridiculing the idea of keeping out the disease by measures of this nature, and speaking in irony of having ships ranged bow and stern along the coast, he adds, “I will hold it (*i. e.* the quarantine) perfectly guiltless of neglect of duty, should the pestilence pass this boundary by day or by night, and effect a landing on our shores.” Again, said Dr. Johnson, in a former letter inserted in the same paper, “I have no hesitation in reiterating my opinion, that if cholera comes to these shores, it will come shorn of its fatality, and coercible to a degree that will make the terrorists ashamed of their ominous predictions and visionary speculations.” Here the *Courier* also takes up the subject, and congratulates their readers on the opportunity “of removing the alarm, caused by the notices inserted in the *Gazette*, at the instance of the Board of Health;” and he adds, “It is satisfactory to see that fully completely exposed by medical authority of real weight.” Again, says he, “every thing in reason and in fact seems to show, that it is not contagious.”—(*Courier*, Oct. 29.) Very well: here is the opinion of the writer distinctly enough expressed, whatever may be its value. What, then, will our readers think of the reasoning powers of this gentleman when we inform them that within a week afterwards he denounced in the strongest terms those who had been weak or culpable enough to act upon the doctrines he had taken so much pains to inculcate. He alludes to cholera as having been imported into Sunderland from

Hamburgh, and then, evidently not aware that the admission of this fact was fatal to the doctrine of non-contagion, he adds, "The crews of several vessels from that port (Hamburgh) are stated to have been suffered to land. * * * If it be true that such negligence on the part of the authorities really did take place, we know of no terms of reproach and censure sufficiently strong to bestow upon them. * * * Is it conceivable that they could have allowed the crews of vessels from Hamburgh to have had communication with the shore merely because they gave no external proof of sickness?"—(*Ibid*, Nov. 4). In taking leave of this sagacious personage, we are fain to apply to him his own words, because "we know no terms of reproach and censure sufficiently strong to bestow upon those" who presume to utter *ex cathedra* opinions on a subject of the very rudiments of which they thus shew themselves to be totally ignorant, and who use whatever influence they possess over the public in the inculcation of a doctrine which, according to their own shewing, is not more erroneous than it is pernicious.

They (says the Times of Thursday, most truly) have much to answer for who would throw a whole nation off its guard, and from stubborn adherence to a gratuitous dogma, persuade men from taking simple and easy (provided there be time) means of repelling or mitigating a pestilence to which so many human lives have already fallen victims.

PRECAUTIONS AGAINST CHOLERA.

CHOLERA has been imported into England by a vessel which left Hamburgh after the disease had broken out there, and which was suffered to come up the river at Sunderland. The first person affected was a "keelman" named Stroud, and the next in succession to whom it spread were members of his family. It is remarkable that Sunderland has for some time been looked upon with sus-

picion, in consequence of some doubts as to the strictness with which the quarantine was there carried into effect. The accounts received in town have hitherto been of the most unsatisfactory nature, being lamentably deficient in particulars. After this, however, each practitioner is to send a daily report to Dr. Daun. Our accounts on Thursday gave fifteen new cases, and ten deaths; all among the lower orders. The time for deliberation, however, is gone by, and that which imperatively demands active exertion, has arrived. The possibility of its importation, and the fact of its being infectious, are no longer matters of doubt with reasonable men; and it behoves the authorities of every town and village in the empire to make preparations for the unwelcome visitor. London, we understand, is to be divided into districts; houses are to be prepared for the reception of the sick; and medical men are to be appointed, who will devote their whole time to the fulfilment of the practical measures deemed requisite. These, of course, will not be composed of the present Board of Health, who have been a deliberative rather than an executive body; and we repeat, that now is the time for action. Whether the present Board will be continued we know not, but the new parts of the machinery will, we believe, be immediately put in operation. We understand that army surgeons, and others over whom the government have power, are every where to be put in requisition; and we would advise farther—that, inasmuch as the disease may be communicated by one who himself has it not or in other words, as medical men may carry it to their patients—so, as a general rule, those who are in frequent intercourse with persons labouring under cholera, ought not to visit others, or go into society—at all events, without changing their clothes and having recourse to copious ablution. We are convinced that, in other coun-

tries, the neglect of this precaution has contributed to the extension of the disease. We shall return to this subject next week: meantime we take leave to ask, what is the use of stopping the vessels at the Nore, while the mail is allowed to come up in two days?

The new Board consists of Colonel Rowan, of the police department, Sir W. Pym, Hon. Edward Stuart, Drs. Russel and Barry; the latter gentleman having just returned from the Continent.

— — — A FALSE ALARM.

A PERSON, walking in Regent-Street, had an epileptic attack, on Wednesday: he was taken into a shop close by, at the instance of the Solicitor-General, who was passing at the moment, and Mr. Arnott, of New Burlington-Street, was sent for; by whom the nature of the attack was at once pointed out. Nevertheless, the circumstance got wind, and was magnified into a case of cholera; the government was informed of it, the Board of Health consulted, and the practitioner above-mentioned examined, all within an incredibly short period. This shews, at least, how much all are now on the *qui vive* on the subject.

WESTMINSTER MEDICAL SOCIETY.

Saturday, Nov. 5th.

The Cholera.

IN our last number we gave some account of the proceedings of the Saturday evening previous, considering them as preliminary to a grand discussion which was to take place here on the Saturday evening ensuing; but in this we have been disappointed, and we regret to say that we feel it our duty to give no report this week. It would be far better, we should think, not to raise public expectation, nor appoint a night for discussing the subject of cholera at all, than for gentlemen to stand up unprepared with the grounds of their opinions, making vague assertions, and wasting the time of the meeting in desultory, inconclusive, and unprofitable chat. We except, of course, two or three of the members, who acquitted themselves on this occasion with their usual ability; but the debate,

on the whole—if debate it can be called—does not deserve to occupy any portion of our columns.

MEDICO-CHIRURGICAL SOCIETY.

THIS society commenced its meetings for the season on Tuesday last. Mr. Lawrence, President, in the Chair.

Want of space prevents us from noticing the proceedings this week. The prohibition of reporting has been removed.

NOTE FROM MR. COULSON TO MR. S. COOPER.

— — —
To the Editor of the London Medical Gazette.

SIR,

You will oblige me by inserting in your next number the following note to Mr. S. Cooper.

I am, Sir,
Your obedient servant,
WM. COULSON.

Dear Sir,—I take the earliest opportunity of acknowledging that I committed an error in my clinical lecture in stating that there were no observations in your *Surgical Dictionary* on the treatment of patients previous to the performance of the operation of lithotomy. The passage to which you have called my attention shews that my observations were too unqualified; but I still think that less of your valuable article on lithotomy is devoted to the particular subject I alluded to than its importance demands. Permit me to add, that nothing was farther from my intention than to make any observation calculated to convey an impression that I undervalue a work which I have always considered of high merit and great professional utility.—I remain, dear Sir,

Yours, very faithfully,
WILLIAM COULSON.

Charter-house Square,
Nov. 7, 1831.

S. Cooper, Esq.

NOTICES.

We cannot attend to the *reclamation* of a dissatisfied author: as he has not liked our first, perhaps he would be still less obliged to us for a second notice.

Mr. Lee will find that what he requests was done in No. 2.

In No. 3, we gave eight additional pages without any extra charge; but as we are this week compelled to add an entire sheet, to meet the great press of matter, the price is necessarily raised on this particular occasion.

W. WILSON, Printer, 57, Skinner-Street, London.

THE
LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, NOVEMBER 19, 1831.

LECTURES

ON

THE THEORY AND PRACTICE OF
MEDICINE;

Delivered at the London University,

By DR. ELLIOTSON.

LECTURE VII.

Nosological Arrangements, their inutility—Inflammation, sometimes a salutary process, frequently a dangerous disease—The Synptoms—Redness—Heat—Swelling—Pain.

I HAVE NOW, gentlemen, made the chief prefatory remarks which I deemed necessary before entering upon the consideration of particular diseases,—upon *special pathology*. I have confined myself to an exceedingly superficial view of the subject of *general pathology*, and to the explanation of a few of those terms which I shall most frequently have occasion to employ.

More minute remarks I shall reserve for various parts of the course: for example, when I come to speak of the causes of inflammation, I shall particularly dwell upon the *modus operandi* of various changes and degrees of temperature in producing disease; and when I come to speak of diseases supposed to be contagious, I shall enter upon the subject of contagion. I think it much better to treat these matters in this way, than to treat them in the abstract before entering upon particular diseases; by this means they will not only be better understood, but we shall be relieved from the tediousness of dwelling long at a time upon general subjects.

Now in the first instance I mentioned that one part of general nosology, or one part of general pathology, and indeed one branch of that part of pathology which is called nosology, is the *arrangement* of diseases—one branch, if I may so speak, of general nosology

is, *methodical nosology*. You will frequently hear the arrangement of diseases spoken of simply as nosology, but that is incorrect; the arrangement of diseases is not simply nosology, but *methodical nosology*—*nosologia methodica*.

It was long before an attempt to arrange diseases was made. I believe Felix Platerus was the first who struck out the idea of arranging diseases methodically; he was followed by Baglivi, an Italian physician, and Sydenham. However, it was a French physician who first actually arranged them systematically—Sauvages, who published his work in 1762. It was in that year that the first methodical nosology was presented to the world. After him came the celebrated Linnæus, who arranged every object in nature, and published his nosology at Sweden in 1763. Another physician, Vogel, published one in 1764. No sooner was the thought put in practice than every body went to work. The arrangement of Cullen was published in 1769; another was published by Dr. Macbride, and one by Sogar, at Vienna, in 1771, so short a time elapsed; then one by an author named Vitenus; one by Dr. Thomas Young, the celebrated English philosopher; and one still more recently by Dr. Good. There have been others, but these are the chief.

Now, however useful it may be to arrange diseases slightly, for the purpose of memory, and for the purpose of general views, I think it must be confessed that all these various *methodical nosologies* only perplex and encumber the mind. I studied the arrangement of Dr. Cullen formerly very minutely, and had great part of him at my fingers' ends, but I confess that my knowledge of it now is but of a very superficial kind, and that it was never of any use to me. The arrangements of Drs. Young and Good appear to me just as useless; and I would not, if I advise you honestly—and I hope I always shall do so—I would not advise you to plague yourselves about nosological arrangements. My own

experience tells me that it is a much greater plague to recollect the arrangement and all the hard words than to recollect the things for which the arrangement is made. I never found it of the slightest use, any more than the barbarous jargon of the *Propria quæ maribus* and *Quæ genus* of the Latin grammar.

On the inconsistencies and defects of these various arrangements, I will not dwell; for if you turn to any methodical nosology you will find them in sufficient abundance. You find exceptions made to the plan which the author has laid down in his arrangement, and you find the most gross absurdities. If you turn to Cullen's arrangement, for example, which is one of the best, you will find a large number of diseases daily occurring that have no place in his nosology. Such was his arrangement, that there was not only no place for many most important diseases, but he arranges itch and fracture together. In speaking thus of Dr. Cullen, allow me to say, that although I think little of his arrangement, there can be no doubt that it was far better than that of any of his predecessors; and I must be allowed to pay a tribute of respect to his memory, as one of the most sagacious and one of the best of men who ever joined himself to our profession. I should be very sorry to endeavour to captivate your minds by attempting to lower any one in your estimation; nothing can be more contemptible; and as it regards Dr. Cullen, nothing could be more unjust. You find in his work the utmost candour, and the soundest information; and although he indulged in hypotheses to which some Scotch physicians have been prone, yet independently of that his descriptions are elegant and simple, and you will find his definitions admirable. It is to be remembered that his work on the Practice of Medicine was not considered by him a perfect work, and he modestly called it "First Lines"—a mere outline; but such as they are you will find them, according to the information of that day, exceedingly useful; and it is on account of the excellency of the original work that I should perhaps prefer recommending to your study an edition of Cullen, with notes, to the work of any other individual. I think the advantage of having Cullen's work, with notes, although it should not contain more information, is, that you have the work itself of Dr. Cullen, which was not surpassed in his day, and has not since been surpassed, as far as respects the knowledge of that period. I think you cannot do better than study the edition by Dr. Gregory. There are many other excellent works, Dr. Gregory's, Dr. Macintosh's, Dr. Good's, any one of which is worth possessing, and would form a good basis for study, but I think altogether you will find Cullen's, with notes, the most profitable to you as students.

The most natural mode, in my opinion, in which we can attempt to arrange diseases in our mind, that which serves best for the purposes of recollection—for an arrangement is certainly useful—although I am not an advocate for a methodical one, so called—that which we most naturally fall into, is a two-fold arrangement; first, as to the nature of affections in general, whether they are inflammatory, structural, functional, mechanical, or parasitical; for whenever we see a case we immediately consider what is the kind of affection; and then, secondly, as to which is the part in which the affection occurs. This is the arrangement which I shall follow in these lectures. I shall first consider general diseases, such as affect every or most parts of the body—inflammation, scrofula, and various other organic diseases; and afterwards, having considered all the affections which may attack any part of the body, I shall proceed to consider those affections and all others, functional, mechanical, and parasitical, as they attack the body from the head downwards—a *capite ad culcem*. I think we all make two inquiries in considering any case; the one is the nature of the disease, and the other is the situation of it. This is the utmost assistance, I think, that the memory can have from arrangement, and this is the plan which is continually followed by practical writers. They write on particular diseases, whatever they may be—inflammation, cancer, &c., and of the whole diseases of particular organs, and sometimes of particular regions. You have one writer publishing a work especially on diseases of the urinary organs; another, entirely on diseases of the nervous system; another, entirely on diseases of the head; another, on diseases of the chest; and this course we naturally fall into. But, independently of that, we must make observations upon the general affections to which all parts of the body are liable. I am quite satisfied that this sort of arrangement—by which we consider general affections which may attack any one part of the body, and then consider the affection as seated in this or that part, will afford us all the assistance that arrangement can; and I hope you will never plague yourselves farther with methodical nosology.

Inflammation.

The first disease of which I will speak is the most general of all affections, one which attacks almost every part of the body, at least all vascular parts, and which is the most frequent of all diseases—*Inflammation*.

In the first place, this is the most general of all diseases; it will attack any part of the body that is vascular: in the second place, it is one of the most frequent affec-

tions. It is a disease which scarcely any person escapes; it is a disease which is seen every day. It occurs simply by itself, and it is the concomitant of a variety of other diseases. Many diseases are always inflammatory, though they are not simple inflammation—though there is something more than inflammation, yet inflammation is united with them; and many other diseases are frequently inflammatory, but not always. Besides, inflammation is continually occurring as a means of benefit to the body; it is not only a morbid process, but a process frequently set up by nature for the purpose of restoration, relief, and prevention. It is a disease, too, which proceeds from a vast number of causes—causes to the action of which we are continually exposed, whether we will or not.

Again, it is one of the most dangerous diseases when it affects certain parts, and any part when it proceeds to a certain extent; and it is always liable to be carried to this dangerous point. However slight an inflammation may be, it is always liable to become aggravated to a dangerous point, and it continually does actually attack the most important organs; and hence we may be justified in saying, that the consideration of inflammation is more important than that of any other affection of the body.

When it occurs not as a disease, but for the purpose of restoration—for the purpose of health, if I may so speak—the circulation of the part becomes increased, the quantity of blood becomes augmented, and more nourishment is afforded than before. If a part be destroyed, or divided, a degree of inflammation is set up, and fresh matters are deposited, so that the part is more or less renovated:—when a bone is fractured, a certain degree of inflammation occurs for the purpose of pouring forth a fluid, which afterwards becomes solid, and produces union.

In the next place, inflammation is frequently employed by nature to prevent the escape of matters into parts where violent inflammation would be produced; and is set up by nature to prevent its own occurrence in another spot. For instance, when an ulcer takes place in the stomach, or in the intestines, if it proceed unchecked, the contents of the stomach or the intestines are poured forth into the cavity of the peritoneum, and a violent inflammation is set up—an inflammation, in the greater number of cases, fatal. Nature, however, very frequently sets up a little inflammation outside the organ, exactly at the part corresponding to the ulceration within, by which means it is glued to the surrounding parts, fibrin is poured forth, the fibrin becomes vascular, the parts become still more firmly glued, and then the stomach or the intestines cannot ulcerate through into the peritoneum. Thus, a slight inflammation is set up in one spot to prevent an inflammation which would be fatal to the

patient. You will see the stomach sometimes attached to the peritoneum, and sometimes to the liver.

Then, again, nature employs inflammation frequently to give an exit to something which is injurious to the body. When a biliary calculus is impacted in the ducts, and is too large to escape, it sometimes happens that these ducts become attached by inflammation to the intestines. The inflammation, after it has excited adhesion, proceeds, perhaps, to ulceration, and when the latter occurrence takes place, the calculus escapes through the opening into the intestines. The inflammation, in the first place, is set up to prevent the communication which would afterwards take place into the peritoneum, so that the bile and the calculus shall not escape into it; and when all these parts are firmly glued, and the danger prevented, then, at the particular spot, inflammation goes on to ulceration, ulceration takes place into the intestines, and in that way an exit is given to the foreign body.

When a part of the body mortifies, whether through inflammation or not—yet when a part has become dead, and useless to the body, and injurious to it from its presence—then inflammation is employed for the purpose of separating it. At the boundary of the living part, inflammation is set up; a red line appears at the extremity of the healthy part, where the dead part begins, and that red line is a line of inflammation, which inflammation proceeds to ulceration. A furrow of ulceration takes place, and goes down till the part is ulcerated through, and the dead part falls off, having been separated by the ulceration of the living disease.

You see, therefore, that inflammation, although a most fatal disease in many cases—although conjoined with the most frequent and violent diseases—is sometimes a most salutary process;—sometimes employed by nature to produce an union of parts; sometimes to prevent the great mischief which arises from an effusion of fluids into parts where they ought not to go; and sometimes for the purpose of giving exit to foreign bodies, or separating dead parts from the living.

The general definition of inflammation is, “redness, swelling, heat, and pain;” that is to say, *morbid heat, morbid redness; swelling and pain* require no definition, but redness is common to some parts, and therefore we must understand morbid redness, and with respect to heat, morbid temperature. These are the four chief symptoms of inflammation—those which make the definition in general—in the words of Celsus, “*rubor et tumor, cum calore et dolore.*”

Redness.—These are the general constituents of inflammation, and if they do not all exist in all cases, still usually the greater number are present. Sometimes one

is absent, sometimes another, but the redness is never absent; the redness is certainly indispensable to our idea of inflammation, and it will sometimes exist without swelling, without heat, and without pain; without any of these individually being induced, without any two of them; and now and then the redness will exist alone without any of the three. Whether the state should then be called inflammation or not, may give rise to a difference of opinion. But frequently you have inflammation without any perceptible swelling; you must conceive the part to be a little increased in size, because there is more blood than natural, but without a sufficient enlargement to justify you in using the word swelling—without much increase of heat, and sometimes certainly without pain; and it is to be remembered that both the increase of temperature and redness, by which we understand a morbid degree of them, may arise from other circumstances than inflammation, as may also the swelling and pain.

Still, although redness is indispensable to inflammation, it is not every redness that we have a right to call inflammation; a certain degree of intensity is necessary to the idea of inflammation. How much cannot be specified; but, like sensations in general, it must be experienced to be learned. The requisite intensity will vary in different parts, according to their natural colour. A blush, or momentary rosiness, is not considered an inflammation, and every part is liable to this momentarily in health. As regards the cheeks, a degree of redness there would not be considered inflammation, which we should be fully justified in calling so if it affected other parts of the body. The most intense redness, that lasts only for a moment, we should not call inflammation. It must be in the cheeks—supposing the person has a natural colour—a very deep red, indeed, and more or less permanent, at least not a transient redness, though intense, for us to say that the cheeks are inflamed.

Again, the same degree of redness, the same intensity of it, would not be considered inflammation if alone, which would be considered so if there were also pain and morbid sensibility. If we saw in the cheeks a great redness, we should not hesitate to call it inflammation if we found the part were morbidly sensible, or if the patient complained of it smarting, and being painful independently of pressure. Although the redness might be less intense, and might not continue so long as alone to justify us in calling it inflammation, yet provided it be in a part which is not naturally red, like the cheeks—not liable like them to occasional flushing and rosiness, a much less degree of it, and a much shorter continuance, might give us the idea of inflammation. You see how many things are to be taken into the account. A know-

ledge of these things cannot be gained at once, and many circumstances must be called in to aid us in our judgment. When persons are speaking of redness in connexion with inflammation, instead of the mere expression *redness*, we ought to say *morbid*, or, perhaps, *preternatural* redness; because what is morbid redness in one part is not morbid redness in another—redness preternatural to the part affected.

Now redness is not only of various degrees of intensity in inflammation, but of various hues, being sometimes of a bright scarlet, sometimes rather purple, and of all intermediate shades. Besides varieties of hues in redness, the redness may be distinctly circumscribed in patches, or it may be insensibly lost in the natural colour of the surrounding parts, so that we cannot exactly say where it ends.

Heat.

We will now proceed to the consideration of the other symptoms, and first of *heat*. It is right to say, that for the same reason which makes one adopt the epithet preternatural or morbid, in regard to redness, we should likewise affix the same epithet to heat, because the body, in this country at least, and in most others, is hotter than the surrounding temperature, and therefore it is always hot. A preternatural heat, if not very great, is not of necessity inflammatory; and if it be very great and yet last but for a short time, even then it is not inflammatory. A certain intensity, and a certain duration, are necessary to constitute inflammation in the case of heat, as well as in the case of redness. The greatest intensity of heat, if transient, would not be considered inflammation. The burning heat upon the cheeks, the palms of the hands, and the soles of the feet, in hectic, is never considered inflammation, even though it is combined with preternatural redness. The most intense heat, and also the most continued, is not of necessity inflammation, unless it be united with a pretty permanent preternatural redness. The whole body may be of a burning heat for weeks, as hot as an inflamed part, and yet the skin is not said to be inflamed. The example of hectic shews that even a combination of heat, with intense redness alone, is not sufficient to give the idea of inflammation; for in these intense spots in hectic there is no pain, nor are the parts most affected tender on pressure. Besides that, I need not say that morbid temperature only comes on for a few hours in the day, and is a mere concomitant in hectic from another disease. In addition to this there is no swelling in this affection, and therefore we have still more reason for saying that it is not at all inflammatory.

In regard to heat, the temperature of the affected part appears much higher to the pa-

tient than it really is, which arises from the parts being in a state of morbid sensibility. In palsy it is not very uncommon, when there is no increase of temperature at all—no inflammation—for the patient to feel every thing hot, so that if he sit on any thing without his clothes, it strikes hot to him, or if he put his hand on any substance, whatever it may be, it appears as if it were heated. This is a peculiar state of the system, and not inflammation. There is in inflammation a general morbid sensibility of the part, and therefore the degree of heat which actually occurs produces greater effects than it would in any other part of the body. But although, from the morbid sensibility of the part, the increase of heat appears greater to the patient than it really is, there can be no doubt that in inflammation the temperature is raised. When the nostrils are inflamed, or the bronchial membrane, or the throat, the air that we expire is so heated in passing over the parts, that, as it comes out at the nostrils and lips, it is felt to be burning. If you apply your hand, which is in a state of perfect health, to the inflamed part, you feel that it is hotter than usual; and if you make use of a thermometer you ascertain the point to a nicety—you continually find the parts inflamed hotter by many degrees than other parts of the body; it is not uncommon for them to be 107° . John Hunter made experiments to shew that the temperature of inflamed parts was but little augmented—at least not higher, he said, nor even so high, as the circulation at the heart and the chest; that if the parts had naturally a lower temperature than the centre of circulation—as for instance the hands and the feet—and they became inflamed, their temperature never rose so high as the centre of circulation. The general temperature is said to be 98° , but it varies in different parts; and the farther you proceed from the heart the lower it becomes, because there is less circulation there, and the parts are more exposed to the temperature of the surrounding atmosphere. There is a great mass of substance at the trunk—there is less on the arms—still less at the extremities of the fingers, at the nose and ears, and there is more exposure.

Now John Hunter produced inflammation in the thorax, in the peritoneum, in the rectum, and in the vagina, very cruelly, and he found the heat but one degree and a half higher than before the inflammation, and he frequently found it exactly the same. He says that the temperature of the vagina, at the utmost, increased to $101\frac{1}{2}^{\circ}$, but he found the temperature of the tunica vaginalis only 92° in health, whereas, when inflammation was produced in it, it rose to $98\frac{1}{2}^{\circ}$, 98° being about the natural temperature of the body. It is to be remembered, however, that the scrotum is a part very much exposed to the surrounding air—a depending part, and therefore as much exposed to the sur-

rounding atmosphere as the hands, feet, ears, or nose; and he infers that the heat is never raised above the natural standard at the centre of circulation. If the natural temperature of the tunica vaginalis be 92° , certainly the increase to $98\frac{1}{2}^{\circ}$ is very considerable. He says that he once found the abdominal fluid to be as high as 104° , and he says that if inflammation attack any part with a temperature of 98° , the heat may proceed beyond that of a healthy person. Now, however correct these experiments may be, you have only in the case of erysipelas to take a thermometer, lay it upon the inflamed part and cover it up, and you will then see the temperature, although it be erysipelas of the leg, often raised to 104 , 105 , 106 , or 107 degrees.

That the temperature of the body may be increased very considerably under various circumstances, is shewn by the following facts: for example, Sir Everard Home observed the oviducts of a frog when about to spawn, and when a great local activity of circulation was going on, to be two degrees higher than the temperature of the heart. Now there was no inflammation here, but merely a great activity of circulation approaching to inflammation, such as occurs in the generative process, and the result was as I have stated; and if a mere natural process short of inflammation will raise the heat above that of the centre of circulation, you may well suppose what inflammation will do. Even in vegetables, when a certain process is going on with great activity, a very considerable increase of temperature takes place. When the *arum cordifolium* and the *arum maculatum* are about to burst, the temperature, by placing a considerable number around a thermometer, has been such as to raise it very considerably. Twelve were placed around a thermometer, and so high a degree of heat was developed in the physiological process of bursting, that the instrument was raised from a temperature of 70° to 143° . Even in fever, by placing the thermometer under the tongue, you find the temperature raised many degrees; and the same occurrence takes place in acute rheumatism. In one case of phlegmasia dolens, on placing the thermometer on the thigh, and covering it with the hand or bed-clothes, I found it rise to the same degree of heat that is frequently observed in fever and in acute rheumatism—namely, to 107 degrees. Indeed one gentleman says that, during labour, he has observed the temperature of the uterus, having introduced a thermometer into the vagina, to be 120 degrees. In the violence of the spasm in tetanus, the temperature is frequently as high as 107 degrees. There can be no doubt, therefore, that inflammation is frequently attended by a great increase of temperature: I have seen it myself in various inflammations of the surface. This, however, is not peculiar to inflamma-

tion; for, as I have stated, it occurs in fever and rheumatism, where the thermometer has not been applied to the rheumatic parts, but put under the tongue and in various parts of the body; and it has been observed likewise in many affections of the system.

But as an increased heat may exist without any inflammation, so inflammation—that is to say, redness and swelling, of an undoubted inflammatory character, as proved by the terminations—may exist without any preternatural heat. You will sometimes have inflammation, or what, certainly, I think we are justified in calling so,—that is to say, preternatural redness and swelling, going on to suppuration—without any increase of heat; and this state of the parts has been called *passive inflammation*. Some object to the term, and say it does not deserve the name of inflammation—it is mere congestion of blood; but it will go to suppuration, and therefore I think we are justified in calling it inflammation, although there is no increase of heat. It is right, however, to tell you that some actually object to calling such a state inflammation, and they limit the term “inflammation” to an active state in which there is pain and all those symptoms which I mentioned;—and, to avoid any quibbling, Andral calls any state in which there is a morbid collection of blood, *hyperemy*—excessive quantity of blood in a part: and when it is actively inflamed, *active hyperemy*; and when this passive state occurs, he terms it *passive hyperemy*. Thus he gets rid of the difficulty altogether, by no longer using the word inflammation, but using a word signifying congestion of blood, and applying one epithet or another to it.

Swelling.

If we consider the third of these symptoms—swelling—we shall find that it also may exist without any inflammation. Swelling, although frequently a symptom of inflammation (more frequently than not), may of course exist, just like an increase of temperature—just like an increase of redness—without inflammation. Any thing, almost, will produce a swelling. The mere displacement of a part, the dislocation of a bone, hernia, the mere effusion of hæmorrhage, or a mere collection of water or any morbid growths, whatever their character, or the presence of air within the body, will all produce swelling;—swelling may be produced by many causes besides inflammation. For a swelling to be considered inflammatory, it must be united with pain or a preternatural redness; and even then it may be chiefly dependent on other causes. For instance, in dislocation you may have inflammation of the ligaments and integuments, producing pain; but the swelling arises chiefly from the displaced bone. Again, it is to be remembered that inflammation may

sometimes exist without swelling, as is occasionally noticed in ophthalmia. In ophthalmia, the turgescence often gives rise to no perceptible swelling; sometimes there is a little swelling, but frequently this affection, and many superficial inflammations of the skin, are not attended by any swelling. Sometimes you will actually see a part inflamed in which there is scarcely any pain and no increase of temperature, but excessive redness and excessive swelling; the congestion is extreme, and gangrene, in such a case, is very likely to take place.

You therefore see that not only the symptoms will arise from various other causes than inflammation, but they are out of proportion frequently, when they do occur, to each other. Sometimes a little inflammation is attended by very violent pain, and sometimes a little inflammation, a little increase of heat, is attended by the most violent swelling. There is no necessary proportion between any of these symptoms.

Pain.

Then if you take the last of these marks of inflammation—pain, we shall find that it will arise from many other causes than inflammation. Pain will arise from a mere spasm; nothing is more painful, for example, than cramp in the legs. Pain arises frequently from scirrhus, or cancer: some call this “chronic inflammation,” but certainly there is something more than that. Pain, too, is frequently neuralgic; a violent affection of the nerves, in which no inflammation can be detected during life, and no effect of inflammation discovered after death. Pain continually arises from inflammation, but it continually arises also from the other sources just pointed out; and sometimes it is absent in inflammation. You will see a part violently red, violently swollen, without any pain at all; and nothing sometimes is worse than this, for the part may fall into gangrene.

Just as there may be varieties of redness and various hues of redness, so there may be varieties of pain. The pain is sometimes tense: if the part be one that will not stretch, then the pain is of a tense character, and the part feels as if it would burst. If it be inflammation inclined to suppurate, the pain is of a throbbing character. If it be inflammation of the skin, the pain is frequently of a smarting or of a tingling character. If it be inflammation of a mucous membrane, you frequently have it characterized by a burning and pricking pain. If it be inflammation of a serous membrane, a stabbing sensation will often be experienced, as though a knife were being plunged into the part. There are, therefore, great varieties of pain, just as there are a variety of hues of redness.

PESTILENTIAL CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

A PAPER having appeared in your last number from Dr. Stanley, on the treatment of cholera, I wish to state in the forthcoming number of your excellent work, that the remedies he mentions were previously recommended by me, in an article on the nature and treatment of this disease, published in the October number of the Foreign Quarterly Review. Indeed, an account of the means both of prevention and of treatment there detailed was sent, several months since, to the Russian Ambassador, with the view of having it transmitted to St. Petersburg; but I doubt if my intention was complied with, for my communication was returned shortly after; but whether a copy had been taken of it or not, I cannot say.

Without noticing the facts from which I have deduced the inference respecting the infectious property of this pestilence, I will merely state the conclusions at which I have arrived regarding its nature generally. Your able and conclusive leading article on the infectious nature of the malady, must, together with what has been adduced elsewhere, particularly in the Review alluded to above, have convinced those who are open to conviction of the truth of our doctrine. More is not required on this topic.

Its Nature.

1. The pestilential cholera is propagated by an animal miasm or effluvium, of a peculiar kind, emanating from the bodies of the affected; and this effluvium being inhaled with the air into the lungs, paralyses these organs, and acts as a poison on the class of nerves which supplies the respiratory, the assimilating, the circulating, and the secreting viscera, vitiating also the whole mass of blood, and thereby occasioning a specific disease, which, in its turn, gives rise to an effluvium similar to that in which itself originated, which also, in like manner, perpetuates its kind, under the favourable circumstances of predisposition, aerial vicissitudes, temperature, &c.; and thus a specific state of disease is propagated far and wide, as long as

predisposing, concurrent, and determining causes favour its perpetuation.

2. The morbid impression made by this effluvium, or poison, on the nerves of organic life, and most probably the effect of its introduction also into the current of the circulation, are of a sedative kind, rapidly destroying the vital energy of the former, and vitiating the latter, and thereby giving rise to the characteristic phenomena of the malady.

3. The impression of this effluvium on the organic class of nerves, and the vitiated state of the blood, may be viewed as the proximate cause, not only of the disturbance evinced by the respiratory, the secreting, the assimilating, and the circulating functions, but also of the morbid actions of the stomach and bowels, as well as of the muscular spasms, the sinking of all the vital and animal powers, of the shrunk and collapsed state of the surface of the body, of the black thick state of the blood, and of the rapid depression of the animal temperature.

4. The morbid state of the perspiration, and the peculiar appearance of the evacuations, proceed from the alteration produced in the condition of the blood; and it is chiefly through the medium of the cutaneous surface, and of the mucous membranes, assisted perhaps also by the other secreting viscera, that this morbid state of the blood is remedied, and its impurities removed, in those who recover; and that the morbid effluvium or poison, which propagates the disease, is formed on these surfaces and membranes during the elimination of the impurities generated in the circulation.

5. The consecutive symptoms of the disease, whether those chiefly depending upon the state of the nervous functions, or of the circulation within the brain, or proceeding from the condition of the abdominal viscera, arise partly from the shock received by the vital energy of the frame in the early stage of the disease, partly from the arrest of the natural secretions, and the unchanged and impure state of the blood; and partly from the congested condition of the large veins and important viscera.

6. That neither the causes which occasion this pestilence, nor the pathological states which constitute its various stages or grades of intensity, have any connexion with those of any of the forms of cholera—whether the common *bilious*

variety, or the more severe form usually denominated the *spasmodic*, the *mort de chien*, &c.; and that, therefore, this name should be discarded from all scientific descriptions of the present pestilence.

Such, sir, are some of the conclusions at which an attentive examination of what has been published respecting this disease, has enabled me to arrive. As to the particular denomination, which may be applied to it, I conceive that some one, pointing to its chief pathological states, and its prominent tendencies, ought to be preferred: the intense influence of its exciting cause upon all the respiratory actions and functions, as well as upon the actions of the heart and state of the pulse, and its marked tendency to propagate itself, and to terminate fatally, induce me to apply to it the name of *Asphyxia pestilenta*, or pestilential asphyxia; and I would venture to offer the following definition of it:—

NOSOLOGICALLY.—Great oppression and anxiety at the præcordia, with giddiness, faintness, and general depression of the vital energies of the frame; cold, clammy, purplish, and shrivelled state of the surface; cold and raw state of the expired air; ejection of the contents of the stomach and bowels, with general tremors, spasms, and distress; a sense of oppression, pain, and heat at the epigastrium, with rapid sinking of the pulse, and of the animal heat.

PATHOLOGICALLY.—Paralysis, more or less complete, of the lungs; arrest of the changes effected by respiration on the blood, and of all the natural secretions; the circulation of a dark and otherwise vitiated blood through the arteries, with congestion and stagnation of the fluid in the large veins, sinuses of the encephalon, and internal viscera; sudden depression and rapid sinking of the heart's action; at last, a black, glutinous condition of the blood, with stagnation of it in the large veins and cavities of the heart, followed by death.

The most important considerations connected with this subject, are such as respect the prevention of the disease: when it is fully developed, there is reason to believe that even the most energetic means of cure will entirely fail, in a large proportion of cases. The measures of prevention which I would

especially recommend for adoption, are the following:—The use of sulphate of quinine, with camphor, or the aromatic spices, in moderate doses, taken night and morning, whilst the disease is prevailing in the vicinity; regular living, with a regulated state of the stomach and bowels, by means of tonics combined with gentle aperients: a firm state of the mind, and absence of all apprehension of the disease.

During the occurrence of the malady in our vicinity, or in our families, these means are more particularly required:—A free ventilation of every apartment ought to be constantly attended to, in conjunction with fumigations, by means of aromatic substances kept slowly burning, or by the vapours of chloruret of lime. The attendants on the sick should especially attend to the measures now prescribed, and ought never to exert their attentions on the affected so near their persons as to inhale the effluvia emanating from them, without fortifying the vital energies in the way pointed out; and they should carefully avoid entering upon those duties with an empty stomach, or when fatigued. Besides burning warm aromatic substances and odoriferous gum resins in the apartments of the house in which any person may be affected, a saturated solution of camphor, in aromatic vinegar, or in the pyroligneous acid, should be occasionally sprinkled on the floors of the apartments. These means, with a thorough ventilation and a due attention to cleanliness, will not only, I am persuaded, counteract the influence of the effluvia proceeding from the affected, and ward off its action on even the predisposed, but will also prevent the clothes, bedding, or furniture of the apartments of the sick from becoming imbrued with it to such an extent as can communicate the malady. They are within the reach nearly of all; and, in the event of the extension of the pestilence to any considerable town or city, if care were taken to see them put in practice, under the direction of Medical Councils of Health, one of which should be formed in each district or quarter, much good would result from them.

Having been required, in the various fields of practice, to treat diseases sometimes as rapid in their progress and as fatal under the usual methods of treating them as the malady now the subject

of consideration, I conceive that it will not be considered presumptuous if I recommend a trial of those means which, from my having employed them beneficially in these diseases, I would myself be induced to employ in the one now so much dreaded. When the usual measures fail, others which have succeeded in similar states of morbid action, particularly when they cannot prove detrimental, should be resorted to; and I am not aware that the following means, particularly as respects the combination of them, have ever been employed in this disease—at least at the time when I transmitted them, with more detailed remarks, to the Russian ambassador, or subsequently sent them to the Foreign Quarterly Review.

As I conceive that the exciting cause of the disease makes its first morbid impression on the nerves of the lungs, the propriety of directing the means of prevention to that quarter, and treatment also, will appear obvious. The means which I have noticed for the former purpose have been directed accordingly; but the morbid actions constituting the malady are of that kind that cannot be removed by medication administered in that quarter further than in a subsidiary manner. If, therefore, I were called to a severe case of this disease, besides directing blood-letting, if the circumstances and symptoms of the case appeared to warrant it, I would recommend the patient to have a bolus consisting of from ten to fifteen grains of camphor, an equal number of grains of calomel, one or two grains of opium, and ten drops of any essential oil—as of cloves, mint, cajeput, &c. with a sufficient quantity of conserve of roses. This should be administered without any regard to the presence of vomiting. If this be retained, another may be given, and repeated in from one to two, three, or four hours, according to the urgency of the attack; but if rejected, it should be immediately repeated, until it at last remains. At the same time external heat should be applied, and frictions, with a liniment composed of two ounces each of liquid ammonia, of olive oil, and of camphor, with three ounces of spirits of turpentine, and a few drachms (from three to six) of hard soap and Cayenne pepper, to which one or two drachms of cajeput and lemon oils may be added, ought to be assidu-

ously employed. From two to four hours after the exhibition of the bolus, a draught, consisting of from two drachms to half an ounce of spirits of turpentine and the same quantity of olive oil, with a few drops of the above essential oils and forty grains of magnesia, should be taken in mint water; and if it be rejected from the stomach, another should be given, and repeated, if again rejected, in half an hour afterwards; if retained, not until from six to twelve hours, when another may be taken. I have seen cases where the most urgent vomiting existed, and yet the above remedies (although both the bolus and the draught were taken at the same time) allayed instead of aggravating this symptom.

In order to promote the influence of these means, a lavement, consisting of twenty grains of camphor, from half an ounce to an ounce and a half of spirits of turpentine, and an equal quantity of olive oil, in a suitable vehicle, should be administered, and repeated according to the circumstances of the case. Much will depend upon the succession in which these remedies should be given, the periods which should be allowed to elapse between their exhibition, on the doses, and the decision with which they may be prescribed. When the irritability of the stomach continues, and if the attack be severe, then flannels, wrung as dry as possible out of very hot water, and immediately soaked in oil of turpentine, ought to be instantly applied, as warm as possible, over the stomach and abdomen, and retained there, or renewed, until a decided effect is produced. This is the most powerful means I am acquainted with, and the most successful in procuring reaction and restoring the heat of the body. In aid of these means, and when reaction is either commencing or even expected, effervescing draughts, composed of the carbonate of ammonia, and the pyroligneous acetic acid, mint-water, and aromatics, either in the state of spirit or of essential oils, may be given from time to time. Having found the above treatment eminently successful in treating diseases of remarkable malignity and fatality—in rousing the energies of life, restoring the secretions, and removing internal congestions—more particularly in the treatment of puerperal and other fevers

of an adynamic or malignant character, I have been induced to recommend it in the present pestilence.

JAMES COPLAND, M.D.

Consulting Physician to Queen Charlotte's Lying-in Hospital, Senior Physician to the Royal Infirmary for Children, &c.

Bulstrode-Street, Nov. 12, 1831.

neglect of any precautionary measures, it would, indeed, be lamentable, as thousands might suffer the penalty of the bad judgment of a few*.

I am, sir,

Yours, &c.

A. COPLAND HUTCHISON.

Duchess-Street, Portland-Place,
14th Nov. 1831.

CHOLERA—THE TOPAZE FRIGATE.

To the Editor of the London Medical Gazette.

SIR,

IN common with all my professional brethren, I feel deeply for the consequences of the invasion of the Cholera Morbus into this country; and likewise I feel deeply the awful responsibility to be incurred by those entrusted with the lives of their fellow-creatures. Under existing circumstances, therefore, the establishment of the truth, with regard to the contagious or non-contagious nature of this destructive malady, is of paramount importance; and to have it in my power to correct a misstatement on either side which might lead to erroneous conclusions, and not to do so, I should consider a dereliction of duty. It has been stated repeatedly that the Topaze frigate conveyed the cholera to the Mauritius, from Trincomalee, in the island of Ceylon; which has been denied in the newspapers, journals, and verbally at the Westminster Medical Society, on the 5th instant, by a gentleman whom the audience considered good authority, being informed by himself that he was in India at the time. This gentleman stated positively that cholera had existed at the Mauritius previous to the arrival of the Topaze; that this ship could not have conveyed the disease, for that in point of fact the cholera was not on board.

The subjoined extract from the journal of the surgeon of that ship, now an official document, written and lodged at a public office years before the present crisis was contemplated, will set this question at rest.

Part of the public have been prevailed upon to believe that the disease is not of a contagious or communicable nature. Should such an opinion, or reliance upon such opinions, lead to the

Extract from the Journal of Mr. James Foy, Surgeon of His Majesty's Ship Topaze.

As many of the sick as could be accommodated were sent to the Naval Hospital at Trincomalee, on the ship's arrival there on the 5th September, 1819; they amounted to twenty-six, leaving forty-six on board. Four of the cases of dysentery that were sent to the hospital relapsed there and died. Whilst in this harbour Robert Thomas, sail-maker, who had been employed working on board the hulk previous to his illness, and Charles Plato, a marine, who attended in the purser's steward's room, and was exposed in that situation to sudden vicissitudes of temperature—both died on board of cholera Indica, after a few hours illness; the former on the 16th, and the latter on the 20th September. Some others were attacked with this disease, but they recovered. David Pearce, seaman, a man formerly much employed in the hold, also died at this time, having had a relapse of dysentery. Permission could not be obtained from the commanding officer to inspect the body of the latter.

The ship being ordered to the Mauritius, all but one of the remaining sick belonging to her were embarked again from the hospital, under the impression that the change of climate would be beneficial to them, for but little essential improvement, if any, had taken place in a great number of them whilst on shore. The situation of the hospital is not considered favourable to recovery. The ship therefore sailed in this state from Trincomalee on the 9th of October, having fifty-seven on the sick list; and immediately after cholera Indica broke out, and attacked seventeen, four of whom fell its victims, viz. Wil-

* The official document was put into my hands by my friend, Sir Gilbert Blane, to whom it was furnished by Sir Wm. Burnett, the medical commissioner of the navy.

liam Smith and John Dixon, seamen, who had not completely recovered from dysentery; the latter was one of the men returned from the hospital, and who had been ill with hepatic and dysenteric symptoms ever since he was on shore watering at Singapore, three months before: James Cooper, seaman, an old man sent on board by the civil power, for smuggling, who was much depressed in mind, and had suffered from scurvy on the passage from Manilla: William Steward, seaman, lately entered from a merchant ship.

There died also on this passage to the Mauritius, Esau Sheldrake, ship's corporal, of acute hepatitis, and Charles Farr, marine, of dysentery; the latter had been received in a hopeless state from the hospital.

On the arrival of the ship at the Mauritius on the 29th October, all the sick that were confined to bed, in number thirty, fifteen of whom were of the number received from Trincomalee Hospital, were comfortably accommodated in the Military Hospital at Port Louis, under the care of the military medical officers. The remainder, with the convalescents, were placed in the quarters on Tonnelier Island, which were given up for their accommodation by the General commanding. Six of the men sent to the Military Hospital died there; viz. four of the sequelæ of cholera Indica, with which disease they had been seized on board; one of these four had brought on enteritis by inconsiderate exertion and exposure on board when convalescent; the other two were long standing cases of dysentery. The whole six were of the number received ill with dysentery from Trincomalee Hospital: of the sick who were placed on shore on Tonnelier Island, four died who had been long ill; viz. John Twaites (cook's mate), Philip Harvey, James Coffy, and Francis Lilly (seamen): all of them had recovered from dysentery and cholera Indica a short time before, but remained extremely debilitated, and had a return of dysenteric symptoms, under which they sunk.

Three weeks after the arrival of the ship at Port Louis, the cholera Indica made its appearance amongst the inhabitants, and continued to carry off in Port Louis from fifty to sixty persons daily, chiefly slaves. It appeared im-

mediately afterward in other quarters of the island with equal fury. It was suspected by the inhabitants that the disease had been introduced by His Majesty's ship *Topaze*; for, although the Medical Committee, composed both of English and French practitioners, had, in their public report, declared the disease not to be contagious, nor of foreign origin, yet some of the same French practitioners suffered the inhabitants still to entertain their suspicions, and even appeared to agree with them, notwithstanding their public declaration. The consternation was unbounded and nearly general. In consequence, remonstrances were made against the ship being taken into the Trou Fanfaron, the only place of safety for a ship in the then approaching hurricane months, and the wishes of the General commanding were acceded to, by taking the ship to the Seychelles Islands, where she remained during the hurricane season; but previous to the departure from Port Louis, six men were sent to the Military Hospital, being unfit to be carried to sea; three of them, severe cases of dysentery, died there; one of the latter was of the number received on board from Trincomalee Hospital ill of dysentery, and was afterwards seized with cholera Indica on board; he recovered from both of these, and became convalescent, but from irregularity in his diet relapsed. Dysentery still harassed the ship's company during her stay at the Mauritius; but no case of cholera occurred after her arrival there, although all the merchant ships in the harbour were losing men by this disease. The weather was warm and without rain; the thermometer ranged from 78° to 82°. The cases of dysentery were separated from the rest of the crew as soon as they appeared, by landing them on Tonnelier Island, under the care of Mr. Porteous, assistant surgeon of this ship, to whose ability and uncommon attention these unfortunate men were indebted for their recovery.

WARM AIR BATH.

To the Editor of the London Medical Gazette.

SIR,

I SHOULD feel obliged by your giving insertion to the following case, in which the "warm air bath" was used with decided benefit.

I am, sir,
Your obliged servant,
W. WOODMAN.

Exeter, Nov. 8, 1831.

On Monday morning, November 7th, I received an urgent message to see Elizabeth Sprague, aged 65, residing in a very confined part of the city. On my arrival I found her writhing with agony, and complaining of excruciating pain and icy coldness of the stomach. The surface of the body was quite cold, the countenance anxious, the pulse almost imperceptible. She informed me she had been in that state about half an hour, but had been complaining all night of some pain in the stomach, with cramps in the legs, and had not been well since Saturday; she had not vomited or been purged, and the cramp had now left the legs; tongue clean and moist.

I immediately gave her 30 drops of cajeput oil in a little warm water; in about five minutes she vomited what appeared to be her breakfast; she now felt relieved for a few minutes, but the pain soon returned with its former severity. A large mustard cataplasm was now applied to the stomach, and 20 drops more of the cajeput oil, given in warm brandy and water; she soon after felt rather less pain of the stomach, but the coldness of the surface, the pulse, and other symptoms of collapse remaining unabated, I was induced to use the warm air bath, and am happy to say with complete success, as, in about five minutes the surface of the body became warmer, and in about eight minutes more it was above the natural temperature, with the pulse full and throbbing, and attended by complete relief at the stomach. In the evening she felt quite comfortable, with a soft pulse and moist skin.

My intention in reporting the above case is to call the attention of the profession to its resemblance in some of the symptoms to cholera, and to the facility

with which, in the short space of thirteen minutes, re-action was completely established, the temperature of the body being raised above the natural standard, and the pulse, from being almost imperceptible, becoming full and throbbing. I should therefore earnestly recommend the different Boards of Health to provide a proper supply of the baths, in order that a fair trial may be made of their utility, should the cholera unfortunately appear among us. Its portability renders it applicable in every instance; and without the least fatigue to the patient we are enabled to surround his body with an atmosphere of warm dry air, which, acting as a stimulus to the whole surface, will tend to relieve internal congestion, and restore the cutaneous circulation to its natural state.

INHALATION IN CHOLERA.

To the Editor of the London Medical Gazette.

October 25, 1831.

SIR,

If you think the following hint likely to be of practical use, it is much at your service for insertion.

During the perusal of some papers inserted in your Gazette, particularly one by Dr. Barry in the 194th Number, in which he describes cholera to be most fatal in its first stage, and when accompanied by the following symptoms—"Sudden paralysis, and rapidly diminishing action of the heart, of the arteries, and of the organs of respiration, with stasis and thickening of the blood, the loss of the power to generate heat," &c., the following idea occurred to me, viz. that these symptoms might perhaps be relieved by the inhalation of the protoxide of nitrogen, or dephlogisticated nitrous air of Priestley. Most of the profession are aware of its exciting effects, and that those effects are immediate, and not followed by the depression usually produced by opium or spirits.

I remain, sir,
Yours sincerely,
M.

ABORTION.

To the Editor of the London Medical Gazette.

Lincoln, October 1831.

SIR,

SHOULD you think the following case of sufficient interest, I shall feel obliged by your giving it insertion in the Medical Gazette.

I am, sir,
Respectfully yours,
FREDERIC KENT.

— Lee, aged —, called upon me, in August, 1830, to request me to attend her in labour, which she expected would take place in November. She has had several children, and reckoned with great accuracy. Thought she quickened early in July. I was not sent for till the 13th of December, at which time she was not larger than a woman four months gone with child. She was complaining of violent pain in hips and loins, sickness, great debility, with loss of appetite and sleep. During the last two or three months her abdomen had been distended to the size of a woman at her full time, when the above symptoms were much increased, and accompanied with pain and difficulty in making water, great pain when having stools, and tenesmus. These symptoms were relieved by the discharge of two or three quarts of a brownish-yellow fluid, not in the least offensive, and generally having several pieces of thin membrane floating in it: occasionally the fluid was mixed with blood. On examination per vaginam, I found the neck of the uterus nearly its natural length—the uterus rather larger than a child's head. I now asked her very particularly about her pregnancy, and, from her account, I was satisfied she conceived about the latter end of February or beginning of March. I do not think it necessary to enter into a detail of the treatment. I tried, upon two occasions, to expel the contents of the uterus, by the *secale cornutum*, but with no good effect. She continued to have the occasional discharges of fluid till the 28th of January, 1831, when, after violent flooding, a placenta was expelled about the size of a child's head. In spite of all sorts of treatment she continued, from this time to June, to have repeated floodings, with discharges of fluid. At the latter

end of June she could not get out of bed; kept alive almost entirely by wine and quinine. In September the floodings had nearly ceased, and she was able to walk about the house.

October 20th.—She still has occasional discharges of fluid, but in less quantity, and is gradually gaining strength.

On laying aside the membrane attached to the placenta, the latter presented a surface covered with hydatids of a purplish colour, varying in size, from a pea to a small walnut; several loose folds of membrane, which appear to be remnants of the burst hydatids; a blighted ovum, rather larger than a horse-bean, attached by a funis near the middle of the placenta; eyes and extremities distinct. It was discharged, as nearly as can be calculated, eleven months after conception.

I consider the above case rather singular, as I believe hydatids, connected with the uterus, are always described as pedunculated; and, on referring to Baillie, Burns, and Blundell, I find nothing similar to the above.

OVARIAN DROPSY.

To the Editor of the London Medical Gazette.

SIR,

THE annexed case is at your service, if you think it worthy the notice of your valuable periodical.

I possess notes of a very interesting case of asphyxia, from hanging, successfully treated. I opened the larynx, and inflated the lungs with a common elastic gum catheter. Also three of lithotomy—respective ages, $2\frac{1}{2}$, 8, and 70, in which I used, with very great satisfaction, Frère Come's instrument, the *lithotome cachée*, so much condemned by some eminent surgeons. It possesses all the advantages of the knife, without that great defect—not being able to regulate the extent of the incision through the prostate.

Should you wish these, I will with much pleasure send them to you.

I am, sir, a subscriber,
and your obedient servant,
THOMAS FEREDAY.

Dudley, Oct. 31st, 1831.

Case of Ovarian Dropsy, complicated with Psoas Abscess and Pregnancy, spontaneously subsiding.

Mrs. Hall, æt. 40, of naturally healthy constitution, and fresh-coloured countenance, had a child in the year 1810, ten months after marriage, and in the twentieth year of her age. Her husband died, and she again married in 1814, and eighteen months afterwards had a second child.

About eight years ago she had uterine disturbance to such a degree as induced her medical attendant to tell her she would bear no more children. What this affection was I cannot satisfactorily learn, the practitioner to whom she applied being dead. She says that a purulent discharge continually issued from the neighbourhood of the uterus, and that a large bougie was occasionally introduced up the vagina. From this and the attendant constitutional symptoms she recovered in about four years.

Menstruating regularly, she again conceived about Midsummer, 1830, sixteen years from the birth of her last child, attended, during the early months, with an aggravation of the usual symptoms of pregnancy. These subsided, but recurred towards its close, accompanied with pain in the loins, a tenderness and tumefaction in the right iliac region, pains in the hips, numbness of the right thigh, a disagreeable taste in the mouth, and slight fever.

March 10th. — The predominant symptoms are the shooting pains in the loins, the numbness of the thigh, and the nauseous taste in the mouth, compared by the patient to rotten eggs. There is an enlargement, about the size of a child's head, with rather indistinct fluctuation to the right of the spinous processes of the lumbar vertebræ.

23d. — The pain in the back has been so severe for the last week that the patient has been confined to her bed. There is evidently a deep-seated fluctuation. A puncture discharged about a quart of good pus, with great ease to the patient, but had no apparent influence upon the ovarian (?) swelling. The wound was closed with sticking plaster, and a flannel roller bound round the loins. In the course of the evening, and after some hours severe pain, the dressings gave way, letting out a quantity of matter, nearly equal to that withdrawn in the morning, and with as great relief.

24th. — There has been severe pain in the back, shooting from thence along the course of the psoas muscle, with much bulging in the former part. For the first time, too, an oblong swelling, about the size of a large hen's egg, is observed midway between the crista of the ilium and the os pubis, above Poupert's ligament, and apparently in the direction of the inguinal canal. The ovarian (?) tumor remains pretty nearly in the same state. The contents of the abscess in the back were again discharged, by which the inguinal swelling was lessened.

In the night labour came on, but so reduced was the patient, from want of rest, suffering, and great secretion, that she became alarmingly exhausted after a few hours continuance of labour. The mouth of the womb being fully dilated, and the head of the child at the brim of the pelvis, (too high to admit of the application of the forceps with effect) I judged it more advisable to deliver by turning, than subject my worn-out patient to a repetition of inefficient pains, gradually becoming weaker. I effected this with very little difficulty, and no violence, the uterus scarcely recognizing the presence of my hand. The placenta and membranes were expelled in about a quarter of an hour by the natural contraction of the womb, the latter organ being distinctly felt as a round hard ball in the hypogastric region. Yet the abdomen did not appear lessened, and it was thought by the attendants that there was still a child *in utero*. It was evident, however, that the enlargement occupied principally the right side. After remaining quiet a full hour, I had her very quietly put to bed, and administered thirty drops of laudanum in a little warm wine and water.

25th. — The swelling in the iliac region continues, as do the pains in the hips, the numbness of the thigh, and the disagreeable taste in the mouth. There is about the usual quantity of lochial discharge.

In the evening of the 26th I was sent for in great haste in consequence of a great and very sudden discharge of watery fluid from the vagina, estimated by the attendants at two or three gallons, but admitting of no accurate measurement, as it escaped among the bed-linen. The ovarian (?) swelling disappeared; the pain in the hips, and the

numbness of the thigh, subsided; and, for the first time for a month, the patient had a comfortable night's sleep. From this time she became convalescent; the discharge from the back continued unabated for about a fortnight, and then very slowly, but gradually decreased; a small quantity, however, still remains, with but little pain, and scarcely any inconvenience. The ovarian (?) disease, with its attendant symptoms, vanished; the pulse, which during the whole progress of the case had been accelerated, became more quiet; and the tongue, which had been coated with a white fur, became clean; whilst by nourishment and fresh air she acquired sufficient strength to suckle her child. Both are now, (October) and have been for some months, stout and healthy, the mother having long resumed her ordinary domestic avocations.

In relating this case, it is obviously not with an intention of describing the treatment adopted: that was necessarily very simple, and such only as tended to relieve general and urgent symptoms. This, therefore, I have not deemed it necessary to introduce.

The great peculiarity consists in the spontaneous discharge of a thin and limpid fluid from the vagina, attended with the immediate subsidence of an intumescence in the right iliac region, together with its attendant consequences, pain in the hips, numbness in the thigh, and disorder of the stomach. These symptoms are readily accounted for; the former from the pressure of the swelling upon the anterior branches of the lumbar and sacral nerves, the latter from the universal sympathy which exists between the generative organs and the stomach.

Now it would seem that the right ovary was predisposed to disease in consequence of the disturbance that existed in the uterus years back, when, possibly, adhesive inflammation had agglutinated the fimbriated extremity of the fallopian tube to the ovary, and that the process of utero-gestation had proved the exciting cause to the dropsical effusion, (in which, perhaps, the fallopian tube was implicated) which, by the bursting of the sac which contained it, had been transmitted through the tube to the uterus, and thus effected its escape.

It could not have been dropsical effusion of the uterus, for that would have

required a closure of the cervix during its formation, which would have been incompatible with a constant and free discharge of the lochia. The same may be said of that irritable state of the vessels secreting the liquor amnii to excess, as mentioned by Dr. Mason Good. It could not have been an hydatid, for it must have had an envelope; its expulsion, too, would have required much uterine pain; in addition to which, the uterus was distinctly recognized as a firm and hard tumor in the hypogastric region, totally distinct from that I conceived to be the ovarian enlargement.

It is universally admitted that a vesicle in the ovary bursts from the stimulus of the seminal fluid, and that the ovum is conveyed from thence through the fallopian tube to the uterus; and why, by a sudden movement of the body, or other agent, may not the sac of an ovarian dropsy be lacerated and its contents escape into the uterus?

If, then, this view be correct, it must be considered a very fortunate and rare termination of a disease very little under the control of medicine. Such an event must not frequently be expected.

I have purposely avoided much notice of the abscess, evidently a psoas. It was unusually rapid in its formation and reproduction, and unattended with disease in the vertebræ. It had been punctured in the back before it pointed in the groin. A depending opening would certainly have expedited the healing, but one was already made, and I hesitated making another through the aponeurosis of the abdominal muscles, thereby rendering weaker a part already too susceptible of important diseases.

AMPUTATION OF HIP-JOINT.

To the Editor of the London Medical Gazette.

Glasgow, Nov. 12, 1831.

SIR,

MAY I request the favour of your publishing, in the next number of the *Gazette*, the following case of successful amputation at the hip-joint, which was under my care in the infirmary here during last summer.

I am, sir,

Yours, &c. &c.

JOHN MACFARLANE, M.D.

Senior Surgeon to the Glasgow Royal Infirmary, &c. &c.

Elizabeth Kerr, two years of age, was admitted about 11 o'clock of the forenoon of 6th July last, on account of a compound fracture of the left femur, in its upper third, with extensive laceration of the muscles and integuments. This severe injury was produced, an hour and a quarter before, by the wheel of a loaded waggon passing obliquely across the upper part of thigh. The bone was fractured transversely, a little below the trochanter, and completely denuded of integuments and muscles anteriorly, to within an inch of its condyles. Posteriorly the soft parts were also dreadfully lacerated and contused, especially at the upper boundary of the popliteal space, where the limb was only retained by a narrow flap of integument and muscle. The femoral artery and vein were torn across, opposite the fracture, but the bleeding was trifling, and consisted only of a slight oozing from the latter vessel and from the lacerated surface. There was also a compound comminuted fracture of all the phalanges of the left little finger.

The child was carried to the hospital from a distance of two miles, the injured limb being extended on a pillow. The countenance was pale and anxious, the pulse rapid and feeble, and the body cold. The usual expedients for exciting re-action were promptly adopted; heat was applied to different parts of the body, and warm wine negus administered. A consultation was immediately called, and I saw her for the first time at a quarter to 12. She still lay in a state of collapse, but was quite sensible, and cried bitterly when the limb was examined.

From the nature and extent of the injury, it was sufficiently evident that the child's life could only be preserved by amputation at the hip-joint. This operation I was anxious at once to perform, but from the absence of my colleagues it had to be delayed, and a second consultation summoned at 1 o'clock. At this hour I was favoured with the presence and assistance of my friend, Dr. Perry, with whose concurrence I immediately proceeded to the operation.

An outer and an inner flap were formed by transfixing the limb with Lisfranc's knife, which was passed so closely around the head of the femur as completely to divide all its muscular coverings; the capsular ligament was opened with a scalpel, the bone disar-

ticated, and the limb removed; the femoral artery was effectually compressed by an assistant, and not more than six drachms of blood were lost; five vessels were secured by ligature, after which the flaps were brought together, and retained in apposition by adhesive plaster, compress, and bandage.

During the operation the child was exceedingly weak, and before the wound could be dressed it was in a state of syncope, and appeared to be dying; the pulse at the wrist was imperceptible, and it was even with difficulty the pulsations of the femoral artery in the wound could be discovered; the face was deadly pale, and covered by clammy perspiration; the body cold; the eyes fixed in their sockets; the pupils dilated; and the breathing hurried, interrupted, and laborious. Stimulants were freely administered, and the child was removed to bed, having been about twenty minutes on the table. She lay in a state of collapse and insensibility till 7 p.m. when, after vomiting once or twice, reaction was gradually established. At 8 the stimulants were discontinued; the pulse was 130; skin hot and dry; the fractured finger was now removed, and a poultice applied.

Ol. Ricini, 3ij. et post hor. duas enema domestic. Mistur. Diaphor. c. Vin. Antimon. 3tia q. q. hor.

7th.—The child is not weaned, and has been sucking freely; was much annoyed with startings during the night; pulse 140; skin hot and dry; respiration hurried; no oozing from wound.

Submur. Hydr. gr. ij. et rept. post hor. quatuor. Contin. Mistur.

Half-past 7, p.m.—Has been in a drowsy state since two o'clock, from which she can with difficulty be roused; eyes clear and sensible to light; pupils contracted; increased heat of skin, and flushing of face; troublesome startings; pulse 160, feeble; no stool.

Enema domestic. Abrad. Capill Bladder with iced water to head. Contin. alia.

8th.—Is more alert, and answers questions readily; respiration still hurried, and there is mucous râle in trachea; pulse 160; bowels free.

9th.—Had rather a quiet night, with less starting; tongue cleaner. On undressing wound, flaps were found ad-

hering throughout, except a small portion at upper angle, which is sloughy.

Omit. Medic. Four ounces beef-tea.

11th.—Pulse still very rapid, but febrile excitement diminishing. Edges of wound clean, cut granulations pale.

Vin. Rubri, $\mathfrak{z}\text{ij}$. in dies.

13th.—Was more restless last night, and is troubled with cough, dyspnoea, and occasional vomiting. Mucous r  le is heard over the whole thoracic parietes. Countenance rather sunk; frequently refuses the breast; pulse very rapid and weak.

Mistur. Mucilag. c. Tr   Opii. Calomel, gr. ij. 4ta q. q. hor. Contin. Vin.

Without continuing the diurnal reports, I may shortly state, that the symptoms of bronchitis continued rather urgent till the 19th, when they began gradually to diminish. From the 22d till the 6th of August, although the wound continued steadily to heal, there existed a good deal of irritability of the bowels, accompanied by diarrh  a, and an aphthous state of the mouth. During this period, three molar teeth cut the gum, and ultimately gave relief to the symptoms. The wound gradually healed; the child improved daily in strength and spirits, and was dismissed cured on the 13th of August.

There were several points in the history and progress of this case which rendered the prospect of its ultimate success extremely problematical. The extensive nature of the injury, and the alarming collapse which it produced, might have reasonably deterred almost any surgeon from undertaking, in circumstances so apparently hopeless, the performance of so severe and formidable an operation as that of amputation at the hip-joint. On first examining the injured limb, I was induced to view the case as almost, if not altogether, hopeless. It was only, however, by amputation that the child's life could be preserved; and although I was aware that this operation might, in a subject so young, and already so much sunk by the shock of the injury, be productive of immediately fatal consequences, I considered that I should have failed in my duty had I not recommended and adopted it. I regretted at the time the delay that took place in obtaining a consultation. The child was in a more favourable state for amputation at eleven than at one;

and had it been performed two hours sooner, I am convinced that the shock to the nervous system would have been lessened by its nearer approximation to, and by its becoming in some measure blended with, the shock of the injury. It was evident that the collapse which existed before, and for about five hours and a half after amputation, was not occasioned by loss of blood, but depended on the depressed condition of the nervous system, produced by the injury and aggravated by the operation. With the view of lessening as much as possible the injurious effects of the operation upon the nervous system, I was anxious to perform it rapidly, and with little loss of blood. Both these objects were in part attained. The formation of the flaps, disarticulation of the femur, and removal of the limb, did not occupy a minute and a half, and not more than six ounces of blood were lost. I am no advocate for the hurried performance of operations: nevertheless, it must be admitted, that when the pain and irritation of a tedious and protracted operation come to tell, as they must do, injuriously on a constitution which has already sustained a violent shock, the most serious consequences may be anticipated; and we are certainly warranted, in such circumstances, in expediting the operation as much as is consistent with its safe and efficient performance.

The youth of the child was also inimical to the success of the operation. The physical irritability which exists during the two or three first years of childhood is so great as to render the performance of any capital operation extremely dangerous. I am not aware that there is any case on record in which amputation of the femur at the cotyloid cavity has been performed on so young a subject; and I have been chiefly induced to communicate this case to the profession, in order to shew, that in serious injuries inflicted on young children we ought not altogether to despair of success, even in the most desperate circumstances, but to proceed with those operative measures which the peculiarities of the injury may demand. I must state, however, that I have witnessed amputation of the hand twice on children under three years of age, and in both cases convulsions supervened, and proved fatal in less than twenty-four hours. We would not will-

ingly select such cases for operation, neither should we decline to use the knife when it is absolutely called for.

We had not only the immediate dangers of the operation to deal with in the case of Kerr, but we had also to contend with a series of untoward occurrences during the progress of the cure. 1st. The collapse was unusually severe and protracted; and, as generally happens, was productive of excessive excitement. 2d. The continuance of this febrile excitement, for eighteen hours, produced an affection of the brain, accompanied by partial coma. 3d. The occurrence of bronchitis, which was rather protracted and severe, was another source of danger; and, 4th, the existence of troublesome diarrhoea, with dentition. The child not having been weaned was, I think, also detrimental to its recovery. The continued anxiety of the mother; her absence from her family, and her close confinement in the hospital, occasioned a continued diminution, and, occasionally, an almost complete suspension of the secretion of milk; and as the child refused spoon-meat of all kinds, it must be obvious that she was often but imperfectly nourished.

Since she was dismissed from the Infirmary, she has completely regained her health and strength, and she is now able to move about the house with the aid of crutches.

MEDICAL GAZETTE.

Saturday, November 19, 1831.

"*Lacet omnibus, licet etiam mihi, dignitatem Artis Medicæ tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso.*"—CICERO.

MODE IN WHICH CHOLERA IS PROPAGATED.

WE last week laid before our readers a *resumé* of those considerations which had induced us, after mature investigation, to conclude, that the cholera was a contagious disease—by which term we understand, a complaint capable of being transmitted, mediately or immediately, from the sick to the healthy. To our minds the narrative, when examined as one continuous chain of events, is convincing; but to produce its impression, the question must be considered in this manner,—if

we content ourselves with a few links only, or view with microscopic eye certain detached facts—we naturally magnify them into unreal dimensions, and lose sight of the general bearings of the whole. Now this is precisely what the non-contagionists appear to us invariably to do: they bring forward certain cases, in which individuals exposed to cholera have escaped, and draw from them the illogical inference, that those who have been similarly exposed, and taken the disease, have done so from some other cause than the exposure. Let us consider this matter a little farther. One of the persons cut off at Sunderland the week before last, was taken to the hospital; of the nurses employed about the patient, one took the disease, and the others escaped. The only rational deduction from these premises is, that so far as one example goes, they shew cholera to be contagious, but not necessarily to affect all who may be exposed to it. But the anti-contagionists are not content with this: they say, no; the escape of the nurses, who did not become affected, neutralizes the evidence. Now let us apply the same mode of reasoning to the agency of any other influence external to the individuals—let us, for the sake of argument, suppose cholera to arise from one of the many hypothetical explanations which have been offered; *e. g.* subterraneous changes. The nurses inhabiting the same dwelling, must all have been exposed to this same subterraneous incomprehensibility; but as some did not take it, therefore she who did, must have taken it from some other cause than that which left the others uninjured. It is thus quite evident, that no argument can be drawn from the exemption of those who escape, which does not apply to all other supposed sources of the malady quite as forcibly as it does to contagion. Again, it is said that the nurse who died was very much afraid of taking the disease

—a most unfortunate argument for the side it is intended to uphold, because fear is universally admitted to render individuals more susceptible of diseases which are communicable, and serves to explain the circumstance which made her system yield to an impression which others resisted. Nor is there any way of getting over this inference otherwise than by holding, that the nurse died of fright, and not of cholera; but few, we suppose, will be disposed to go as far as this.

We have adduced the preceding illustration, because it has actually been brought forward since our former article was written, as an argument against the accuracy of the views which we adopt; whereas, it proves only—what every one knew before—that all who are exposed to the contagious influence of cholera do not take the disease, any more than all fall sick of small-pox, measles, or scarlatina, who are exposed to their several contaminations. This, however, leads us to make some observations on the nature of the communicating principle, for which neither time nor space served us last week. It appears to us, that the proportion of mankind, who are susceptible of cholera, is smaller than holds good with regard to the other transmissible diseases; but that they who are susceptible, have the susceptibility in a more exquisite degree, so that a much more considerable dilution of the poison becomes necessary before it is rendered innocuous. This dilution generally took place to a much greater extent in India than in Russia, and hence the propagation of the disease by transmission from man to man was much less conspicuous in the former country than in the latter. In India, the spare garments of the inhabitants, their copious ablutions, and the universal admission of air into every portion of every dwelling, tended so much to counteract the influence of the poison, that many were constantly

exposed to it with impunity, and hence the true mode of its propagation long escaped detection; but in Russia, the sheep-skin clothing of the lower orders, which is but rarely changed, the stove system, and the close apartments of all ranks, from which indeed every breath of air is carefully excluded, contributed to present the deleterious impregnation in a more concentrated form, and hence the fact of the disease passing from man to man there, became much more apparent. The result, in both cases, was such as might have been expected. Dr. Russell came from India impressed with the opinion, that the disease was not communicated from man to man; he comes from Russia convinced that it is. Dr. Lawrie, an intelligent surgeon from India, observes, “I feel myself called on to say, that, while in India, and even after I had commenced writing the present remarks, I was convinced that cholera was not contagious. I must candidly confess, however, that a closer investigation of the history of the Indian disease, added to a due consideration of the progress which cholera is still making on the continent, has shaken my previously established opinion*.” Another Indian practitioner presents himself in Mr. Kennedy, who has gone so far as to assume the principle of transmission in the name he has bestowed upon the disease,—that of the “contagious cholera.” And Mr. Orton, known as the author of a most valuable work on Cholera, after enumerating the facts in favour of non-contagion, adds, “These facts appear so clear and strong that if we looked no farther, they would seem to put the matter at rest; but with regard to the general question of the contagious, or (if another term must be used) infectious nature of the disease, *they appear to me no more than dust in the balance against those which may be opposed to them.*”

No country could be more calculated

* Glasgow Medical Journal, November.

than India to prevent the communicable nature of the disease from being forced upon the attention, because the universality of thorough ventilation constantly enabled many to be exposed to the sick without harm, and these daily instances of immunity diverted men's minds from the truth. On the other hand, probably no quarter of the civilized globe could be better adapted than Russia to bring into obvious and unequivocal display the full development of that principle, from the pernicious influence of which the greatest peril is in concentration, and the only safety in dilution. This doctrine cannot be impressed too strongly on the public mind with a view to the common safety. All can understand, that if a dose of arsenic be put into a glass of water, it cannot be taken without almost certain death; that if it be put into a cistern, there would still be danger, though not to the same degree; but that if it were cast into the Thames, the whole town might drink of it with impunity.

That so many escape who are in close attendance on the sick, proves how few comparatively are susceptible of the disease; that so many become affected without obvious exposure, proves how great the dilution is which is requisite to rob the poison of its power over those whose systems render them open to its attacks. It seems clearly established, that approximation to the sick is by no means necessary to the production of the disease, and consequently the links which form the chain of communication are constantly subject to interruption. Thus we are informed by Drs. Russell and Barry, "that neither the near approach nor the immediate contact of an infected individual were indispensable to the infection of a healthy individual." From this it would appear that the influence producing cholera is of a more diffusible, though not less energetic, character than that which emanates from small-pox, or any of the other avowedly infectious maladies.

Now there are two modes in which a poison of this description may be supposed to be conveyed to the previously healthy, without their approaching the sick: first, we may suppose it to impregnate the atmosphere, and be wafted by currents to a distance more or less considerable; and, secondly, that it may be carried by the persons or garments of individuals who themselves resist its impression, or by goods of various kinds. Of both these, numerous illustrations might be adduced, but a few must suffice. Of the former, a striking instance, which we subjoin, is given in the Madras Reports.

"Of two vessels, arriving and remaining at anchor in Madras Roads, one was attacked with the epidemic ten days after arrival there, and the other remained free from it for a fortnight longer; but on the first, in which it was still prevailing, changing her birth, and taking up a situation a quarter of a mile to the windward of the healthy vessel, it was immediately attacked; and those who slept on the side next the infected ship suffered particularly*."

And again: when the disease was raging at a town in Persia, a body of troops passing in that direction "were prevented, by guards situated at the gates, from entering it; but several of them passed the day under the walls. During the following day the disease manifested itself among them, and they suffered from it very severely†."

The above, though strong, are by no means solitary examples of this law; so far otherwise, that it is in accordance with general observation, that places to the leeward of an infected locality were, *ceteris paribus*, decidedly more apt to suffer than others.

The power possessed by those who have been exposed to and imbibed, but without being themselves brought under, the influence of the poison, is a circum-

* Madras Reports, p. 25.

† Med. Chir. Transactions, vol. xii. p. 359.

stance of still greater importance, because more generally doubted, and more calculated to lead to the unsuspected and unintended propagation of the disease. The opinions to this effect, however, and the examples adduced in attestation of its accuracy, are too numerous and too well authenticated to be got rid of by any summary expression of incredulity. Hear the account given by the Captain of the *Bellona*, a Russian man of war:—

“ ‘The *Bellona* frigate, 44 guns, 320 men, had 16 sick of cholera; first case on 30th June, (O.S.):—This man had not been on shore. *The first communication which the ship had with the shore was on the morning of that day, when the captain, some of the officers, and a boat's crew, went to Cronstadt.* In the evening the first man was seized, and in quick succession fifteen others, not one of whom had been on shore. No officer was attacked.’ ”

Again, we are informed by Captain Baron Loeventhall, that

“ ‘The *Phoenix* brig had been stationed for two months midway between Cronstadt and Peterhoff, about six versts, or four miles, from either, to the S.E. of the former, for the purpose of telegraphic communication with his Majesty. She continued in this situation to the 15th of July, in perfect health, though she sent her boat twice a week to the fortress for provisions. On the 15th she took up her station one verst to the S.W. of the town, and had two men attacked with cholera on the 17th. Neither of these men had been on shore. The surgeon only had gone to Dr. Lange's hospital on the 16th, for the purpose of observing the cholera, which he had not seen before. The doctor said to him, ‘*You had better not remain here long, else you may carry the disease to your ship, and I shall be responsible*’.”

Another circumstance, which tends strongly to establish the same position, is, that cholera first broke out among the Poles after an engagement with the Russians; and a second time, in a different quarter, the Polish army, which

had been separated from its own sick, was again affected with the disease, immediately after an engagement. Now, from the acute and violent nature of the malady, it cannot be imagined that those actually labouring under it could go to battle; the legitimate inference, therefore, is, that they carried with them the contamination, whatever it may be, and communicated it to the opposing army with which they came into contact, not merely at the moment of contest, but many of them subsequently as prisoners. “It seems tolerably well ascertained,” says Dr. Russell*, “that the cholera has not broken out spontaneously in any place without communication by persons or effects coming from infected places; but it is somewhat singular and unaccountable that the disease has appeared in situations where the persons arriving did not themselves labour under the disease at the time of their arrival.” Certain it is, that persons not obviously labouring under the disease appears frequently to have been the medium of its communication; and this is a very important fact, both as serving to account for the difficulty which frequently exists in tracing out every link in the chain by which it is transmitted from man to man, and also as affording a necessary caution with regard to the degree of intercourse to be held between the healthy and the attendants of the sick. Generally speaking, the period intervening between the effectual exposure and the attack is very short, not exceeding a few days, but in other instances an individual may be exposed to the virus, without its influence being developed for a considerable interval, and he may thus transport it to a greater distance. We are informed that “a convict was sent from a village where cholera prevailed, to Chanda, eighty miles off. Two hours after his arrival he was attacked, and speedily

* Official Papers, p. 54.

* Private letter.

died. Three of the four persons who carried him to the grave, and another who attended them there, were the next sufferers; and shortly the disease became general*."

The inference from all this is, that an individual may commence a journey apparently well, and travel on transmitting the disease to others as he passes; or, on the other hand, if on board ship, he may have already escaped the first stage of cholera, and be on the sick list merely as labouring under fever. "Nothing (say Drs. Russell and Barry) is more certain than that persons may come to the coast of England, apparently labouring under common feverish indisposition, who really and truly are cases of cholera in the second stage†." These observations, we trust, will receive the attention they deserve from all concerned in the planning or execution of measures connected with the public health: even those who think differently from us on the subject of transmission, will still probably admit, that while any uncertainty is attached to the question, the public are entitled to the benefit of the doubt.

CHOLERA AT SUNDERLAND.

THE disease at Sunderland seemed for a few days to have been checked, and the fears which its announcement excited were quickly subsiding—indeed many expressed their conviction that *the* cholera had not been there at all. When, however, we call to mind the capricious and insidious manner in which the pestilence has repeatedly behaved abroad, we fear that the confidence with which its immediate disappearance is inferred, is at least premature. In a place so dependent upon its trade, and with so many interested in concealment, from the Noble correspondent of the *Standard* downwards‡, the difficulty

of getting at the truth becomes prodigious. Indeed the most contradictory statements have been put forth, on the authority of the medical men at Sunderland, with regard to the disease and its origin. They are very anxious to prove that it was not imported, and that it is not of a contagious nature. They add further, that the town is more healthy than it usually has been at this season of the year. However, it remains undisputed (only because it is indisputable) that suspected vessels were suffered to come up the river, through the shipping to the Wear, to perform quarantine; and that at least one such vessel had left Hamburgh after the disease was acknowledged to be there. It farther appears, that in Sunderland the disease first broke out among persons connected with the shipping, and spread to several members of the same family; while a nurse, who assisted to remove the body of the first patient who died in the hospital, became affected with the malady in a few hours after. Now, without going farther, we crave leave to ask, how the sudden appearance of a disease, which carries off, within a few hours, more than one-half of those attacked with it, is in their opinion to be accounted for, in a town *unusually healthy*? and by what train of reasoning they have been able to come to the positive conclusion that the vessels from the infected place did not bring it? One question more, and we have done. What kind of *diarrhœa* may that be to which several sudden deaths are set down, at a time when the town is "*unusually healthy*?" Dr. Daun complained, in his reports, of want of co-operation, and it is quite evident that concealment to a considerable extent was, and probably still is, adopted. Meantime he has positively declared the disease to be *the* cholera, whatever its origin may have been, and whatever its future course; an opinion in which the Central Board in London fully

* Mr. Montgomery. Medical Repository, May 1826.

† Official Papers.

‡ See the Marquess of Londonderry's letter in the newspapers.

concur. Although held at Sunderland, as at almost every other place when actually present, not to be communicable from man to man, the utmost precautions are taken to prevent the operation of the contagious principle. This is all we want: be practically contagionists, and we care not a rush for theoretical differences of opinion.

MALIGNANT CHOLERA AT SUNDERLAND.

New cases. Deaths.		
Up to Nov. 3	7	5
4	0	0
5	0	0
6	1	0
7	6	2
8	3	5
9	17	10
10	7	4
11	1	2
12	2	0
13	0	1*
14	7	3†
15	3	5†
	59	37

THE COURIER v. THE MEDICAL GAZETTE.

THE Courier has replied to our exposure of their inconsistency last week, but in a subdued tone, and without being able to show that we were wrong on a single point; in fact, the case did not admit of it. They say it is "impertinent" to call Dr. Johnson's series of letters, in their paper, "protocols:" we know, however, that, in expressing our disapprobation of medical men embracing the opportunity afforded by public excitement, to bring themselves forward through the medium of the daily papers, we do but echo the sentiments of the profession at large. It is a system of advertising, let gentlemen disguise it as they will, and one against which we always have and always shall set our faces, come from what quarter it may. Dr. Johnson, we observe, is now a "contingent contagionist," and he talks

of cholera being not "*very infectious*," *à la bonne heure*: if infectious at all, those on whom the attempt to exclude it devolves, ought not to be told, that they would be held blameless, though the disease should pass their boundaries. As to the rest, no man regards the disease as necessarily communicated to those exposed to its influence, except other "contingencies," such as predisposition, conspire to give it effect*.

BOARDS OF HEALTH.

THE new arrangements connected with the public health, which we announced last week, have been carried into effect. We understand that the members of the original Board are to constitute a Committee of reference, for the guidance of the Privy Council, with regard to medical questions, on such occasions as their advice may be deemed requisite; while the new, or "Central Board," will devote their whole time to the details, and take upon themselves the entire executive department.

LOUIS-LE-GRAND'S PRESENT TO HIS APOTHECARY.

THERE is at Mr. Garden's, the chemist, in Oxford Street, a superb vase of rich-toned bell-metal—a specimen of the age and munificence of Lewis the 14th. It is a noble mortar, measuring nearly two feet in diameter at the mouth, and weighing upwards of two hundred and eighty pounds—presented, with characteristic propriety, by the *Grand Monarque* to his beloved state apothecary. In the founding, the following inscription was cast upon it in relief:—"Charles Angibaud, M^e. App^e. et Ordinaire du Roi. A Paris, 1678:" this goes round it beneath the upper margin. On the body is a shield of emblematical heraldry, having reference to the tree of life, and supported on either side by the arms of France. Two rams' heads project from the body by way of

* Besides one death of common cholera.

† Besides one death of diarrhœa.

* In our last number, article headed "Courier v. Medical Gazette, p. 214, for 'the Editorial writers,' read 'Editorial authorities;'" and in the quotation from the Times, at the conclusion, for "(provided there be time)," read "(provided they be timely)."

handles, though, if M. Angibaud could lift it by them, he must have been able to effect more than any two of his descendants, "in these degenerate days." The whole is in the most beautiful state of preservation, though it bears marks of having seen some service too, as no doubt it should, like other *mortars* of the days of Lewis the Great.

WESTMINSTER MEDICAL SOCIETY.

Saturday, Nov. 12.

DR. STEWART IN THE CHAIR.

Cholera.

DR. JAMES JOHNSON announced that he had in his possession a letter of Dr. McWhirter's, of Newcastle, which he would beg of the Chairman to read.

The Chairman then read the letter, the substance of which was, that the writer and some other physicians at Sunderland, had had a special interview with Dr. Daun, and *extracted* from that gentleman the opinion, that there was *no proof* of the cholera having been imported into the town by infringement of the quarantine precautions. Dr. McWhirter also mentioned, that there were *two* nurses employed about the corpse of the patient who had died of cholera at the infirmary; that the one who sunk under the supposed infection had been greatly alarmed; the other one was not frightened, and escaped.

DR. J. JOHNSON resumed, by observing that these were important facts; and that, coupling them with the bulletin of the night, that but one more case had occurred since the last announcement, he could not help thinking that a *little* too much alarm had been excited. There were circumstances under which cholera might be considered as occasionally contagious; but if ever those circumstances did exist anywhere, it was at Sunderland, where the lower orders were continually drinking spirits, and the quarters where they resided were the filthiest that could be conceived. Saffron-hill and St. Giles's were palaces to them.

DR. SIGMOND was glad to find Dr. Johnson beginning to entertain right notions on the subject of contagion.

DR. J. JOHNSON.—"I was always an advocate for *contingent* contagion."

DR. SIGMOND could not desire a more

satisfactory avowal. The most decided contagionists held no more. The *contingency* existed in the predisposition, and some few other circumstances which were generally admitted.

MR. KING rose to announce himself a contagionist; but he considered *all* diseases to be contagious as well as the cholera morbus. The honourable member then entered into a profound argument to prove that morbid matter existed in the body of every person who was sick, and therefore might be communicated to somebody else. Certain other remarks which fell from the honourable member induced

MR. HUNT and MR. DOUCHEZ to give their opinions on the communicability of typhus fever, and specific diseases, by contagion.

MR. SEARLE being generally called for, rose, and expressed his willingness to communicate whatever he knew on the subject of cholera. He denied that any of the medical attendants on the sick, so far as he could know, and he had ample means of knowing (placed as he was at the head of one of the chief hospitals in Warsaw), ever suffered from the complaint. There was one apothecary's boy, indeed—a drunken, idle rascal—who fell a victim to it; but none of the physicians were attacked, and they were very numerous from all parts of Europe. It was not true that the members of the French medical commission were boys, as asserted in a certain periodical; they were mostly professors—and such indefatigable men he (Mr. S.) never saw. They performed a thousand experiments upon themselves; they inoculated their persons, tasted the excretions, &c.; they fed pigeons, dogs, and cats, upon the offal of the sick, inoculated them, &c., yet none of the consequences ever tended to prove that the disease was propagated by contact or infection. He dwelt considerably upon the fact of his own immunity, notwithstanding his perpetual and laborious attendance upon cholera patients, in all stages of the complaint; he had frequently been obliged to lie down on the hospital beds, debilitated with his labours—attending from an early hour in the morning until ten o'clock at night; nor could he always procure perfect ventilation and cleanliness: coming to the hospital sometimes unexpectedly, he found the windows close shut, and various things in the filthiest state. He spoke altogether from facts which

had fallen under his own personal cognizance; yet he would decline to announce himself either a contagionist or non-contagionist—though the evidence, in his mind, certainly weighed in favour of the latter side of the question.

DR. GRANVILLE, after expressing his satisfaction at Mr. Searle's communication, requested that gentleman to inform the Society further, what he knew relative to the battle which was said to have infected the Poles.

MR. SEARLE explained, that the Polish troops had previously made a forced march of twelve German leagues, or about fifty English miles; they captured a large body of Russians, certainly—but they encamped in a marsh, and the mortality, even on the first night, was above fifty. He thought that too much stress was laid upon the circumstance of the hardships of the Poles. Why, he would be glad to know, was it always stated that the Poles were infected by the Russians on these occasions, and not the Russians by the Poles? He had heard it jocularly observed by a distinguished person in Poland, that this complaint of the cholera was one of the best things that had ever happened his countrymen.

PROFESSOR BURNETT, though in common with the other members of the Society, he was much obliged to Mr. Searle for his kind communication, yet could not but confess himself unconvinced by that gentleman's remarks. He had been much more impressed, on the last night of meeting, by the able arguments of Dr. Macleod; who had shewn that, though many (perhaps the majority) of the individuals who had seen the disease in India, had in their reports subscribed themselves non-contagionists, yet the three great Boards had come to the opposite conclusion. Nor was the Bengal Board an exception; for though Mr. Jameson, the individual who drew up the report for that Board, professed himself a non-contagionist, yet, in the body of the report, he stated his opinion that, though the disease was not communicable from man to man, it was so from one body of men to another body of men! Then there was the disease travelling simultaneously northward by the Volga, and southward by the Don, and in the teeth of the winds; there was also the disease distinctly shut out of Teheran along with the caravan, and forced to go onward to a more distant

locality. The fact, too, of the village which had been one half ravaged by the disease, and the other half preserved by a barricade, was very convincing. The learned Professor then proceeded to expose the quibbling of the non-contagionists, who were now driven to the admission of a modified or *contingent* contagion. He illustrated the nature of predisposition and infection by reference to the growth of seeds; and, in conclusion, dwelt upon the advantages of precaution: *Melius est sapè cavere quam pati semel.*

DR. GRANVILLE rose to reply to some of the arguments of Professor Burnett, and complained of the treatment which his (Dr. G.'s) party received from the contagionists. He particularly alluded to the *tranchante* manner in which the Medical Gazette of that very day disposed of the non-contagionists and their opinions. The learned Doctor now drew from his pocket a newspaper containing the propositions of Drs. Russell and Barry, relative to the contagiousness of cholera, and endeavoured to shew that these gentlemen thought exactly on the question as he did!—(Surprise and laughter).

DR. GREGORY moved an adjournment, in consequence of the lateness of the hour, and wished to know whether it would not be rather better to exchange the question of the communicability of the disease for that of its nature and treatment?

DR. COPLAND was proceeding to make some remarks, when the meeting broke up, as we understood, with the intention of resuming the debate on Saturday next (this night).

The room, during the evening, was thronged with members and visitors: indeed we never recollect an attendance so full.

CLINICAL LECTURES,

Delivered in the Dispensary of the London University,

BY

DR. ANTHONY TODD THOMSON.

LECTURE V.

November 10, 1831.

Epilepsy—Chorea—Lumbago.

GENTLEMEN,—There are two cases of epilepsy at present on the books of this esta-

blishment. The one, that of a girl, about fourteen years of age, of a melancholic temperament, with black, straight hair, and sallow complexion, in whom the disease has existed for several years; and although the frequency of the attacks has been much diminished by the means which have been employed, yet there is little hope of the disease being ultimately cured. Indeed, from the physiognomy of the patient, there is every reason to fear that the result of its continuance will be a state of fatuity. Tracing the history of this case, it appears "that the disease may have originated from a blow upon the occiput, which the girl received six years ago, as the epileptic paroxysm displayed itself soon afterwards, and has continued ever since, with short intermissions. In this case, it is probable that the injury done to the brain may have produced a predisposition to the disease, but as the seat of the mischief is beyond our reach, it is not likely to be removed. The second case is that of Arthur Leny, a green-grocer, who was admitted on the 27th of October: a case to which I have already twice alluded. I have stated that the most singular circumstance in the attack of this case, in which the epileptic paroxysm first displayed itself twenty-three years ago, is the long period which intervened between the first attack and the second, which did not occur till seven years afterwards. Since that time, until within two years ago, the patient suffered generally two or three attacks every year, which, however, were of a slight nature; but two years since they became more frequent, and for a few weeks prior to his application at the Dispensary, he seldom escaped having three or four attacks during the twenty-four hours. The remedies which have been employed, and to which I shall afterwards direct your attention, have been so far successful as to check the frequent recurrence of the fits, none having taken place since the day after he began the use of them.

In all cases of Epilepsy, there appears to be a peculiar predisposition of the person's system—an augmented susceptibility of impression in the brain, which may be denominated the *Epileptic diathesis*. Various circumstances, therefore, which in the ordinary states of the habit, produce little or no effect upon the nervous system, excite it so strongly in those labouring under this diathesis, that convulsions, and spasmodic action of various kinds, may result. In some individuals, particularly in females, hysteria is the consequence; in others, ordinary convulsions may take place; while in others, Epilepsy is produced. The usual causes to which Epilepsy is ascribed are either injuries done to the head, preternatural growths of bone from the skull, or other mechanical causes of irritation in the head. It is not, however, necessary that the irritation be

confined to the brain: cases having occurred in which the disease has been traced to mechanical irritation on nerves out of the cranium. De Haen, for instance, has recorded a case in which it arose from a gangliform concretion adhering to the phrenic nerve. Dr. Short mentions one which was cured by the extraction of the irritant, a tumor, which pressed on the tibial nerve; and the Memoirs of the Medical Society of Geneva describe a fatal case, that evidently arose from an osseous body, about the size of a small nut, with a rough surface, which pressed on a branch of the sciatic nerve. Worms also, or other morbid irritants in the intestinal canal, acting by sympathy upon a highly-susceptible state of the brain, are exciting causes of Epilepsy. Sometimes, however, the exciting causes are of a mental kind, such as sudden grief, violent passions, or terror; or occasionally the attacks can be referred to the retrocession of disease; such, for example, as repelled gout, or cutaneous eruptions; and occasionally to uterine irritation, in which case they may be complicated with hysteria.

The variety of the disease to which I would refer the case under consideration, is that which is termed Cerebral Epilepsy; and I am disposed to regard it as the result, in this instance, of sudden gusts of passion—of a very fretful temper. I am induced to conclude that this is the fact, from strict inquiry into all the circumstances which might have induced this attack, and from having found nothing which would lead me to refer it to any mechanical irritation, or to repelled disease. I can only attribute it to some mental emotion; and the physiognomy of the individual indicates anything but a good-natured and calm disposition. The effect of mental causes in producing Epilepsy was known at a very early period, and obtained for the disease its Latin appellation—*Morbus Comitialis*—from the circumstance of its being by no means an unfrequent result of the high state of excitement produced by the harangues of the Roman orators, in their popular assemblies. By this man's account of himself, also, it appears that he has had much to fret him, and were it possible to trace it, we should probably find that the first paroxysm originated in a gust of boyish passion.

That the disease is Cerebral Epilepsy, is distinctly pointed out by some symptoms which seldom accompany any of the other varieties of Epilepsy: for instance, the constant winking, or nictitation, which still continues, although the fits are suspended; and the giddiness, which also still remains in the absence of the headache. During the fortnight that the paroxysms were of frequent daily occurrence, the patient complained of severe headache; an occasional loss of sight for some time after each paroxysm, and intolerance of the light of a candle or a fire.

These symptoms sufficiently point out the state of the cerebral organ, and with the predisposition which obviously exists, it is not difficult to comprehend that irregular nervous action and resulting convulsions would follow any mental emotion sufficiently violent, either to determine a sudden rush of blood to the head, or to produce sympathetic congestion in the brain.

We know that, in some states of apoplexy, depending on transitory fulness of vessels, epilepsy is not an unfrequent accompaniment; and in the direct ratio of the strength of the epileptic tendency, is much blood-letting unnecessary. Another reason for suspecting mental irritation, rather than permanent fulness of vessels, to have been the exciting cause in this instance, is inferred from the small frequent pulse and singing in the ears, which were present at the time the patient first applied for advice.

In none of the fits does he recollect having experienced any peculiar sensation which might be regarded as its harbinger; neither have the paroxysms kept any stated periods, or made their attack at any particular hour of the day.

In the treatment of the first case which I have mentioned, the only remedies that appeared to be productive of benefit were active purgatives and the introduction of an issue in the neck. The issue is still discharging, and when the bowels are freely open the recurrence of the paroxysms is comparatively distant. I am also inclined to attribute some advantage to the girl being kept in constant occupation, so as to divert, as much as possible, her mind from the contemplation of the disease. Before she applied to the Dispensary, the mother conceiving that her daughter was in a disease which required rest, allowed her to remain in a state of inactivity; but since she was informed that this was improper, and since the girl has been occupied in the ordinary duties of her station, the paroxysms have been less frequent and decidedly less severe. Previous to the introduction of the seton, all the usual means were employed without any obvious advantage. From the state of apathy in which she appeared to be, and from there being a manifest aura epileptica, dry cupping along the spine, stimulant embrocations, and powerful anti-spasmodics, both of a vegetable and mineral kind, were freely tried. It has generally been supposed, that we are to look for some local irritation in the part where the aura epileptica arises; but I have have had sufficient reason to think, that this symptom is as often an accompaniment of the disease, when it depends on general constitutional derangement, as on local irritation. My chief reason, however, for ordering the cupping along the spine, was an idea that there was disease existing there, as the limbs seemed often to give way, prior to the ap-

proach of the fit; and I knew, from the dissections of Mons. Esquirol, that affections of the spine, particularly softening of the cord, has been detected in post-mortem examinations of Epileptic cases. The hip-bath daily, and mercurial alteratives, were also employed for nearly two months, without any benefit; indeed, nothing seemed to check either the severity or frequency of the paroxysms except the purgatives, the seton, and the mental occupation.

In the treatment of the second case, although no permanent plethora manifestly existed, yet I considered it necessary to unload moderately the vessels of the head, by cupping behind the ears. Eight grains of Calomel were directed to be taken immediately afterwards, and to be followed by a brisk cathartic. On the 29th of October, two days after he had used these remedies, he had had no return of the epileptic fit; but from the feelings which he then described—the singing in the ears, the nictitation, and the sensation of wind being in the head—I was induced to continue the purgative system, not so much with a view to carry off any existing acrimony in the bowels themselves, as to set up in them a degree of counter-irritation, which might operate by lessening the irritability of the brain; and so far it appears to have been successful.

I would take this opportunity of remarking to you, gentlemen, that, not unfrequently, a course of cathartics alone has cured epilepsy; and although Dr. Hamilton, who revived the attention of the profession to the influence of Cathartics in Epilepsy, perhaps expected too much from their employment, yet the public owe him a debt of gratitude for expelling the prejudices against the employment of Cathartics, which prevailed for nearly a century before his time. You will find, in his work on purgatives, many cases illustrative of their powerful influence in the treatment both of this and several other nervous affections. The use of purgatives, however, in Epilepsy, is not of modern date; you will find it insisted upon by Celsus, who says—"Necessarium est, ducere alvum, vel nigro veratro purgare, vel utrumque facere, si vires patiuntur." The choice of the Cathartics in Epilepsy, is a matter of some importance: where we suspect the existence of mechanical irritation in the head, those which tend to unload the vessels of the system generally, and thereby to diminish the quantity of circulating fluid sent to the head, are the best adapted to fulfil the indication in view. Where worms are supposed to be present, as we generally find that they are either ascarides or tania, perhaps the rectified oil of Turpentine, in large doses, should be preferred; and when, as in the present instance, our object is not so much to unload the bowels and improve the secretions as to produce a re-vel-

lent effect, or to determine from the head, I prefer very moderate doses of Calomel in combination with Camphor and Henbane, given two or three times a-day, and, in the intervals, small doses of the common Senna mixture, with Wine of Colchicum and Camphor Julep. My object in prescribing the Calomel and Colchicum is to favour the free excretion of bile into the duodenum, so as to afford a general stimulus to the whole intestinal canal; whilst, at the same time, the Camphor and Henbane may moderate the general irritability of the nervous system. The Henbane has one advantage over all other narcotics—it does not confine the bowels; and there is no other which I would prefer, unless where the epilepsy has followed a repelled eruption; in which case, I should be disposed to employ the extract of Belladonna, from its known property of producing a scarlet efflorescence on the skin, owing to its peculiar action on the cutaneous capillaries. It is, however, but candour to say, that as I have never seen Epilepsy proceeding from such a cause, I have had no experience of the effects of Belladonna; and therefore my idea of its proving beneficial must be regarded as conjecture.

As no fits had returned on the 5th of November, and although the headache had completely disappeared, there was still giddiness and constant winking of the eyes, I was induced to order, in addition to his former medicines, moderate doses of the arsenical solution.

In no disease has a greater variety of tonics been tried than in Epilepsy; each has had its day, and the greater number have sunk into merited neglect. I may safely assure you, that you will derive no benefit whatsoever from the use of any of the vegetable tonics. On the contrary, many of them, in particular Bark and the salts of its alkaloïds, appear rather to have done mischief. Indeed, in all cases in which the object of prescribing a tonic has been to allay irritability, and to diminish the spasmodic tendency, vegetable tonics have been found inefficient. Yet they are still used; and it is melancholy to observe that a credulity, worthy of the worst periods of superstition, continues to display itself in modern times, and that medical practitioners can be found weak enough to place confidence in the Mistletoe and similar remedies. Almost all the metallic tonics have proved more or less beneficial; amongst these the preparations of Zinc, Copper, and Silver, have been most generally approved. As far, however, as my own experience authorizes me to judge, I do not place much confidence either in the Oxide or the Sulphate of Zinc, although in the use of both the doses have been carried to their highest extent. I have given six and eight grains of the Oxide, and fifteen of the Sulphate of Zinc, three times in the twenty-four hours,

for many successive days, without any very perceptible advantage. As to the Salts of Copper, I have always regarded them with a suspicious eye, on account of their poisonous qualities, and therefore probably may not be able to give a fair opinion concerning them, as I have never carried either the Sulphate of Copper, or the Ammoniated Copper, beyond a very moderate dose.

Soon after Dr. Power made his experiments with the Nitrate of Silver in St. Bartholomew's Hospital, I was induced by his success to try that salt in two cases of Epilepsy; in one the dose was carried to the extent of six grains three times a day; in the other to eight grains; and to its use the disease yielded. But I confess that I have been prevented from employing it since by the dread of the known effect which it has in colouring permanently the skin; and I am of opinion the more effectual it is in small doses, the more likely is this discolouration to be produced. I can scarcely believe it possible, that any stomach can bear even six grains of the Nitrate of Silver for a dose, unless a considerable quantity of free Muriatic Acid, or of the Murates, be present in it. Now, if this be the case, the greater part, or the whole, of the Nitrate, will necessarily be converted into an inert Muriate; and therefore only that which remains undecomposed can operate as an effective tonic. On the contrary, if a small dose produce a beneficial effect, there is reason for supposing that it has entered into the system; and, under such circumstances, we can have no certainty that it may not be decomposed in the rete mucosum, and permanently retained there as a Chloride, affording, from the action of light, and that of Sulphuretted Hydrogen, not unfrequently exhaled from the skin, the dark colour which it has unfortunately too often produced. I hold an opinion, that such an effect is likely always to take place, when the rete mucosum actually exists in the European; for it is there that colouring matters of an insoluble character, even when introduced mechanically under the skin, remain unabsorbed for life; and as the Chloride of Silver is insoluble in the animal juices, it will necessarily follow the same law when formed in that organ. How far the administration of diluted Nitric Acid, at the same time that the Nitrate of Silver is given, might prevent such an event, experience has not enabled me to determine. These being the reasons which have prevented me from prescribing some of the metallic salts, I prefer the Arsenical solution for two obvious reasons. In the first place, its influence in allaying irregular nervous action has been well established, in the treatment of intermittent fevers; secondly, because it has effected decidedly beneficial results in the Epileptic attacks of the delicate, and in worn out habits; in the drunken Epileptic when ple-

thorax is not present, and in many other spasmodic diseases not attended by plethoræ. It is not so likely to interfere with the purgatives as the Salts of Zinc or of Copper, and it does not accumulate in the habit, nor has it the disadvantage of colouring the skin, which may attend the administration of Nitrate of Silver. I do not wish you, however, to suppose that Arsenic more than any other metallic salt, is applicable to every case of Epilepsy; such an idea would mislead you. Much depends on the circumstances attending the case, and the constitution of the patient. In the present instance the spare habit of this man, and the absence of plethoræ, seem to indicate its use, even although the fits observed no regular periods. Our patient has already benefited by its use, and he will, I have no doubt, obtain a permanent cure.

You may, perhaps, gentlemen, be surprised that I have not noticed any of the direct Antispasmodics in mentioning the general treatment of Epilepsy; but I have never seen the smallest benefit accrue from the administration of any of them, except Musk, which is too expensive a remedy for a Dispensary. In one very severe and unmanageable case, of several years' continuance, which came under my care in private practice, I gave Musk, in gradually increased doses, to the extent of half a drachm, three times a day, with most decided benefit. The fits, which for several years had amounted to two or three daily, became gradually less frequent and less severe, until, the full dose of musk being given, and continued for some time, the fits were suspended for five months, and they might not, probably, have returned again had the patient lived, but he soon afterwards died of typhus fever.

As in the treatment of every other disease, Diet and Regimen require to be attended to in that of Epilepsy; the former should be moderate in quantity, not more than the stomach can fully master, of a nutritious quality, but light: all fermented liquors should be avoided. Exercise ought to be regular, but not carried to fatigue; and the mind and spirits should be maintained as far as possible in a cheerful and elastic state.

Chorea.

Since I last lectured, another case of nervous affection, *Chorea*, has been admitted. The patient, Elizabeth Brady, aged seven years, with light hair, and of a sanguine temperament, tall, and spare in her habit, was in perfect health until about a fortnight ago, when immediately after her return from school, the disease displayed itself. She complained that she could not sit still; there was constant jactitation of the arms and legs, and uninterrupted movement of the fingers, a rolling of the head, and twitchings of the

muscles of the face, which continued even when she was in a recumbent position. She has scarcely slept at nights, not more than half an hour at a stretch; then generally awakes with a start, and the movements of the limbs and head immediately commence. She walks with great difficulty, the limbs twisting in various directions, so that she requires to be held up to prevent her from falling. Her speech is a little impaired, and her eyes have a very peculiar, rolling character. The bowels are open; the urine is high coloured; the pulse small, feeble, and quick; and the tongue slightly furred. The alvine evacuations are of a dirty, unnatural colour; and the perspiration has a peculiar offensive odour.

My first object in this case was to remove all acrimonious matter from the bowels, and to improve the character of the secretions. My second to restore the tone of the habit, and thereby overcome the spasmodic tendency. With these views, the patient was ordered a powder, containing five grains of Calomel, and twelve of Jalap, to be taken immediately, and repeated at the distance of eight hours, for three times successively; after which, she was directed to take ten grains of the Carbonate of Iron, and three of the powder of Conium, every third hour during the day; to observe a light diet; and to have friction applied along the spine and over the abdomen, as she was incapable of taking exercise. As the girl has not made her appearance again, I must reserve my remarks upon the case for our next lecture.

Rheumatism—Lumbago.

The cases of Smith and Sparrow have been discharged cured, and as West has not again paid us a visit, I have every reason to believe that he also is well. One case of severe Lumbago has been admitted since these cases, and the pain still continues with little abatement. The patient is an old man, with a broken-up constitution and of irregular habits; he has been cupped across the loins twice, blistered, and put under a course of purgatives, with Calomel, Colchicum, and Opium, and, latterly, he has taken also a Terebinthinate mixture, with large doses of the Tinctura Conii. The pains, however, continue. Although he has been strictly enjoined to observe regularity in diet, yet, I am apprehensive, that these orders are not obeyed, and to this circumstance, in part, may be ascribed the little benefit which treatment has produced in this case. It is one of many instances in which the influence of medicines, otherwise most effective, is completely counteracted by the abuse of dietetic stimulants. Such is also the case with the woman Eadwell, to whose case I formerly alluded as an illustration of that mixed form of rheumatism and gout, com-

mon'y called rheumatic gout As long as this lady remains sober, she improves; but as habit has confirmed her irregular tendencies, and her hours of sobriety are, "like angel visits, few and far between," I have little hope of being able to effect a cure in her case.

SANITARY HINTS RESPECTING CHOLERA.

Council Office, Whitehall, 14th Nov. 1831.

Sir,
THE Central Board of Health having maturely weighed all the information which has been transmitted to them relative to the progress of the Asiatic Spasmodic Cholera in various parts of Europe, but more particularly guided by the conclusions on this head, to which Drs. Russell and Barry have arrived after a five months' careful and laborious observation of the character of that disease in those parts of Russia which they have visited, beg leave to suggest for your consideration the following sanitary hints:—

I.—As to Precautionary Measures.

In order to ensure the adoption and realize the benefit of any system of sanitary arrangements in a large community, the first essential point is to divide that community into subordinate sections, and to form District Boards of Health, each to consist, if possible, of a resident clergyman and a number of substantial householders, and of one medical man at least.

These Boards should be charged with the following duties in their respective districts, viz.:

1st, To appoint inspectors. Each inspector to visit daily, and to inquire carefully after the health, means of subsistence, cleanliness, and comfort, of the inmates of, say, (100 houses, more or less,) according to local circumstances.

2dly, To receive and examine the reports of these inspectors, which should be made up to a given hour on each day.

3dly, To endeavour to remedy, by every means which individual and public charitable exertion can supply, such deficiency as may be found to exist in their respective districts in the following primary elements of public health, viz. the food of the poor, clothing, bedding, ventilation, space, cleanliness, outlets for domestic filth, habits of temperance, prevention of panic.

4thly, To report to their principal Boards respectively on the above heads, as well as on the actual state of health of their districts.

The subordinate divisions of each district ought to be numbered or lettered, and each

district named, the names of the members of each Board, or of the medical men attached to each, and of the visiting inspectors employed, should be placarded in conspicuous places.

Principal Boards of cities, towns, or parishes, to report directly to the Central Board in London:—

1st, On the actual state of health of their whole population.

2dly, On the precautionary measures already carried into effect.

3dly, On the measures contemplated.

4thly, On suspected sources, if any there be, from whence this particular disease might possibly spring.

With regard to precautions as to intercourse with suspected or really infected persons or places, the Board are confident that good sense and good feeling will not only point out, but morally establish, as far as may be practicable, the necessity of avoiding such communication as may endanger the lives of thousands.

But they strongly deprecate all measures of coercion for this purpose, which, when tried upon the Continent, invariably have been productive of evil. The best inducements to a prompt acknowledgment of the disease having entered a family, as well as to an early and voluntary separation of the sick from the healthy, will always be found in the readiness and efficiency with which public charitable institutions attend to the objects noticed in S. 3.

It is with much satisfaction that the Board feel themselves authorized to declare, and it will no doubt be highly consolatory to the public to learn, that under proper observances of cleanliness and ventilation, this disease seldom spreads in families, and rarely passes to those about the sick, under such favourable circumstances, unless they happen to be particularly predisposed.

It will not therefore be necessary, where there is space, and where due attention is paid to cleanliness and purity of air, to separate members of families actually affected by the disease, nor to insulate individual houses, unless in cases of crowded, filthy, badly-ventilated habitations, and other contingencies, which involve the health and safety of all.

It having been proved, by ample experience, in more than one city in Europe, that the fitting up and furnishing of hospitals for the reception of the poorer classes supposed likely to be attacked by the disease, at a period too long before its actual breaking out, has been productive of great waste of means, by the spoiling of various articles, and the consequent want of wholesome accommodation when most required, the Central Board would recommend that proper and sufficient house room only be secured

and prepared in the first instance, and that the charitable be called upon only to *pledge* themselves to furnish at a given notice such articles of bedding, furniture, &c. or the value of them, as they would have at once contributed.

By this means the deterioration of perishable articles will be avoided, and should the district entirely escape, the contribution will be saved.

The situation which the Board would recommend for temporary Cholera hospitals would be, those most detached, insulated, and thoroughly exposed to free and open air; the description of house, such as would admit of the most perfect ventilation and cleanliness, and the largest space around the sick.

The Board would recommend, when a family is reported to be in an unhealthy state by the sub-inspector, and the disease confirmed to be Cholera by a medical member of the District Board, that the head of such family, if unable to afford proper accommodation at home, be advised to send the sick person forthwith to the temporary hospital, and that the other members of the family be supplied with such additional means and comforts as their state may require to enable them to resist the influence of the infected atmosphere in which they live.

II.—Medical and Dietetic Precautions.

These will be found of considerable importance, from their contributing to prevent or diminish the susceptibility to infection which individuals may possess at the moment the disease breaks out.

No sudden nor extensive alterations should be made in the usual modes of living. All changes of food, to be useful, indeed not to be absolutely prejudicial, should tend to render it drier, more nutritive and concentrated; moderately costive bowels, the almost invariable consequence of a dry, invigorating diet, will be found more conducive to exemption from cholera than an opposite habit.

Whenever aperients may become indispensable, those of a warm aromatic kind in moderate doses, or domestic means, should alone be resorted to.

What is generally understood by salts, viz. Glauber's salts and Epsom salts, as well as other cold purgatives, should not be taken in any quantities, nor on any account without the express prescription of a medical man.

The medical members of the Board beg to state, in the most decided manner, that no specific preventative against cholera is known to exist, and that the drugs hitherto offered with this pretension, in countries where the greatest ravages have been caused by this disease, not only did not possess the negative virtue of doing no harm, but were found to be absolutely injurious.

The true preventatives are a healthy body,

and a cheerful, unruffled mind. Looseness of bowels should be immediately checked, and any thing like periodical chills or cold perspirations should be met by quinine in suitable doses; but habitual drugging, at all times *improper*, is to be deprecated in the strongest terms, when epidemic disease is apprehended.

The Board has been anxious to lay before the public, as early as possible, the above precautionary outlines, which they trust will tend, together with the suggestions emanating from the wisdom and observation of your and other local Boards, if not to exempt the whole population of these realms from the scourge of spasmodic cholera, at least to enable them to meet it, in the event of its appearing amongst them, with physical and moral constitutions the least likely to suffer from its virulence.

The Central Board will avail themselves of the earliest opportunity to transmit to you any further sanitary suggestions which may occur to them on the subject of precautionary measures, as well as an outline of instructions now in preparation for communities supposed to be actually attacked.

I have the honour to be, Sir,

Your most obedient servant,

E. STEWART, Chairman.

DRS. RUSSELL AND BARRY ON CHOLERA.

THE above named gentlemen, when at Calais, made a memorandum of the conclusions at which they had arrived, on a mature consideration of all they had witnessed abroad relating to Cholera. A copy of this has appeared in various newspapers, but containing several very important mistakes: the source from which we have received that which follows leaves no room to doubt its correctness.

“ After having meditated on the above facts and documents from the moment they came to our knowledge—after having weighed them with all the attention of which our minds are capable, and after having compared the opinions which each of us separately, and without discussion, had grounded upon them, we find our impressions as to the mode of origin and spread of the late epidemic at St. Petersburg and its neighbourhood, so perfectly identical in all important particulars, that we now agree to and sign the following propositions, containing the heads of our unanimous opinions on this part of the business of our mission:—

“ 1. That the germs of the disease were brought to St. Petersburg by the boats and barks which arrived from the interior this year, previously to the 14th (26) of June.

“ 2. That those germs were diffused and the disease propagated in two ways; one

which may be called personal, by the dispersion over the whole city immediately after their arrival, of several thousand passengers and boatmen, who had come from infected places, or had been exposed to infection on the passage or on board these vessels. The other, which may be termed atmospheric, by emanations from the barks, and their contents suspended in and carried by currents of air to susceptible persons, independently of direct communication.

" 3. That the germs of the same disease were carried to Cronstadt, and propagated there by boats and lighters which had been loaded directly from the barks already mentioned, by persons who had recent communication with these barks, or had been in their immediate neighbourhood.

" 4. That the disease was introduced into all the villages round St. Petersburg, in which we have been able to obtain authentic intelligence of its progress, by persons directly from the city, or from other infected places.

" 5. That neither the near approach nor the immediate contact of an infected individual were indispensable to the infection of a healthy individual susceptible of the disease at the moment.

" 6. That the epidemic of St. Petersburg did not possess those absolute and indiscriminating communicable qualities attached to the plague and small-pox, and that the risk of infection incurred by the healthy who approached the sick, was in direct proportion to the want of cleanliness, ventilation, and space, around the latter.

" 7. That in a generally infected atmosphere the additional danger of infection incurred by approaching one or more individuals labouring under this disease was not greater than would accrue from approaching one or more typhus patients under similar circumstances.

" 8. That under favourable circumstances of body and mind, personal seclusion did afford protection against the disease, more particularly if that seclusion had been accompanied by shelter from currents of air passing through sources of infection.

" 9. That those continued exempt from the disease who retired from and avoided communication with infected places; and those who resided to windward of, and those who were protected from the currents of air passing through such places; that the next in point of immunity were those who, though living in the midst of general infection, avoided large accumulations of sick placed in confined atmospheres, the young, the vigorous, those who could afford to live well, yet lived temperately. In short, those who were placed under circumstances the most favourable to health, cheerfulness, and comfort of every kind.

" W. RUSSELL, M.D.

" D. BARRY, M.D."

DEBATE ON CHOLERA.

IN consequence of the very crowded state of the room in Sackville-Street, during the two last nights of meeting of the Westminster Medical Society, the Committee have summoned the members to meet, on Saturday next, in the Great Windmill-Street Museum; when the discussion on cholera will be resumed.

BOOKS RECEIVED FOR REVIEW.

An Essay on the Epidemic Cholera of India. By Reginald Orton, Surgeon, H. P. late of his Majesty's 34th Regiment of Foot. Second Edition, with a Supplement.

Observations on the Nature and Treatment of the Cholera Morbus, now prevailing epidemically in St. Petersburg. By George William Lefevre, M.D.

A Translation of the Eight Books of Aul. Corn. Celsus on Medicine. Second Edition, carefully revised and improved. By G. F. Collier, M.D. of the Academy of Leyden; of Magdalen Hall, Oxford; and of the Royal College of Physicians of London.

A. Cornelii Celsi de Re Medica Libri Octo. Ex recensione Leon. Targæ. Accedunt J. Rhodii, Dissertatio de Celsi Vita, Schillingii Questio de Celsi Ætate, L. Targæ Præfatio et Index Libb. MSS. Editorumque, excerpta de Balneis, de Ponderibus et Mensuris Romanis Monitum breve, cum Conspectu Capitum. Præfixis Characterum, Balnearum, Instrumentorumque Tabulis. Editio Secunda accuratissime emendata opera et studio Georgii Frederici Collier, ex aula Magd. Oxon., e Colleg. Reg. Medic. Lond., et ex Acad. Lugd. Bat. M.D.

ERRATA

In Dr. Elliotson's Lecture, published last week.

P. 169, line 2, after *body* insert *often*.

P. 170, line 23, for *false*, read *fallacious*; same page, line 31, for *minutia*, read *minuteness*.

P. 171, col. 2, line 1, for *celer*, read *celeris*; line 3, same column, for *celer*, read *celer*.

NOTICES.

We have received a copy of the *real* opening lecture delivered at the College of Surgeons in Ireland. We are astonished. We thank our correspondent, but need scarcely tell him that the thing is utterly unpublisable.

Dr. Elliotson's Lectures.—The arrangement by which the accuracy of these Lectures is secured, renders it impossible for us to comply with the request of several "Students" to insert more than one Lecture in each number.

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LECTURES
ON
THE THEORY AND PRACTICE OF
MEDICINE ;

Delivered at the London University,

By DR. ELLIOTSON.

PART I.—LECTURE VIII.

Inflammation(continued.)—Pain—Morbid Appearances—Divisions—Common Inflammation—Symptoms.

IN the last lecture, gentlemen, I commenced the consideration of special pathology—the consideration of particular diseases ; and I stated that I should first speak of those diseases which attack various parts of the body,—that I should then consider such diseases, as seated in particular parts, and consider also those diseases which do not attack various parts, but are confined each to peculiar situations.

I therefore began the consideration of general diseases, if I may so speak ; and, as the most important, the most common, and the most connected with all other affections, I commenced with inflammation. I stated that inflammation was characterized, generally speaking, by four principal signs—redness, swelling, heat, and pain. I considered, as far as appeared necessary, the three first of these ; and, at the conclusion of the lecture, I was speaking of the fourth—namely,

Pain.

I stated that pain, like two of the other symptoms of inflammation, might arise from other causes than inflammation—that is, swelling and heat may exist independently of inflammation, and so may pain. Pain frequently arises from spasm, from scirrhus and cancer, or from neuralgia ; in the latter of which it depends upon a peculiar state of

the nerves, not necessarily inflammatory. But I stated that pain was a very common attendant upon inflammation, though not invariably present.

The pain of inflammation is generally increased by pressure, at least by sudden pressure, whereas pain arising from spasm is generally relieved by pressure. You have this very strikingly shewn in colic and in enteritis. In the latter, the pain, of course, arises from inflammation ; if you press the abdomen, even with the point of one finger, the patient experiences exquisite pain ; whereas, in the former, which is a mere spasm of the intestines, the more you press the more is the patient relieved ; so that I have found a patient able to bear the pressure of both hands, with my feet almost raised from the ground, standing on tiptoe, and the more I pressed the greater was the relief : but the two may be connected, and then you have a certain portion of relief and a certain portion of aggravation.

However, it is necessary to say, that although the pain of inflammation is increased by pressure, it is rather by this being *sudden* than gradually increased ; for frequently, if you press an inflamed part very slightly, and gradually augment the amount, very considerable pressure can be borne, provided you press the *whole* part. If you press only one spot, even slowly, you are sure to give pain ; but if you can embrace the whole part, and press it all gradually, so as to empty it of its blood, you will frequently produce ease. This is shewn every day in the case of inflammation at the bottom of the foot. If you have a blister upon the sole of the foot, or at the ball of the great toe, and you rest gradually upon the part, the pain becomes mitigated, till at last it seems to be almost entirely removed ; but the moment you take off the pressure and raise the foot from the ground, you feel the part begin to throb—to throb with violent pain. It appears, therefore, that if an inflamed part be pressed suddenly it is sure to have its pain increased, if it be pressed even gradually, but only par-

tially, you generally produce the same effect, because, if you press only one part, the other becomes more gorged with blood; but if you can press the whole together, so as gradually to empty all the vessels, or diminish their contents, then pressure can be borne, and, more than that, it frequently gives relief.

Hence pressure has been recommended as a means of cure, by certain writers, in some inflammatory diseases. Pressure has been recommended for the cure of gout and rheumatism, by Dr. Balfour, of Edinburgh; and a French surgeon (Velpeau) has recommended it in erysipelas. But it frequently happens that, although relief may be given in some cases, considerable aggravation is excited in others. It is necessary that the part should be all compressed—that the pressure should be equable; for any deviation in this respect, so that a part of the vessels were allowed to become the more distended, would increase the mischief. Very frequently, too, inflammation does not depend altogether upon local causes; there is a tendency to it in the constitution, and although you impede the circulation of the part, you do not stop the impetus of blood running to it; the large arteries around are found to throb violently against the part compressed, and pressure frequently cannot be borne at all. On all these accounts, it is exceedingly difficult to cure inflammation by pressure. As a general rule, however, we may say that the pain of spasm is diminished by pressure, and the pain of inflammation increased by it; and this criterion is continually resorted to as a means of diagnosis—as a means of distinguishing whether pain is inflammatory or spasmodic.

Pain is, generally speaking, greater in proportion to the tension of the part attacked. The less a part will yield, the greater generally is the pain. Hence much inflammation of a theca of a tendon is generally attended with great agony, and from a similar circumstance, when matter is formed, if it cannot escape, from the part not yielding at all, the pain is most agonizing; whereas, by relieving the tension in making an incision, so as to allow the escape of even the smallest quantity of matter, and the yielding of the part, the pain instantly disappears; and more than that, the great irritation of the system, which, perhaps, amounted to a violent delirium, also vanishes. All these things frequently cease, as it were, by magic, when an incision is made. Hence, too, the great use of incisions in certain inflammations of the skin and the cellular membrane. In what is called *phlegmonous erysipelas*, the cellular membrane becomes excessively gorged with blood—excessively gorged with fluids—the skin is distended, and the greatest agony is felt; but a few incisions will put a stop to all this. I presume it is for the same reason, that, in inflammation of

a serous membrane, you have far more violent pain than in inflammation of a mucous membrane. These serous membranes in general are tense, and give the most violent pain when they are inflamed; whereas the mucous membranes are all of a yielding character, and inflammation of them is consequently never attended by a violent stabbing pain. You know that serous membranes are generally spread out pretty tightly, at least a considerable portion of them, and therefore you generally have a violent stabbing pain when they are inflamed; but sometimes this is not the case, and then the less degree of pain may arise from the affection being seated in a portion that will yield.

But pain, according to its situation in various parts, may be sharp, dull, smarting, or burning. Sometimes it is a mere soreness—no pain is felt unless the part is irritated mechanically, or some particular stimuli are applied. Parts which have little or no sensibility in health, acquire sensibility under inflammation, and frequently very intense sensibility. Many parts which may be cut in health without the individual, be he man or brute, experiencing any sensation, when inflamed cannot bear the slightest motion or the slightest pressure. If, therefore, parts which show no sensibility in health become very sensible in inflammation, you may suppose that parts which are naturally sensible in health may become very painful when in a state of inflammation. However, the degree of pain will not be commensurate with the degree of original sensibility, because it depends so much likewise upon the tension or the yielding character of the part. If it so happen that a part which has no sensibility, or but little, but which is nevertheless an unyielding part, become inflamed, the pain will be dreadful; more dreadful than it would be in a part which naturally has sensibility and yet is of a yielding character.

Diagnosis.—Inflammation is generally known by pain increased on pressure, by feverishness, and a disturbance of the function of the part. When we see a person labouring under disordered function, and labouring under feverishness—pyrexia—and under pain, which pain is increased by pressure, although we cannot see the part, we have no doubt that it is inflamed—that the proximate cause of these symptoms is inflammation. Sometimes there is no pain experienced, but simply disturbance of function and feverishness; and when that is the case, we are not sure that there is an internal inflammation. When a part can be seen, the symptoms which I have already mentioned, few or more of them, will be sufficient to characterize the complaint; but we are certain also of the existence of inflammation when the part is invisible, when we cannot ascertain whether it is red, hot, or swollen or not; but where

there is pain increased on pressure, the function of the part disturbed, and the patient labouring under what is called "feverishness, or pyrexia."

Morbid Appearances.

These are the four symptoms of inflammation during life, but they may have existed at that period and none of them remain after death. The pain and the heat, of course, must go, but the redness and swelling you might expect to remain. When, however, we have seen a part inflamed during life, or such symptoms have been present as to leave no doubt of the existence of inflammation, in both cases you may find, on examination after death, no marks of inflammation at all; and yet the absence of these does not in any way invalidate the opinion which you formed during life; for it will happen that after death every mark of inflammation will disappear. You may sometimes, in inflammation of the surface of the body, or of the eyes, visible enough during life, and of which the patient died, find both parts pale after death; the skin, perhaps, will be slightly swelled, but the swelling will be greatly diminished, and the redness entirely gone. This is a certain fact respecting inflammation in general, because it is observed with regard to external inflammation. After apoplexy you will frequently find nothing. Although during life the head appears to be bursting from an accumulation of blood, and the patient has died of the complaint, yet you will frequently find after death no morbid appearances. All the vessels, when the temperature of the body cools, recover themselves, or the blood in some way or other goes to other parts; but this occurrence does not always take place. You will sometimes find that where there was no doubt from the symptoms that internal inflammation existed, the marks of the complaint disappear after death; sometimes they do, but it is by no means general.

However, on the other hand, you must recollect that it is not every preternatural redness that you discover after death which is inflammatory. You may find an intense redness of the internal parts in the bodies of patients, whom during life you did not suspect to be labouring under inflammation, and you are not on that account *alone* to suppose that they did; for just as the redness of inflammation will frequently disappear after death, either in a great degree or altogether, so after death, without any previous inflammation, you may have morbid redness of the parts. If the patient have died with great difficulty of breathing, so that a large accumulation of blood has taken place in the lungs, you may find the liver gorged with blood, and you may find the mucous membrane of the stomach and intestines exceedingly red, not from inflammation, but simply because the blood

was obstructed in the lungs, or in the heart, and consequently congestion took place. In a case of great debility, a continuance of the patient in one posture during the latter period of his existence will cause such an accumulation of blood as, after death, to give rise to great redness; and these appearances are always greater in proportion to the number of capillary vessels in the part to which the blood inclines by its gravitation. The blood will not merely accumulate in the vessels in this way, but it will transude from them so as to dye the surrounding parts perfectly red, and prevent your distinguishing the appearance from inflammation. This is particularly the case in hot climates and in hot weather. Dr. Davy, the brother of Sir Humphry, says, in a paper published in the tenth volume of the *Medico-Chirurgical Transactions*, that he has found it in hot countries often impossible to distinguish the redness of inflammation from the redness of transudation, or the redness from the circumstance of a piece of membrane being merely steeped in blood; and from all his observations and experiments he draws the conclusion, that if in a hot climate a body be not opened within twenty-four hours after death, the surfaces become red; that the serum of the cavities likewise becomes bloody, and the viscera livid, so that no opinion can be formed as to what was the state of the parts during life. The reason is this—the hotter the temperature within certain limits, the sooner does decomposition take place; the more a part is decomposed, of course the more easily does transudation occur. The solids become more soft, more spongy, and blood which comes in contact with them easily pervades them, oozes through them, and dyes them and any of the liquids which the parts may happen to contain.

You notice in dead bodies which have lain any time upon their back, especially in hot weather, that the posterior parts all become livid, and filled with blood, while the anterior parts are pale. The body being placed usually on the back, if you open it and examine the lungs, you find their posterior part heavy, filled with blood, livid—in fact exhibiting exactly the same appearance as the anterior part does in violent bronchitis; and every one accustomed to make post-mortem examinations allows for this occurrence; he never thinks of considering it a morbid appearance; it is simply the effect of the blood gravitating to the lowest parts. You will observe also when bodies are at all decomposed, that red streaks appear along the surface, in the direction of the various veins. The blood accumulates in all the veins after death, and as the substance of the coats becomes more and more decomposed, it allows the blood contained within the veins to transude, and in that way the coats themselves become thoroughly dyed with blood,

and the cellular membrane around the vessels becomes dyed likewise; you may trace the superficial veins in the dead body from this appearance. This is nothing more than a transudation of the blood, through a degree of decomposition having occurred.

The observations made by Dr. Davy many years ago, respecting the importance of examining bodies early in hot weather, and shewing the importance of knowing that such appearances as are induced by inflammation may take place merely from decomposition, have been all lately confirmed by Andral, who, to make his observations the more striking, has examined the same part of the body at different periods. Early after death he has found an organ pale; but if it were afterwards so placed as to favour the gravitation of the blood to a certain portion of it, that part of the organ, when subsequently examined in a few days, decomposition having proceeded, had become intensely red.

The blood will not only accumulate in the solids in this manner, and not only exude in sufficient quantity to dye any liquids that may be within the cavities, but it will itself exude to such an extent, that a canal may be found containing a large amount of pure blood. By causing a considerable portion of intestine to be dependent, the blood has gravitated to such an extent as to pass through the vessel into the cavity, and there lie in the form of a hemorrhage. Two French writers have made numerous experiments upon this subject, Rigot and Trousseau; and they say that they have frequently made the blood ooze from the inner surface of the intestines, by merely giving a portion of them this dependent position.

You must therefore be aware, that the mere appearance of redness is not sufficient to justify you in saying that a part has been inflamed. A part may be red because it was inflamed during life, but you must always take into consideration, whether the body has lain long, so as to be partly decomposed, and allow the diffusion of blood into the substance of the parts. Indeed, a degree of putrefaction not merely gives this infiltration of blood into the parts, but the parts themselves become dissolved into a liquid substance—they liquify. As the body putrifies it becomes soft, and a large quantity of liquid is produced, in addition to the pure blood, which assists materially in dyeing the different parts. You have therefore to take into consideration what is the period at which you examine the body, and also what is the position of the part, whether it is such as is likely to have favoured the gravitation of its own blood, and that of other parts, in any quantity. You have also to take into consideration another circumstance, independent of time and of position—that is, whether any mechanical obstruction

existed during life. You may examine a body immediately after death, so that putrefaction has not occurred, and placed upon its back with the stomach high, so that the blood could not have gravitated there, and you might see such redness of that organ that you would consider it inflammatory, if you did not also know or ascertain that there had been a mechanical obstruction. If there have been a mechanical obstruction to the return of the blood through the lungs and heart, then that will explain redness of the stomach without your being under the necessity of referring it to inflammation. There are therefore three circumstances to be taken into account—the period at which you examine the body, the situation of the part as to gravitation, and the previous existence or non-existence of mechanical obstruction. A simple inspection of the parts which are red, provided there is nothing but redness, will give you no information whatever. You must ascertain whether there were any symptoms of inflammation during life, and observe the three circumstances to which I have just alluded.

There is, however, another circumstance which will enable you frequently to say that inflammation actually existed, and that is the presence of the products of inflammation. If you find a part intensely red, and you see a layer of lymph effused, or you see a large quantity of serum—perhaps turbid serum—perhaps, too, with flakes, then you may be sure almost that the redness is inflammatory. The redness, it is true, may be increased by decomposition—may be increased by mechanical circumstances; but if you see the products of inflammation, then you may fairly conclude that the redness, either altogether, or the greater part of it, is inflammatory. These observations you will find of use with respect to inflammation in the inner surface of the heart. The lining membrane of the cavity of the heart, especially at the valves, is sometimes very red without any inflammation. Some persons will say there was inflammation during life; others will say that there was not. Of course the point generally may be cleared up by minute observation—by feeling whether the heart is soft or not, so as to allow the membrane to be stained by transuded blood; but if you see an effusion of lymph upon the membrane, then you may be sure that the redness is of an inflammatory character—that it is the result of inflammation, either in part or altogether.

These are the observations that I have thought it necessary to make, gentlemen, respecting the marks of inflammation during life and after death.

Divisions.

Inflammation is usually divided into two kinds—the one *common*, the other *spe-*

cific. "Common" inflammation is that which we see every day where a local injury takes place, or when a part has been exposed to the vicissitudes of temperature; but the epithet, "specific," is given to inflammation when it runs a peculiar course, or when it arises from a peculiar cause, neither mechanical, nor simply irritative. When inflammation runs a peculiar course, it is then by many called "specific," but more frequently it receives that appellation only when it arises from a peculiar cause. For example, inflammation of the urethra, from the forcible introduction of a bougie, or a catheter, would be common inflammation; but the inflammation of gonorrhœa, arising from a peculiar cause, is termed specific inflammation. In specific inflammation you have all the symptoms which occur in common inflammation, but some peculiarity is superadded. Common inflammation is the groundwork, but in specific inflammation some other circumstance is superadded.

Common Inflammation.

I will now speak of the course of common inflammation. Common inflammation may begin with pain, with a blush, and with a feeling of unusual warmth. When an inflammation is first set up, it may begin in this way, but the redness is sometimes not in the form of a blush, but occurs in one particular point, and from that it spreads. If the part be a secreting organ, its secretion will become changed in its appearance—perhaps changed, likewise, in its smell, and at the same time it is generally increased in its quantity. The pain and the redness increase in degree, and also in extent, and then a swelling occurs, or tension, or swelling and tension. The parts swell to a certain point; and if they cannot swell farther without difficulty, they become tense. As the inflammation proceeds, the secretion of the part generally diminishes again, though perhaps it does not come down to, or does not descend below, its natural amount; but the secretions still remain unhealthy in appearance, and, perhaps, also in smell. As all this goes on, or when it arrives at a certain point, the constitution begins to feel its effects. The pulse becomes quickened; and it likewise becomes variously altered in its volume, firmness, strength, and regularity. Thus the constitution sympathizes at last. The affection of the pulse is not always according to the danger; for in mere inflammation of the tonsils, or in mere rheumatism of the joints, both of which are unattended by danger, you will sometimes find the pulse as rapid and as violent, or perhaps more so, than in many dangerous inflammations. The same remark may be made with regard to the temperature. The temperature of the whole body will sometimes be greatly elevated, although the inflammation may be one of no

danger. In a case of mere acute lumbago, from which a man very speedily recovered without any bleeding—merely by the exhibition of colchicum, I found the pulse at 160, and the heat of his whole body 107°. I find, in a note which I made, that he required no blood letting at all, and that he merely took vinum colchici for three or four days, yet that his pulse was 160; and by the thermometer employed by myself, and placed in various parts of the body—under the tongue, and in the axilla, the temperature was not less than 107°. The heat, too, frequently varies in different parts of the body. Besides the alteration of the pulse and the temperature, the patient becomes restless, uneasy in any position, and complains of general soreness; the whole surface of the body frequently becomes sore—the patient feels tender, he says, all over. The head frequently becomes painful, and likewise the loins; and sometimes the pain in these two parts are among the severest constitutional symptoms. The constitutional derangement extends to the secretions, so that most of them become diminished and altered, and from this circumstance you will have, in the first place, thirst. The tongue will be dry or white; and sometimes, if there be much gastric disturbance, it is yellow, or of little strength, dark. The skin becomes dry, though in one particular inflammation—active rheumatism—it generally falls into profuse sweating. The urine becomes scanty; and though it remains clear it is high coloured, and has a strong animal smell—it smells something like gravy soup. The bowels become torpid, and the faces unhealthy, both in colour and smell. These are all instances of diminished or altered secretion.

Now these constitutional symptoms altogether are called by Cullen and his followers *pyrexia*—not fever, but *pyrexia*—fever being the name given by them to certain distinct specific diseases. Some persons denominate these symptoms fever, under all circumstances. We have no English word for this term, *pyrexia*, except we use *feverishness*, but *ish* implies only a slight degree of any thing; whereas the *pyrexia* may be very great, and therefore the term "*pyrexia*" is always better. The word is used by Cullen to distinguish this constitutional excitement under all circumstances, from those specific diseases, which he calls, whether remittent, intermittent, or continued, *fever*; to distinguish it from fever properly so called. However, some persons, aware of the confusion which might arise from calling both these symptoms "*fever*," and other specific fevers, such as typhus, also *fever*, denominate these symptoms when they are dependent upon inflammation, or other local cause—*symptomatic*; and they call the others—fevers, properly so termed—*idiopathic fevers*—fevers

dependent solely upon themselves—grounded by themselves. You will, therefore, find these symptoms, which are sometimes called “constitutional excitement,” “constitutional derangement,” denominated by some writers, “symptomatic fever,” to distinguish them from *real* and specific fever, which they call “idiopathic;” but you find in Cullen the word fever not given to this state in general, but that he uses the word “pyrexia.” For the purposes of clearness, it can make no difference whether, in the case of inflammation, we say pyrexia, or symptomatic fever; for all that is necessary is, that we should make ourselves understood.

Symptoms.

Now it very frequently happens, that when the local disease first begins, before it has increased to such an amount as to quicken the pulse, and induce the other symptoms which I have mentioned, the person complains of chilliness, a general soreness, and even rigors, so that he shivers with cold; the pulse at this time being small, the face pale, and the skin rough—from its deprivation of blood, and its low temperature, it is rough, and is called *goose's skin*—*cutis anserina*, or this state is called *horripilatio*. After the existence of these symptoms—this cold stage, for a certain time—the inflammation appears, and the excitement of the whole constitution gradually begins. Sometimes, therefore, inflammation begins, and after a certain period is attended with constitutional excitement; but sometimes, the moment inflammation begins, the system falls into a cold stage, like fever—exactly such as happens in the cold stage of an intermittent, so that the local inflammation appears simultaneously with the constitutional symptoms.

The general symptoms are usually proportionate to the intensity and the extent of the local inflammation, and likewise to the importance and sympathies of the organ affected; and sometimes, as I have already mentioned, the general symptoms are out of all proportion in their violence, so that if you depended upon them, and did not ascertain the state of the part affected, and recollect its nature, you might suppose the patient was in the greatest danger, when that was not the case. If you found an individual with a temperature of 107° , and a pulse of 160, you would suppose him in imminent peril, if you did not ascertain that the inflammation was inconsiderable, or was seated in a part not at all necessary to life—not at all of importance to the system; whereas, the same derangement attending inflammation of an important organ, or attending a very extensive inflammation of another organ even not so important to life, would point out the greatest danger. You see, therefore, from all I have stated relative to the colour of parts after death, and

the excitement of the system in inflammation, a proof that what I before remarked is well founded, that we are not to depend upon one symptom—one circumstance—in a case, generally speaking, but we must always take into view as many particulars as we can ascertain.

Besides these local and these general symptoms, the functions of the part inflamed are disturbed. This I have already mentioned so far as the secreting organs are affected, but other organs are likewise disturbed. If it be the brain that is inflamed, you have delirium. If it be the lungs that are affected, you have dyspnoea. If it be the stomach, you have vomiting. If it be the intestines, you have either constipation or diarrhoea. If it be the bladder, you of course have a frequent and painful desire to make water.

Buffy Coat.—It is not, however, merely these disturbances—these changes—that take place. The blood itself becomes changed in inflammation. The red particles separate so fully from the fibrin, that this fibrin generally remains colourless and transparent at the top, and, when this occurs, it is said to have a *buffy coat*. There must some change take place in the blood for this phenomenon to occur; and, as soon as you remove a portion of blood from the body, you find that some alteration has taken place, for such are the effects. Occasionally the fibrin, white and clear above from the red particles below, is drawn into such an excavated form, its centre so depressed, that it resembles a cup, and such blood is called *cupped*. Blood may be buffy without being cupped; the red particles may separate entirely from the fibrin, and leave a buffy coat; but sometimes, in addition to this, the fibrin is drawn into an excavated form, so that it is buffed as well as cupped.

Occasionally, this buffy appearance of the blood takes place, it is said, without inflammation. It is usual in pregnancy, but then really a pregnant woman is in an inflammatory state; it is not a state of actual inflammation, but the condition of the womb is really inflammatory. You see that the whole body frequently in pregnancy wastes; the eyes become hollow, and the whole health, for the most part, suffers a little. The pulse becomes very quick—quicker than in health, and all the blood-vessels of the womb become greatly enlarged. The womb itself, if it be not in a state which justifies us in calling it inflammatory, is, nevertheless, in a state of such great activity, that it comes as near to inflammation as possible. And, in truth, with regard to other periods of generation, when animals are in heat, and copulation takes place, the genital organs become violently red, violently hot, perhaps blacken with blood, as if they would burst, and all the blood-vessels around throb. If that state be not to be called inflammation, still it is

very clear that it is all but so, and whatever word you choose to employ, you must look at it as physiologists and pathologists. We cannot therefore wonder, that, in pregnancy, the blood should frequently be buffy.

But this state of the blood is sometimes absent in inflammation. You will find a patient undoubtedly labouring under a most violent inflammation, and on using the remedies for that affection, you produce the most decided effects, and cure him, and yet no buff may be present in the blood. This is by no means uncommon in bronchitis, where a patient has a rapid pulse, great heat of body, cannot turn in bed without coughing, suffers under a difficulty of breathing, so that he is obliged to breathe in a shallow manner; cannot expand the lungs without extreme soreness, tightness, and extreme cough; the tongue is white, and violent pyrexia is present; and if you bleed, you produce instant relief to the patient, so that both the symptoms and the treatment prove the existence of inflammation. The blood will not in bronchitis in many, *many* cases, show the slightest morbid appearance; whereas, if it were another membrane that was inflamed, instead of the lining membrane of the air tubes, as, for instance, the pleura, the smallest quantity of blood that you could take away would most likely show an intensity of buff, and be cupped in addition. The absence of this buffiness of the blood is by no means a proof that there is not inflammation; but, if it be present, it is generally in proportion to the violence of the inflammation. If you be in doubt as to whether the patient is labouring under inflammation or not, the appearance of this buffiness might justify you in concluding, that the symptoms which you suspected to be inflammatory really were so. The absence of it is no proof at all that inflammation does not exist; but the presence of it will, in doubtful cases, very much confirm your conclusions, that the symptoms you were treating were inflammatory. The buffiness, however, is not necessarily in proportion to the *danger* of the inflammation, because in acute rheumatism, where there is no danger whatever—sometimes there is danger in acute rheumatism from internal inflammation, but in a case where there is no danger whatever—the blood will generally be intensely buffed, and not unfrequently intensely cupped at the same time. You are, therefore, usually to consider this as a mark of inflammation, or an inflammatory state; but its absence will not warrant you in saying that there is no inflammation; and the intensity of the buffy or cupped state is not to be considered by you as a proof that the inflammation is of an exceedingly dangerous character*.

ON THE
CEREALIAN ORIGIN
OF
CHOLERA.

By Dr. HAYCRAFT.

It seems rather unaccountable, that amidst the disputes concerning the causes of cholera, that the influence of a vitiated diet, in producing the disease, has not of late years been discussed. Contagion or some peculiar state of the air, or both of them combined, have been assigned as the only probable causes.

That improper diet has been a predisposing cause of cholera in this country as well as in Germany, and also of the epidemic cholera of India, has been allowed by Sydenham, Richter, Sprengel, Rankeen, and many others; and that it usually begins at, or soon after, the harvest period in each country respectively, is also stated by most of these writers. This, added to the consideration that cholera, in its various forms, is essentially a disease affecting the stomach and bowels, and always begins with gastric and splachnic symptoms, would lead us to suppose, that whatever the exciting causes may be, the predisposing cause might consist in the use of food made from corn in its too recent state, or otherwise unfit for the human constitution.

Dr. Rankeen, who was with the army on the Bengal station, in 1818, (see Edin. Med. and Sur. Journ. Jan. 1823,) ascribed the epidemic of the time “chiefly to a diet of rice or other grain vitiated by the wet of the season:” to this Dr. Mason Good, I think without foundation, objects, “that the grain and other food of such wet seasons is usually the product of the year before, which may have been peculiarly dry and healthy.”—(See Study of Med. vol. i. p. 281.) From inquiries I have made of gentlemen well qualified to give an opinion, I find that this objection will not hold good, at least in this country, and probably not in any other. I am informed, that as early as the month of August it is the custom in London to use one part of recent to two parts of old wheat*, and that in some parts of

* In Dr. Elliotson's lecture published in our No. for Nov. 12, p. 173, col. 2, l. 62, for “medicine,” read “médecine.”

* Unless old is mixed with the new wheat, the bread will not “rise,” as it is called, occasioned, I suppose, from the panary fermentation being imperfect.

the country the bread is occasionally made entirely of the most recent wheat, namely, that which a week or ten days previously had been standing in the field. Now this occurs in the month of August, which Sydenham* affirms, and which every body knows, is the month in which cholera in this country is peculiarly rife.

Sprengel† and Richter‡ both accuse a bad quality of diet as a cause of cholera; the latter states, that it prevails in Germany from harvest till August and September, and also that the epidemic cholera of children, in North America, is most frequently about the harvest months.

The Russian cholera of 1829 also made its first appearance in the month of August, or about the harvest period, and according to the statements of Dr. Onufriev, physician for the circle of Orenburg, it appears that during the prevalence of the epidemic, there was scarcely a single inhabitant who had not some symptom of disordered digestion. This was ascribed by some of the physicians to a peculiar epidemic state of the human constitution; others ascribed it to an unusual abundance of fruits, especially of gourds and water melons; but we have too little information on the subject to determine if these universally prevailing symptoms of indigestion were occasioned by some general cause, independent of the state of the air, namely, a vitiated diet. That improper diet, or some general depravity of the usual articles of food, was in fault, is probable; for, we are informed by Dr. Hawkins, in his history of the Epidemic Spasmodic Cholera of Russia, and also by others, that the disease has made its principal ravages among the poorer inhabitants, who "obtain a scanty, and often improper nourishment;" and the Warsaw Committee of Health reports, "that few persons of easy condition have been ill, and that the disease has expended itself chiefly on the poorer inhabitants of that low and thickly peopled city." Indeed, almost all the writers on cholera who have observed the disease, have stated that it attacks chiefly the poorer classes, and those that are badly fed.

The epidemic of 1830 first appeared in June at Rescht, lat. $37^{\circ} 18' N.$ a town in Ghilan, in Persia, situated on the

southern coast of the Caspian Sea: it then appeared at Baku, lat. $40^{\circ} 2' N.$ early in July, and on the 27th July was observed at Teflis, lat. $43^{\circ} 20'$; and about the same time, namely, the 19th July, appeared at Astracan, lat. $46^{\circ} 22' N.$ From Astracan it arrived at Tgaritzza, lat. $48^{\circ} N.$ on the 4th August. On the 6th of the same month it appeared at Saratof, lat. $52^{\circ} 4' N.$; and at Penza, still more to the north, it appeared on the 17th August. "About the end of August" it reached as far north as Nisen or Nischenei-Novogorod, lat. $56^{\circ} 34' N.$; and from this place it branched off in two directions, namely, to the east and by south to Casan, lat. $55^{\circ} 43' 58''$, where it appeared on the 9th September, and also to the west and by south to Moscow, lat. $55^{\circ} 45'$, where it broke out about the same time as at Casan, namely, "about the middle of September."

The reader, if he examine the foregoing paragraph, which I have condensed chiefly from the review of Dr. Lichtenstadt's *Die Asiatische Cholera in Russland*, contained in the *Edin. Med. and Sur. Journ.* July 1831, adding the latitude to each place, will perceive that the date of its first appearance at each place corresponds with the harvest period, which, for the most part, agrees with each respective latitude, with one or two slight exceptions, which may be explained from local circumstances: so that, for example, Casan and Moscow, lying almost exactly in the same latitude, began to be affected almost at the same time, although nearly five hundred miles apart.

The epidemic of 1830 did not cease as did that of the previous year, "but continued to spread with little intermission" westwards, and arrived at Warsaw in the beginning of May. But as it now appears to have assumed a contagious character—on which, however, there are doubts—we should not expect its progress to be distinctly marked by the harvest periods of the different places which it visited. What I would now remark is, that the common diet of the lower classes in the countries which have suffered from the disease, is chiefly bread from rye corn—a species of grain which has always been accused of being more subject than any other to vitiation, both from diseases to which it is liable, such as blight, ergot, &c. as well as to a foreign

* See Opera Universa, p. 177.

† Handbuch der Path. § 123, 127.

‡ Spec. Therap. 4. Band, s. 124.

admixture of injurious seed, such as from *Raphanus raphanistrum*, the *Agrostina*, the *Centaurea cyanus*, the *Nigella sativa*, and the *Lolium temulentum*, all of which the cultivation of rye peculiarly favours. In Georgia, as well as in most parts of European Russia, in the northern parts of Prussia, Poland, and even in Hamburgh, I am informed that the common food of the poor is from rye: often the bread, or rather cakes, are made from grain uncleaned, ground with the husk, imperfectly fermented, and half baked, and in this state kept for use. In Persia, from whence the disease is supposed to be traced, the usual food is rice, the supply of which, we are informed, owing to the improvidence of the people, is seldom sufficient for the supply till the following harvest, so that the inhabitants are obliged to use the new crops in their most recent state. Is it not, therefore, worth inquiring if the propagation of the disease in question is not occasioned, or at least favoured, by an improper quality of diet, especially from *corn bread*, arising from the corn being used in too recent a state, or from a vitiation to which rye is more subject than other grain?

It may, perhaps, assist our inquiries if we turn our attention for a few moments to the epidemic which, at different periods, from the year 1577 to 1771 was so fatal in Silesia, Voightland, Lower Saxony, and other parts in Germany, as well as in Sweden, Switzerland, and in France, between the Cher and the Loire. This disease has, by all writers on the subject, been ascribed to the causes we have mentioned, namely, to an improper quality of bread corn, especially of rye, which was the common food of these countries: it also presents so many resemblances to the epidemic now prevailing in Europe, that a comparison between them will not be uninteresting to the reader. I have, therefore, copied *verbatim* Drs. Barry and Russel's graphic description of the cholera as it occurred at St. Petersburg, and have condensed Richter's description of the epidemic spoken of, which he designates by the name of "Die acute oder fieberhafte Kriebelkrankheit (*raphania* of Mason Good), as contained in his *Specielle Therapie*, vol. vii. p. 607. I have placed each description in alternate paragraphs, that the comparison may be made with greater ease.

Dr. Barry states that "Diarrhœa, at first feculent, with slight cramps in the legs, nausea, pain or heat about the stomach, malaise, give the strongest warning."

According to Richter, the warnings (*vorbothen*) consist in a loss of appetite, furred tongue, pain in the region of the stomach, numbness or slight trembling of the limbs, formication.

"When violent vertigo, sick stomach, nervous agitation, intermittent, slow, and small pulse, cramps beginning at the tip of the fingers and toes, and rapidly approaching the trunk, give the first warnings, there is scarcely an interval."

There is vertigo (*schwindel*), obscure pain in the occiput; pulse small, quick, and intermitting; clonic, alternating with tonic, spasms in some parts.

"Vomiting, purging, or both of these evacuations, of a liquid like rice water or whey, come on."

Vomiting of sometimes nearly black bile. (This dark-coloured vomiting is sometimes observed in the Indian cholera.)

"The features become sharp and contracted, the eye sinks, the look is expressive of terror, and wildness, and as it were, a consciousness on the part of the sufferer that the hand of death is upon him."

The appearance is pale, sunken, and hippocratic; the eyes roll in their sockets, which gives the patient an horrible appearance; he squints, and the pupils are contracted.

"The lips, the face, the hands, the feet, and soon the thighs, arms, and whole surface assume a leaden, blue, purple, black, or deep brown tint, according to the complexion of the individual, varying in shade according to the intensity of the attack. The fingers and toes are reduced at least a third in thickness; the skin, and soft parts covering them, are wrinkled, shrivelled, and folded; the nails put on a blueish pearl white; the larger superficial veins are marked by dark lines, of a deeper black; the pulse is either small as a thread, and scarcely vibrating, or else totally extinct; the skin is deadly cold, and often damp; the tongue is always moist, often white and loaded, but flabby and chilled, like a piece of dead flesh."

The pulse is small, very quick, spasmodic (*krampfhaft*), frequently remitting. The patient has a cold fit (*frost*),

the skin is either rough or dry, or breaks out in a cold clammy sweat, the tongue is always discoloured, and finally almost black. (N.B. In the Indian cholera, the tongue has been observed to have a dark fur towards the end.)

"The voice is nearly gone; the respiration is quick, irregular, and imperfectly performed. Inspiration appears to be effected by an immense effort of the chest, whilst the *aknasi* (in the most hopeless cases, and towards the close), instead of expanding, collapse and stop the ingress of air. Expiration is quick and convulsive. The patient only asks for water, speaks in a plaintive whisper (the *vox cholericæ*), and only by a word at a time, from not being able to retain air in his chest for a sentence."

Respiration is difficult, and performed with great effort, (*heftigen anstrengungen*;) there is, as Mason Good expresses it, "a sense of suffocation, and a difficulty of articulating distinctly," with an unquenchable thirst.

"He tosses incessantly from side to side, and complains of intolerable weight and anguish around the heart. He struggles for breath, and often lays his hand on his stomach and chest, to point out the seat of his agony. The integuments of his belly are sometimes raised into high and irregular folds, whilst the belly itself is violently drawn in, and the diaphragm upwards and inwards towards the chest. Sometimes there are tetanic spasms of the legs, thighs, and loins, but we have not seen tetanus, or even trismus."

The extremities are alternately drawn up and extended, to which is conjoined an irresistible inclination of the patient to restrain these spasmodic movements, in which he succeeds very imperfectly. There is a burning sensation in the internal parts; then follows grinding of the teeth, and even trismus, by which the tongue is not seldom injured.

"There is occasionally a low suffering whine. The secretion of urine is always totally suspended; nor have we observed tears shed under these circumstances. Vomiting and purging, which are far from being the most important or dangerous symptoms, and which in a very great number of cases of the present epidemic have not been profuse, generally cease, or are arrested. . . . Towards the close of the scene the respiration becomes slow; there is a qui-

vering among the tendons of the wrists; the mind remains entire. . . . After the cold blue period has lasted from twelve to twenty-four hours, or upwards, the pulse and external heat begin gradually to return. . . . A profuse critical perspiration comes on from the second to the third day, and leaves the sufferer convalescent; but much more frequently the quickness of pulse and heat of the skin continue; the tongue becomes brown and parched, the eyes are suffused and drowsy; there is a dull flush, with stupor and heaviness, about the countenance, much resembling typhus; dark sordes collect round the lips and teeth; sometimes the patient is pale, squalid, and low, with the pulse and heat below the natural standard,—but with the typhus stupor, delirium supervenes, and death takes place from the fourth to the eighth day, or even later," &c.

Towards the close of the hot stage of the disease the patient falls into a mild delirium, mutters between his teeth, is sad and low, is indifferent to surrounding objects, and neither hears nor sees correctly; he shews all the common symptoms of typhus, *cum torpore, nervosa, stupida*. After some days, colliquative and putrid symptoms set in; meteorismus and emphysematous enlargements, petechiæ, ecchymosis, abscesses and real carbuncles on the extremities, quickly going into mortification. The fingers and toes are first discoloured, and then gangrene; the local mortification soon spreads universally; death happens commonly about the seventh day of the disease.

The reader will perceive two remarkable differences in these diseases, namely, the sense of formication in the extremities at the beginning, and the tendency to mortification of the extremities towards the termination of the disease, which are not observed in cholera. It appears, however, from the authority of Sprengel, Wichmann, and Taube, that these two symptoms belong rather to the chronic raphania; Dr. Mason Good has therefore omitted them in his short sketch of the disease in question, and has with propriety referred the tendency to mortification of the extremities to the chronic disease, under the name of *gangrena urtilaginea*, or mildew mortification, a disease universally ascribed to the same cause as the one before us.

There are many points of resemblance between the two diseases, which we have not yet noticed, namely, the season of the year in which raphania made its appearance—about the harvest period; it also chiefly attacked the poor and ill-fed; it was also remarked that in raphania the violence of the spasms did not indicate the greatest danger; also the most successful treatment was similar, such as large doses of calomel (Taube), antispasmodics, and externally the warm bath, spirituous and stimulating liniments, &c. The post obituary symptoms were also similar, as dark spots on the stomach and intestines, distended with air; the abdominal vessels were found distended with much black carbonized blood (mit vielem schwartzen verkohlten Blute angefüllt). This disease has, notwithstanding its acknowledged cerealian origin, also been supposed to become contagious.

The absence of the congec evacuations, and the presence of bilious vomiting, would induce us to consider the disease as akin to bilious cholera; and the violence of the spasmodic symptoms, among which even trismus has been observed, would give it a nearer resemblance to the spasmodic cholera; the typhoid symptoms, however, would seem to bring it into a nearer relation to the Russian cholera. The chief difference between this disease and the various species of cholera seems to be a less tendency to collapse in its beginning, by which the cold or blue period is less strongly marked; there is likewise a less strikingly observable reaction in the system after the cold stage; also a typhoid character is more observable throughout its whole course.

The chief use I would make of this comparison between the acute raphania, or, as it might be called, the Silesian cholera (from Silesia, where it has been most frequently epidemic), and the other species is, that as that disease has been universally allowed to have had a cerealian origin, it would strengthen our suspicion that the other species may have the same cause.

That diet has some influence in the propagation of epidemics has never been denied; that unwholesome diet prepared from corn in an improper state, especially from rye, which is, as before remarked, more subject than other grain to become unfit for food, has been a frequent cause of fatal epidemics, has

been generally believed. I suspect we may trace somewhat of the acknowledged injurious effect of this kind of diet in modifying the usual bilious cholera of Germany, which, as described by Sprengel, Richter, and other writers, is a much severer disease than we are commonly acquainted with in England, being frequently accompanied by convulsions, and inflammations tending to gangrene, of the stomach and bowels—symptoms that have long since been ascribed to the use of rye food. It may be remarked, likewise, that cholera both in India and Persia, countries whose inhabitants do not cultivate rye, has existed for many years, without exhibiting typhoid symptoms, or being usually supposed to be contagious; yet, as soon as it visited Russia and other rye countries, it puts on a typhoid form, and people begin to believe it to be contagious. I believe also in such countries diseases in general, and especially fevers, do not bear bleeding so well as in this country, and are more apt to run into a typhoid form.

On the whole I am inclined to think, although direct matter-of-fact proofs are not sufficiently numerous to make it certain, that the continental cholera has for its chief predisposing and modifying cause the use of rye as the common diet, and perhaps of that and other grain in a vitiated state; or in other words, that the peculiar epidemic constitution of the human system, by which it becomes predisposed to the Russian cholera, is the said use of rye, immature, or otherwise vitiated.

Should this view be correct, it would be a consolatory reflection that in this country, at least, we shall be exempt from this scourge of the human race, or should it visit us, it will be disarmed of half its terrors.

Even in those instances which have lately been reported, where individuals of the higher ranks, and in whom a vitiated diet may not apparently be accused with justice, yet have received the disease, as is supposed through contagion, such cases, should they be fully proved, would only shew that other causes beside diet would also modify or determine the character of the disease in those individuals: they would not in the least invalidate the idea that the general character of the epidemic has been influenced or determined by the nature of the diet.

I think Dr. Prout's doctrines of *mer-organization* will, when more fully developed, throw much light upon the subject before us: for example, that grain, in its too recent state, may be supposed not to be so perfectly *merorganized* as to be perfectly fit for food. If we consider the *merorganizing* power as something distinct from the organizing, plastic, or formative principle of Sir Gilbert Blane—which latter builds up, as it were, the structure both in vegetable and animal life—it will then be understood to be that which forms or modifies the materials which the organizing power uses in its operations. Merorganization may, then, be considered as a chemico-physiological exposition of digestion, chylication, sanguification, assimilation, secretion, &c.; in all of which, merorganization, we suppose, forms an essential part. Yet, it not only contributes to organization as a preparatory process, but it must also depend upon the latter, because, without organization it could not have place. In grain, when first ripe, the organization, we suppose, is complete; yet we have reason to consider that the greater part of its substance, which is designed for the food of the future plant, as still undergoing the merorganizing process. If we regard the perfection of the panary fermentation as a test of the grain being sufficiently aged to be fit for food, or, in other words, that its *merorganization* is perfect, it would appear that this process requires several months for its performance; for I am informed that the new grain is not fit to be used by itself till about Christmas, until which time it is usual, in London, to mix a considerable proportion of old grain before sending it to the mill*. If grain, then, is used in its too recent state, we suppose that, its merorganization being incomplete, too great a tax is laid on the merorganizing power of the stomach, and probably of the whole system; and this will, if other causes conspire, produce diseases of the stomach and its collatitious viscera; of which diseases cholera is one.

I have also been informed, by good

authority, that the late harvest in this country has not been productive of grain of so good a quality as is commonly believed. Mildew had been unusually prevalent—a disease of corn in which it is observed that a number of grains in each ear are small, and its organization imperfectly developed: how far this may have contributed to the unusual prevalence of cholera after our late harvest, I leave the reader to judge.

The following deductions, then, may be made; but, as we have not all the facts before us necessary for their demonstration, I will merely set them down in the form of queries.

1st. Is it not probable that grain, in its too recent state, or in an otherwise vitiated condition, is a common predisposing cause of cholera?

2dly. That as rye corn is more subject to such vitiation than other grain, and has always been accused of producing disease characterized by its resemblance to Russian cholera; and as the sporadic and epidemic cholera of rye countries have these characters; is it not probable that the use of vitiated rye bread is the predisposing cause of that disease?

It might also form a proper subject for inquiry, how far the various salts used in the manufacture of bread (which salts have been considered as merorganizing substances) may be useful to the human constitution, in preventing a predisposition to cholera; and this inquiry may, perhaps, be made in connexion with the interesting facts mentioned by Dr. Stevens, as well as those communicated through Drs. Barry and Russell, relative to the use of such substances in the treatment of the Russian cholera.

TRANSFUSION OF BLOOD IN CHOLERA, &c.

*To the Editor of the London Medical
Gazette.*

SIR,

As the subject of cholera is becoming daily of more fearful interest, I am sure you will concur with me in the opinion that every suggestion, however remote the benefit of its practical application, demands publicity at the present moment.

* Riechter informs us that the government of the day recommended the inhabitants of the districts most subject to the epidemic, as a prophylactic measure, not to use the rye or barley too new; and also, when thrashed, carefully to separate all other seed, and even to wash and dry the corn in an oven, before sending to the mill, &c.

I offer no further apology for the following hasty observations; and, sir,

I remain, yours, &c.

T. W. W. SMART,

M.R.C.S., &c.

Cranborne, Nov. 14, 1831.

In the first place, I wish to be included within the number of those who object to the appellation of this pestilential disease, which, having been assigned to it on the assumption of a false analogy, is calculated to misdirect us into the paths of mischief and error. Before we may establish the identity of any two diseases, it is essential that the comparison made should be completely accordant in all its parts, neither exclusive of one particular symptom on the one side, nor giving the preference to another on the other side. The history of both must accurately agree. If a partial view alone be taken, we are continually liable to be misled; for how many differing diseases are there not, which manifest symptoms common to all of the same class? For instance, the acute eruptive diseases—in all of them the prefatory febrile affections are alike, and even when the cutaneous inflammation first appears, a mistake is very likely to be made by an unpractised eye, though the more skilful never would confound the early stage of scarlatina with measles, or that of measles with small-pox; for he would apply his power of observation to discover the peculiar diagnostic mark which each wears, in addition to its common resemblance to the other. So is it with English and Asiatic *Cholera*—as it is called. They may have some symptoms in common, but there exist others sufficiently strong to determine a wide distinction between them. Vomiting and purging, the very characteristics of our native disease, are by no means the invariable or most prominent symptoms of the other; spasms, so constant in the Asiatic, are of unfrequent occurrence in *ours*. Then the fatality of the one compared with the almost innocuousness of the other; its decidedly contagious nature; besides, those symptoms of universal paralyzation, that sudden abolition of all physical energy, present to my mind phenomena with which English cholera bears no parallel, not even in degree. It sometimes happens in this country, that

patients under this complaint, (females particularly,) seem very depressed, low, and faint, but not more so, nor so much, as they do often under an attack of hysteria. It frequently happens too, that in some of its severest forms the symptoms of nervous depression are scarcely developed at all. I had an opportunity of remarking this the other day, in one of the most acute cases of cholera I had ever witnessed. It began as usual by rigors, febrile action, pain, vomiting, purging, and the matter voided was of a yeasty pultaceous quality; this was the first stage. The next was ushered in by spasms of the legs, thighs—in fact of the whole frame—accompanied by the most torturing pain; this stage lasted eight hours. Still there was no exhaustion, no debility beyond what one would reasonably expect to ensue after copious evacuation; for I promoted the object of nature to expel the contents of a loaded, stuffed alimentary canal, by the use of purgatives, and they brought away quantities of black, horribly offensive feces. The man was a free liver, a great eater, and he had experienced for two or three days previous to the attack an increasing distention of the abdomen; but when the bowels had been cleared, there was no more distention, no more pain, no more spasm, no more *cholera*,—and this was effected in twenty-four hours; in twenty-four hours more he was well, to his astonishment, considering the previous acute suffering, but still complained of muscular tenderness. This was a specimen—a strong specimen—of English cholera. I might have prescribed the fashionable nostrum, cajeput, in its dose of 40 drops, or preferably to that the oil of turpentine, as it unites the antispasmodic virtue of the former with its own purgative quality; and these remedies might have succeeded *à merveille*; for my idea is, that the cause of the disease is not the presence of acrid bile (*χολη πικρα*), but a spasmodic action of the muscular fibres of the alimentary canal, depending probably on nervous irritation, brought on by the distention, or the excitement produced by an excess or an impurity of the food. But are we to expect success from these, or similar remedies, in the Asiatic pestilence, now threatening to devastate our country—that deathblow of our nature, as it were—that corrupter of the fountain and springs of life? I am incredulous.

lons of the advantage to be derived from their use, as well as from other popular modes of treatment. What, may I ask, are venesection and calomel expected to effect? means, the best adapted, in my opinion, to finally extinguish the still flickering flame of life. When I spoke of cholera as a "corrupter," I meant to allude to the changes which take place in the blood in that disease. This is a fact known to foreign pathologists; it is also known by Clanny and Stevens, that changes, perhaps similar changes, take place in the blood of typhus patients. Hence, there is some reason for assimilating the two diseases, as well as on the additional ground that, after the violence of the attack of cholera is past, fever of a typhoid character is apt to ensue. If the restoration of the healthy properties of the blood be so important a point in the treatment of typhus, as the researches of those gentlemen would affirm, and if this can be effected by the internal administration of carbonic acid gas or the neutral salts, I have long thought, why should not these substances be directly conveyed into the current of circulation by injection? Again, another point of resemblance between the cholera and typhus is seen in that stage of prostration of vital power, which, again, bears some analogy to the prostration resulting from excessive hæmorrhage. I would therefore propose, not only to try the injection of the above remedies into the veins, but also the transfusion of pure blood. The object of treatment seems to me to be that of exciting the vital powers, in fact, of simply keeping the body alive; and would not the substitution of a few ounces of pure blood, in the room of an equal quantity of vitiated fluid, be likely to prove a powerful, as it is the natural, stimulus of the heart's action? I leave these suggestions with your readers, determined myself to act upon them, should circumstances call for my interference. With regard to other plans of treatment, I would mainly rely on brandy and opium, stimulating frictions to the body, stimulating injections, and the hot sand-bath, as it will afford the same advantages as the hot water bath, without incurring the necessity of removing the patient from bed. Steam may be similarly applied.

Note on the late Influenza.

THE influenza visited the part of the country where I reside, in the middle of August, and continued till the latter end of September.

I attended more than 100 cases, but these were not all that occurred.

It attacked persons of all ages, both sexes, of all constitutions, and in all situations. The old, and those weakened by previous diseases, particularly by chronic coughs, felt it most.

It began with the usual symptoms of slow fever, which, in some old people, assumed the typhoid type, and reduced them very low. Then cough, of a spasmodic character, sometimes like whooping-cough, attended with mucous secretion, not affecting the respiratory sound, at least in those cases I subjected to the stethoscope; coryza, epiphora, sore throat; but, whenever it affected the power of deglutition, very little inflammation visible; aphonia, in some cases.

Duration of cases in general about a fortnight.

Only two fatal cases, and those indirectly so. The one, a young girl, who had suffered with spasmodic asthma seven years; the other, an old man, a cripple, with a chronic cough.

In every case, in which my attention to the point after a while was directed, I found tenderness of the spine, chiefly of the upper dorsal and cervical vertebrae, so much, in some instances, that very slight pressure could not be endured; and the intensity of this local affection was in direct ratio with the violence of the general disease. I found antimony, with Dover's powder, the best medicines.

Blisters to the spine relieved the pain in the head and the cough; tartarized antimony ointment irritated.

I consider that the disease was more spasmodic than inflammatory, resulting probably from spinal and other nervous irritation.

T. W. W. SMART.

GALVANISM IN CHOLERA, &c.

To the Editor of the London Medical Gazette.

SIR,

I do not consider a newspaper as the legitimate channel for the conveyance of medical opinions in general, but, under existing circumstances, some allowance should be made for an anxiety

to communicate any information that we may deem important, on the all-engrossing subject of cholera.

Strongly impressed with the conviction that galvanism might be employed, with good effect, in the alarming cold stage of cholera, I addressed a short letter to the editor of the *Globe* and *Traveller*, on Sunday last, (published in that paper on the 15th instant,) with the hope, that the hint might be immediately conveyed to some of my professional brethren at Sunderland.

With your permission, I am desirous of making a few further observations, through the channel of your excellent periodical.

If we may depend upon the experiments, recently made, touching the identity of the "nervous power" and galvanism, as connected with the fact of the evolution of caloric from arterial blood, they may lead to the application of galvanism, for very important remediate purposes, in other diseases, although my present views are principally directed to "spasmodic cholera."

It has fallen to my lot to witness and to treat *very many* of the worst kind of fevers, modified by the different climates of the four quarters of the globe; and it is the conviction of my mind, from all that I have seen and read, that the modified spasmodic cholera, as it has appeared on the continent of Europe, is a fever, only differing in degree, and in a very few symptoms, from some other fevers of a malignant nature.

The poison producing these fevers, (whether arising from malaria, or from the effluvia of the human body,) appears to me to make its primary attack upon the nervous system; and, by its powerful sedative effect, deprives the body of that nervous influence by which only, caloric can be evolved from the arterial blood. Hence the coldness of the body immediately manifested outwardly; and which (coldness) diminishing the capacity of the bloodvessels near the surface, and forcing the blood to retire inwardly, produces that state of congestion of the vital organs so strikingly illustrated by the laboured breathing and languid and oppressed state of the circulation, as well as by the gastric symptoms which generally supervene. The spasms I also attribute to the interruption of the supply of the nervous influence to the different muscles.

With all due deference to those gentlemen who recommend the use of internal stimulants, I must take leave to say, that I think they frequently do much harm. I sincerely believe that the congestive state is rather increased than relieved by giving such quantities of liquid fire.

If I might be permitted to recommend a plan of treatment, I should say, *immediately* on the attack of cholera, place the patient in a warm bed, solicit a return of the circulation to the surface, by means of the hot air-bath, and, if necessary, by passing a gentle and continued stream of galvanism from head to foot of the patient; and then relieve internal congestion by the abstraction of blood from the arm. If the gastric symptoms are severe, I believe that moderate, not large, doses of calomel and opium, frequently repeated, will be found most serviceable in allaying irritation; and even the exhibition of a few drops of prussic acid might be resorted to, in the event of vomiting being a very urgent symptom. The calomel would also have, in the course of a short time, a very beneficial effect, by emulging the liver; for even in the severe cases of English cholera, I have observed that the flow of bile is generally suspended.

It is well known that venesection has been resorted to, with much benefit, in the cold stage of obstinate intermittents. There can be little doubt but that bloodletting relieves, by abating the internal congestion, which had been brought on by the coldness of the surface of the body; but, in these cases, if the hot air-bath were applied at the same time, to solicit a return of the circulation, to the blood-vessels near the surface, the good effect of venesection would be more evident.

On the subject of contagion, I will say but little. I believe the following to be the real state of the matter. When a person, at all predisposed to receive infection, is exposed to the poison of cholera in a concentrated state, he will certainly get the disease; but if not in some measure predisposed to receive infection, he will in all probability escape, even when exposed to the concentrated poison: on the other hand, a person must be strongly predisposed to receive infection, before he can contract the disease, when exposed to the poison in a *diluted* state. Hence arises the

discrepancy of opinion on this subject.

I will close this letter by mentioning an apt illustration of the susceptibility of the same person, at different times and under different states of the body. In the summer of 1801, I had under my care, on board a hospital-ship, in the river Medway, more than a hundred cases of the worst type of typhus fever. For several months I visited these patients three or four times daily, and slept in apartments divided from the sick only by a thin partition of boards, and consequently breathed the hospital atmosphere night and day. Yet, I escaped the fever, although many died around me. One night, however, I unfortunately slept at a friend's house, a few miles distant in the country, and being anxious about a French officer, a prisoner of war, then under my care, I walked from my friend's house, when, heated, and rather tired, I entered the hospital, without resting, or taking my breakfast. I caught the fever, and *narrowly, indeed*, did I escape with my life.—I remain, sir,

Your very obedient servant,
JOHN TWEEDALE, M.D.

Lynn, Norfolk,
Nov. 18, 1831.

APPLICATION OF GALVANISM IN CHOLERA.

To the Editor of the London Medical Gazette.

Lynn-Regis, Norfolk,

SIR,

You can readily imagine that the inhabitants of this place, from their connexion by shipping with the port of Sunderland, are at the present moment in no small degree under the influence of fear of cholera coming amongst them, and that there exists some foundation for this fear no one will deny. The consequences of the panic are, that every individual is putting his house into order; and the magistracy, the head of which possesses energy, and the best of all senses, *common sense*, have to the utmost of their power instituted the wisest precautionary measures, and we are now, as men on the eve of battle, anxious, yet vigilant and prepared.

Of course the medical men at this juncture are not sleeping at their posts, but are cheerfully co-operating, giving their time and thoughts to every measure that is likely to conduce to the health and safety of the inhabitants.

I now address myself to you, for the purpose of eliciting your opinion, and the opinions of the profession generally, through you, (if you deem it worth while) on the probable utility of the application of galvanism in cholera. I have forwarded a letter to the president of the Medical Board in London on the subject, and have no doubt, if the suggestion is of the smallest value, it will meet with attention. I give the following extract from the communication alluded to:—

“The paralyzing effects of the cholera poison upon the viscera of the chest and abdomen, through the medium of the pneumo-gastric and sympathetic nerves, are so abundantly apparent that it has long since struck me (as most probably it has others) that the instantaneously stimulating powers of galvanism may be brought into operation in the stage of deathly collapse, so as to induce reaction, and by giving vigour to the circulation, &c. enable us to pursue a course of practice which the developed disease demands.”

I have not now leisure to proceed further with the subject, but may just mention, that having seen the extraordinary resuscitating powers of galvanism in the case of my own child, who I have every reason to imagine was restored by the influence of galvanism applied for more than twenty minutes along the course of the nerves proceeding to the heart and chest—after being laid lifeless, to all appearance, on its couch from struggling with continued and appalling convulsions for five days, I cannot but entertain a hope that the same means may prove useful in rousing the system out of the overwhelming depression of all its energies, which appears to constitute the most appalling feature and dangerous symptom of cholera.

I have the honour to be, Sir,

Your most obedient servant,
THOMAS INGLE.

CHOLERA—A HINT TO THE LADIES.

—
To the Editor of the London Medical Gazette.

SIR,

AT a time like this, when every mind is agitated by direful apprehension of the awful plague which, having ravaged so large a portion of the continent, has at length reached our shores, no suggestions, however apparently trifling, can, I trust, be deemed really unimportant if they tend, in any degree, to promote precautionary measures, especially, too, when those suggestions are addressed to the "better half of man," and may possibly guard them from much danger. I would, therefore, through the medium of your highly-valuable and intelligent journal, earnestly call their attention to the few following observations, and beseech them to put them into practice, since, as they cannot at all events be injurious, they may be eminently beneficial. First, then, I would strongly recommend them to pay particular and constant attention to the preservation of warmth, which is universally acknowledged to be one of the most efficient means of prevention. With this view, they must, of course, give strict attention to their clothing, selecting such articles of dress as are of the warmest texture, without so much regard to appearances as too generally prevails, and especially keeping warmly covered the neck, bosom, feet, and arms—parts of the body the chilling of which occasions often the most lamentable results. I would advise them to discard the thin and flimsy silk stockings which are so much worn, and adopt woollen ones, with boots; for cold feet in an eminent degree contribute to produce severe affections of those very organs most concerned in cholera. What are called snow-boots, also, would be very useful and comfortable for exercising in, both in carriages and when walking: they may not be very elegant; but surely that should be no consideration when the preservation of life is put in competition. Next I would advise them to avoid the practice of tight-lacing, than which nothing can at any time be more injurious, but now of the most vital necessity; for only let them consider the importance of the organs which are compressed and squeezed out of all natural

shape and proportion by such a habit; and let them be informed that two of these—the lungs—are great outlets to carry off any deleterious matter in the system, which function they cannot perform if their size is diminished, and their action hindered by the forcible compression of the chest, occasioned by tight stays.

Next, I would most seriously urge upon them not to frequent large, crowded, and heated parties and assemblies, where they are compelled to breathe over and over again the contaminated atmosphere created by the congregation of many persons, and where also they are more subject to the effects of cold air: when quitting the heated and crowded room they have often, in winter, to encounter a cutting, frosty wind, and perhaps sleet or snow, in entering their carriages. In addition to the foregoing advice, I would also recommend those of a nervous temperament, and those with languid circulation, to have the limbs and surface of the body well rubbed every morning and evening, for at least half an hour each time, so as to excite a healthy action of the nerves and blood-vessels, and regularly to take exercise by walking in the open air for two hours at least daily, and to observe regular and earlier hours for their meals than are now the fashion. And last, though not least, let them, putting a firm trust in that Almighty power which can alone direct and stay this plague, discard as much as possible unnecessary fear, and employ themselves in cheerful occupations, with which domestic affairs will supply them; and in a blessed way may their minds be further preserved from personal apprehension by their preparing, as far as is in their power, articles of clothing, and other comforts, to distribute among their poorer fellow-creatures, who, if attacked by this horrible malady, will in this manner be better provided to withstand its effects, and whose blessings will carry joy and peace to their hearts. If, sir, these few suggestions should be thought by you worthy of insertion, you will much oblige one who is anxious for the welfare of the fair sex, and who begs leave to remain your obedient servant,

PHILOGYNAIKOS.

Nov. 15th, 1831.

A BRIEF SKETCH
OF
THE BOTANY OF THE VICINITY OF
SYDNEY, NEW SOUTH WALES.

BY GEORGE BENNETT.

HAVING arrived at this interesting colony on the 22d of January, 1829, my attention was soon directed towards its numerous vegetable productions; but my brief sojourn did not permit any extension of my *herborizations* far inland. The Botanic Garden, however, established at Sydney (under the superintendence of Mr. Frazer), afforded me ample opportunities of viewing the more distant indigenous productions of the colony.

The country in the vicinity of Sydney was strewed with a profusion of flowering shrubs, thriving in an arid soil; but there seemed to be a peculiar character in the vegetation of this country, the foliage of the Australian trees and shrubs having a dry appearance, and being destitute of that lustre so observable usually in those of other countries. This want of lustre is attributed, by Mr. Brown, to the equal existence of cutaneous glands on both surfaces of the leaf*. Another peculiarity is the lofty trees being destitute of branches, excepting at the summits, and shedding their bark; some being observed perfectly decorticated, and in others the cortex is seen hanging in long strips about the trunk. All these peculiarities conveying to us different ideas from those which had been formed from the vegetation of other countries.

Among the extensive Proteaceæ family was the *Grevillea sericea*, *Isopogon anethifolius*, &c. and the beautiful *Banksia* genus (the honeysuckle of the colonists)†, the wood of which is used

by the colonists for boat-building: among numerous species of this genus was the *spinosa*, *serrata*, *integrifolia*, *ericæfolia*, &c. The *Hakea* genus was also abundant. That curious tree of the *Asphodelæ* family, the *Xanthorrhæa*, or yellow gum-tree, attracted observation by its peculiar growth, long grassy foliage (from which it derives its name of grass-tree from the colonists), and towering scape, terminating in a cylindrical spike, either crowded with its small white melliferous flowers or dark-coloured angulated capsules, containing small black seeds.

At a beautiful glen at Little Cudjee, near the south head of Port Jackson, among other plants I collected the *Smilax glycyphylla*, or sweet tea-plant of the colonists, which was, on the first establishment of the colony, extensively used instead of the "China shrub." It is a scandent plant, and the leaves have a sweetish taste, resembling that of liquorice-root, which afterwards leaves a very slight bitter. Among the ferns collected were the *Davallia elegans*, *Gleichenia microphylla*, *Polypodium attenuatum* (Brown Prod. Nov. Holl.), and others. In the vales were found that elegant Proteaceous shrub the *Warratah*, or tulip-tree, *Telopea speciosissima*, and the *Xylomelum pyrifforme* (Proteaceæ), or native pear-tree, so named from its pyriform fruit.

In the Botanic Garden, the species of *Acacia* indigenous to Australia were very numerous; among them was the *A. stenophylla*, from the south-west interior, and was the only tree seen on the horizon at the *ne plus ultra* of Oxley's expedition of 1817, at the termination of the River Lachlan; the *A. fillifanilius*, from the deserts of the south-west interior (vide Oxley's Expedition, 1817); *A. floribunda*, *pubescens*, *falcata**, &c. &c. The magnificent Norfolk Island pine (*Araucaria excelsa*) also grows in the garden as well as at other parts of the colony, where it forms an adornment to the avenues, &c.

The olive trees thrive well in the colony, and attain, it is said, a greater size in a shorter period of time, than even in their native country. Young plants are propagated by layers, and are given to such of the settlers as are desirous of raising this valuable shrub. The *Casuarina palludosa*, *Hibiscus spec-*

* "It is at least certain that on this microscopic character of the equal existence of cutaneous glands on both surfaces of the leaf, depends that want of lustre which is so remarkable in the forests of New Holland."—*Sketch of the Botany of the Vicinity of Swan River*, by R. Brown, Esq. F.R.S. published in the Journal of the Royal Geographical Society of London, 1830-31.

† "When the different species of *Banksia* first come into bloom, the natives collect from the flowers a considerable quantity of honey, of which they are particularly fond, and gather large quantities of the flowers (*moncat*) to suck. It is not, however, always to be procured: the best time is in the morning, when much dew is deposited on the ground; also in cloudy, but not wet weather."—*Description of the Natives of King George's Sound (Swan River Colony) and adjoining Country*, by Mr. Scott Hind, published in the Journal of the Royal Geographical Society of London, 1830-31, p. 35.

* This species is found very abundant at most of the Polynesian Islands.

tabilis, and several new plants and shrubs from Moreton Bay, with a collection too extensive to enumerate, sufficiently engages the attention of the visitors to this garden. The castor oil tree (*Ricinus communis*) abounds almost in a wild state, and yet the oil is still imported into the colony, when any quantity that may be required could be expressed, not only for medicinal purposes, but for burning in the lamps, for which latter purpose it is used in some parts of South America.

On the 13th of February (1829), I visited Botany Bay, and collected numerous botanical specimens. Among several of the *Encalyptus* genus was observed the *E. resinifera*, or red gum tree, *E. corymbosa*, &c. The leaves of most of this genus have a peculiar smell when broken, somewhat resembling the cajuput oil; there is one species in the botanic garden which had been brought from the interior, the leaves of which yielded a quantity of oil resembling the cajuput; on breaking a leaf in the hand, the oil which exuded was very abundant. During this *herborization* I collected among others the following plants:—

<i>Burronia polygalafolia</i> .	<i>Oxalis Australis</i> .
<i>Leptospermum Juniperina</i> .	<i>Epacris microphylla</i> .
<i>Melaleuca Fimbriata</i> .	<i>Exocarpus cupressiforme</i> .
<i>Metrosideros lancifolia</i> .	<i>Sympheonia montana</i> .
<i>Viminaria juncea</i> .	<i>Liptomera acida</i> .
<i>Epacris palludosa</i> .	<i>Enokelia major et minor</i> .
<i>Calytus Australis</i> .	<i>Lobelia gracilis</i> .
<i>Hibbertia volubilis</i> .	<i>Lambertia formosa</i> .
<i>Blandfordia nobilis</i> .	&c. &c. &c.

Botany, as well as other branches of natural history, has an extensive range in Australasia; the discoveries made have been very numerous; and when it is considered that an immense extent of land still remains unexplored, many treasures in natural history may yet be looked for from that new and extraordinary portion of the globe.

London, Nov. 12, 1831.

REMARKS ON SOME REMEDIES IN THE TREATMENT OF INSANITY.

To the Editor of the London Medical Gazette.

SIR,

It appears to me, upon perusing the report of Dr. Seymour's Croonian lecture on the medical treatment of Insanity, in

the Medical Gazette of October 29th, that some errors must have occurred in the abridging of it, which render the lecturer's meaning obscure; nay, perhaps, pervert it.*

Dr. Seymour is therein reported to advise "blood-letting, principally from the jugular vein, if on accurate inquiry the patient has been exposed to causes sufficient to produce inflammation of the brain, redness of the countenance, &c." because such symptoms "point out increased vascular action, with increased power." A case is cited to prove the good effects of this practice; but no symptoms are described indicative of cerebral inflammation, though many of mental derangement. As Dr. S. subsequently cautions against general blood-letting in cases of insanity, I think it is clear he never would prescribe it because a patient had merely been exposed to sufficient cause to produce inflammation of the brain, for he has too much experience and judgment not to know that exposure to the same causes may produce either acute inflammation, or simple derangement of the faculties of the mind.

I am the more anxious that the doctor should be clearly understood on this subject; for having very fully discussed the effects of abstractions of blood in the cure of insanity†, and knowing that medical practitioners are beginning to be aware of the danger of copious resection in such cases, misconception might occasion a relapse into former errors.

Again, there is another passage which strikes me as a misinterpretation.

Dr. S. is reported to say, that physicians, "in seeking for means to allay the inordinately increased sensibility, principally, of course, occurring in that division of insanity termed *dementia*," should commence with the application of cold, in the form of ice, the shower-bath, or in a descending column of water, termed the "douche," not only to diminish vascular excitement, but as having a really sedative effect.

I fully appreciate the beneficial effects of refrigeration in a high state of vascular excitement, and the sedative powers, when the brain is the seat of excitation from that cause. *Si calor, aqua frigida multa perfundere caput*

* The circumstances under which the paper was published, render it impossible that this should have been the case.—ED. GAZ.

† Commentaries on Insanity, pp. 533-533.

expedit, is a Celsian axiom never to be forgotten in the cure of insanity. But it should be noted, that this form of the malady (dementia) is rarely accompanied by vascular excitement, and inordinately increased sensibility.

Dementia is considered by Esquirol, and others, to be owing either to the sensitive organs becoming weakened, or because the brain itself has not sufficient powers to receive and retain the impression which is transmitted to it. In fact, it implies that the energy of the brain is impaired, and therefore demands remedies, not to allay, but to stimulate and strengthen its energies,

There is, however, an acute form of dementia, attended occasionally by symptoms of vascular and cerebral excitation; and, during the existence of those symptoms, and only then, local sedative applications to the head are indicated. It is to this form of dementia solely that, I have no doubt, Dr. Seymour refers.

But the "douche," once so infallible, and so great a favourite with French physicians in the treatment of insanity, like most extravagantly vaunted remedies, has had its day, and is now comparatively little used as a means of cure. When resorted to, it is more as a means of repression, and a moral agent in furious cases. Death, disorganization of the brain, and incurability, has been not unfrequently the result of its application. As a remedy, it is now actually proscribed, in many of the best-conducted continental lunatic asylums, as absolutely dangerous. In British asylums, objections have always been made to the use of the douche.

The case quoted by Dr. S. (p. 113) is an excellent illustration of the good effects of the shower-bath in *furious mania*; but your reporter, Mr. Editor, has, no doubt erroneously, made the learned lecturer conclude, that, from the statement in that case, "it is to cases of *dementia* that it is applicable."

In a malady so obnoxious to remedies as insanity, every one suggested by respectable authority should have a fair trial. On Dr. Jenner's recommendation principally, I gave an ample trial to the inunction of tartarized antimony, as a counter-irritant. My success was by no means commensurate with the expectations raised by his account of its good effects in maniacal cases. The only cases wherein I found it useful

were when accustomed cutaneous eruptions had omitted to appear, or when they had suddenly receded, or when there had been, previously to the maniacal attack, great gastric irritation.

Dr. Jenner's formula* for this remedy is the best, because the most expeditious in its effects on the skin. If counter-irritation be the sole object of blistering in mania, the tartarized antimony is very much to be preferred to cantharides. Vesication, by the application of the latter, especially if there exists much vascular excitement, commonly increases violence, and often superadds the irritation of strangury. From the pustular eruptions of the former, such consequences are not to be apprehended.

In respect to the effects of narcotics, especially of opium, in cases of insanity, about which so great discrepancy prevails, I shall take the liberty of offering a few observations, by way of explanation. Having very fully treated on this subject elsewhere, I will be here as brief as possible†.

I suspect that the discrepancies concerning the effects of narcotics, in the treatment of insanity, have arisen from ignorance, or disregard of the distinct stages which this malady assumes.

Mania, certainly, comes on sometimes very suddenly; but commonly there are three distinct stages observable—1, incipient; 2, active; 3, decline. In each of these stages the symptoms differ in form or intensity, and in each the exhibition of a narcotic, particularly of opium, will produce different effects.

The good, or ill, or no effect of an opiate, is chiefly regulated by the observance or omission of having previously removed cerebral congestion and excitement, by local abstraction of blood from the scalp, and by emptying the primæ viæ. Unless evacuation be premised, sleep may sometimes be enforced by an opiate, but the degree of excitation will assuredly be aggravated.

Dr. S. is misinformed in stating that that powerful sedative, hydrocyanic acid, had not been prescribed in this country in the treatment of insanity.

During a tour I made of the public lunatic asylums in Great Britain, in the years 1821 and 1822, I learnt that very full trials had been made of its effects in

* Supra cit. p. 623.

† Ibid. pp. 610-618.

several of them. As of all other remedies, its effects were differently represented. Some practitioners had abandoned it as inefficient; others thought it had been beneficial in a few cases; others again had a high opinion of its antimanical powers.

The physician who, I believe, has given it the longest and most extensive trial, is my friend Dr. Balmanno, of the Glasgow Lunatic Asylum. I have adverted to his experience of the effects of prussic acid in my work on Insanity, and the result of my trial of its virtues on the recommendation of this experienced physician*. It is candid to confess, that mine was not a fair trial of its virtues; for, knowing its active and dangerous properties, I was unwilling to entrust its exhibition to any one but myself, and as I could not always be present to see it administered with due regularity, I did not persist with it.

In answer to an inquiry I made of Dr. Balmanno in 1828, whether he continued to exhibit this preparation, he says, "I still use the prussic acid, and generally in combination with hyosciamus. The formula is the following:—

R Extracti Hyosciami, ℥j.
Syrupi Sacchari, ℥xij. Misce.

"This is 5ij. of the extract to ℥j. of the syrup. Of this syrup I never give less than 3ij. for a dose, commonly with gtt. xxx. of the prussic acid. When this does not procure rest or quietness, I increase the dose; and I increased it in one case to an ounce and a half of the syrup, and eighty drops of the prussic acid, for one dose, with complete effect. In short, I never fail to quiet the most highly-excited maniac by this medium. Generally, throughout the day following a sufficient dose, the patient is calm, or somewhat stupid. I never saw the smallest injury produced by the largest dose I have given."

Aware of the fatal result attending in Dr. Ferrus's cases at the Bicêtre, in Paris, from the hydrocyanic acid being administered prepared from a different formula to that which he had been accustomed to prescribe, I requested Dr. Balmanno to inform me what preparation of it he made use of. In a letter I received from him last year, he observes, "I have not yet slain any of my patients by the prussic acid; but I still give the

medicine with the hyoscyamus; and both in mania, and in the delirium of typhus fever, I find the combination effectual. The prussic acid which I use is that which is commonly procured from the shops."

In respect to arsenic, the utility of which Dr. Seymour suggests in maniacal cases, and which he believes has not been tried, I may remark that it has been recommended by several British authors in chronic insanity assuming a decidedly periodical type; and in Germany especially, and in other countries of Europe, arsenic has been most extensively used with the same intention.

Having before fully commented on the other remedies discussed by Dr. S. in the work referred to, I shall not lengthen this letter by a repetition of my remarks. Several, however, are omitted, such as digitalis, gyration, and the swing, highly deserving notice in a review of the remedies applicable to the medical treatment of insanity.

I am, Sir,

Your obedient servant,

G. MANN BURROWS.

64, Great Russell-Street, Bloomsbury-Square,
November 1831.

CASE OF PROLAPSUS UTERI,

Cured by a New Operation.

BY MARSHALL HALL, M.D. F.R.S.E. &c.

THE subject of the case which I am about to detail, was a poor woman, whose bread depended upon the labour of her hands. Her sufferings, from the prolapsed state of the uterus, were often extreme, and she was frequently disabled from engaging in her various occupations.

For several years there had been complete prolapsus of the uterus; to this were also conjoined a partial descent of the bladder at the anterior, and of the rectum, formed into a pouch, at the posterior part of this prolapsus. The os uteri protruded at least two inches beyond the os externum.

It occurred to me that, if the canal of the vagina could be considerably, permanently, and firmly, reduced in its diameter, the uterus would be supported

* Supra cit. p. 656.

in its place, and prevented from resuming its prolapsed situation; and that this might be done by removing a portion of its mucous membrane along the anterior part, and by bringing and returning the denuded surfaces in contact by successive deep sutures, until they should unite by cicatrix.

This operation was performed by Mr. Heming, of Kentish Town. The uterus being protruded as much as possible, by the efforts of the patient, two parallel incisions were made through the mucous membrane, from the sides of the os uteri, along the course of the protruded vagina, to the os externum; the portion of this membrane situated between these incisions was then removed, leaving a space of one inch and a half in breadth, and of the entire length of the vagina, completely denuded. A suture was then inserted, near the os uteri. This suture being tightened, the os uteri was obviously pushed upwards. A second, a third, and other ligatures, were then inserted, in the same manner, at short intervals, to the os externum; each ligature, on being tightened, moving and supporting the os uteri upwards.

This operation was attended with little pain; the only sensitive parts of the membrane being those near the os uteri and os externum.

The patient was directed to keep quiet in bed. The bowels had been opened. An opiate was given. No pain or fever followed. In four or five weeks the denuded parts had firmly united, and shortly afterwards the ligatures were come away.

On examination, six, eight, and ten weeks after the operation, the os uteri could be just felt *in situ*, by the finger passed through the vagina: the vagina was firmly contracted along its whole course.

The prolapsus of the uterus was thus completely remedied. The descent of the pouch of the rectum was lessened.

14, Manchester-Square,
Nov. 19, 1831.

P. S. The principle upon which this case was treated, is illustrated by a fact, detailed to me by Dr. Holland, of Queen-Street, May-Fair. A pessary, introduced in a young person to support the uterus, subject to be completely prolapsed, induced great inflammation. This was followed by such firm contraction of the vagina, that the uterus ever

afterwards remained in its proper situation.

MEDICAL GAZETTE.

Saturday, November 26, 1831.

"Licet omnibus, licet etiam mihi, dignitatem *Ar-
tis Medicæ* lueri; potestas modo veniendi in pub-
licum sit, dicendi periculum non recuso."—CICERO.

THE "BURKING" SYSTEM.

THE fearful disclosures made within the last week or two, through the activity of our excellent police, have thoroughly and justly alarmed the public mind. The cry of "burking" is once more revived; and, candidly speaking, (apart from the horror which we entertain in common with the rest of society for this worse than cannibal crime,) we are not sorry that it is revived. The existence of such a state of things in any community, as that wretches should be induced, under any circumstances, to murder the living for the sake of the profits arising from the sale of the dead, is dreadful; but if it does exist, it is undoubtedly better it should be known—else will the said community for ever remain obstinately bent upon avoiding the only reasonable measures for its prevention.

The preventive remedy is no secret—yet its use, we see, is constantly declined, because, forsooth, some say, it would interfere with certain supposed sympathies of persons unknown, or rather perhaps (if the truth were more honestly told) because it would prove to be productive of inconvenience to an influential class of individuals interested; and all this, while the evil to be remedied—especially in its recent hideous shape, is held to be of uncommonly rare occurrence.

We really believe that many were beginning to think of the Edinburgh atrocities as a species of horror unique and *sui generis*, and that its foul perpetrator was a monster such as the world never saw before, nor were

likely ever to see again: certain it is, that vivid impressions of this "deed without a name" (until one was given it after the arch criminal) were fast wearing out, and that many recurred to the facts almost as they would to some harrowing spectacle which they had witnessed on the mimic stage, rather than on that of real life. From time to time a faint alarm would be raised of the existence of similar criminality here and there through the country; but the rumours were ill-defined, and such frequently, as upon being discovered to be destitute of foundation, served but for merriment; until a period at length elapsed (however short) sufficient to tranquillize every apprehension about the reappearance of similar guilt, and to render people unprepared for a second revealment of "burking." This false security is now disturbed—the film is removed from the eyes of the short-sighted—and they are painfully awakened to the dread possibility of this horrible crime going on within the very range of their daily walks. We are sorry we cannot condole with them: we do not on this question belong to the party of the tender-hearted incredulous, but to our shame, (if they will have it so) be it spoken, we confess ourselves to be rather content with the disclosures which have been made, and strongly inclined to congratulate the public upon them. As to surprise—we are sorry to say that it is an occasion on which we can feel none.

Truth to speak, whatever may have been our surmises before, we never had the slightest doubt since the revolting confessions in Edinburgh, of the more than possible existence of the same system in London. This great capital, we thought, was too notorious as the Babylonian nursing-mother of every vice under heaven—too full of a progeny ingenious in every branch of iniquity, not to possess at least a reasonable number of adepts deep in the mys-

teries of the new crime, if mystery there can be said to be in any thing about "burking," except in the concealment. We scarcely wanted a Gibbon Wakefield to warn us of our danger; though the recent pamphlet of that writer appears most opportunely to confirm our view of the circumstances, and to convince all good citizens of the desperate gangs of wretches who hover around them every hour—ruffians, rabble, and desperadoes, capable and ready for the commission of any enormity. We were satisfied with what we knew of the habits and capabilities of one set of miscreants tolerated in the community—we had the resurrection-men before our eyes. Far be it from us, however, to raise a single prejudice against the individuals of that class who await their trial at the Old Bailey: we only speak of the class abstractedly, as the most obviously chargeable with a crime like that for which those individuals will be arraigned. Let us take a single human being of that order—one who has fallen, from whatever cause, into that lowest of all vocations, the business of a resurrection-man. His employment is a secret, or known but to a few ill-trusted wretches of his own condition; it suffices not, with all its dangers, for his maintenance; he cannot live by it alone; he becomes a thief—that is, if he have courage enough to raise himself a degree above the abominations of body-snatching; the instruments for grave-breaking are also those for house-breaking, and the same vehicle which is used for removing the contents of the grave serves also for conveying away stolen goods*. This, however, is the picture of a resurrection-man not quite destitute of the humanities—one not yet deadened to the impulses of a sort of stealthy industry, and willing to save himself from the shifts of the lowest and most brutal de-

* See the evidence of the resurrection-men, A. B. and C. D., in the Parliamentary Report of the Committee of Anatomy, 1828.

gradation. But we can conceive another wretch, capable of adding one crime more to the catalogue of his iniquities. He sees before him the prospect of a prize, far more promising than the amount of nine out of ten of the robberies which he might have it in his power to commit; he has within his grasp the fruits without the risks of body-snatching; the temptation from the high premium in view is irresistible, even though the facility of concealment were infinitely less. Now comes the victim in his way—some solitary wanderer, helpless, or rendered so by the stupor of intoxication:—the deed is done!—the murderer is familiar with the appearances of death—he hampers up his *subject*, and hastens for the possession of the lucre which he has earned. The entire order of these miscreants must be abolished: the wretches who live by the spoliation of the grave—much less those who add murder to that offence—must not pollute society with their presence.

But the preventive remedy, as we have said, is no secret. Amend the laws as they affect the practice of dissection—if not for the safety of the public, at least for the preservation of our national character as regards our legislative arrangements. Foreigners should no longer be permitted to observe, that British law is in one part so dangerously absurd, as to ordain that certain of its enactments can only be complied with by a violation of other of its ordinances. Yet so it is; and these are the very enactments and ordinances which profess to regulate our profession, and have at length given rise to a species of crime unknown to former ages, and startling to the contemporaries of this. The time is come when it is imperative on the legislature to interfere: the laws *must* be amended—some provision *must* be made for dissection, *or the practice of anatomy must be prohibited altogether*:

there is no alternative. This point, however, is so clearly expressed in a very sensible letter which appeared in a late number of the *Times*, that we cannot deny ourselves the pleasure of a short extract from it by way of conclusion.

“While such a traffic exists, there can be no security for the public. The legislature of the country can no longer turn a deaf ear to the voice which has so often warned them on the subject. They must interfere in one or other of two ways:—either they must prohibit the teaching of anatomy altogether, and allow medical and surgical science to go back in the next generation to what it was some centuries ago; or they must remove those obstructions which the common law of the land (as it is construed by the judges) has imposed on the study of anatomy by dissection. My firm conviction is, that the latter alternative is completely within their power: that by some very simple enactments the schools of anatomy may be supplied with subjects, without the smallest offence to public decency, without the smallest outrage on the feelings of individuals, and without any danger to human life. No one who will be at the pains to peruse the report of the Anatomical Committee of the House of Commons, and at the same time to make himself acquainted with what is done in France, Italy, Germany, and even in some of the United States of America, can, as I conceive, entertain a doubt as to this opinion being correct.

“If, however, those in whose department it is to make the laws by which we are governed, come to a different conclusion, let them at once and altogether prohibit anatomical instruction. The medical profession, that is, we who are now established in practice, cannot complain that our interests are in any way compromised if they do so. On the contrary, we shall thereby obtain all the advantages of a monopoly. We may then, if we are so disposed, contrive to exercise our art even in our dotage, for there will be no succession of younger and well-educated competitors to push us from our stools. The children and grandchildren of those who now exist, may suffer for the want of medical and surgical assistance, when they sink under the depressing influence

or writhe under the tortures of disease ; but to us, the existing race of medical practitioners, if we were selfish, we could wish for no better consummation than this*."

CHOLERA AT SUNDERLAND.

THE good people of Sunderland really appear in no very favourable light. It seems pretty clear that the public safety is, in their estimation, a very secondary object when brought into competition with the sale of coals. We last week stated our opinion, founded on the evidence of the returns, that the whole truth was not made known ; and Dr. Daun's letter, which has since been published, confirms the accuracy of our suspicions. Upon his representation, and that of Dr. Barry, it is to be presumed that the Privy Council issued the order which appeared a few days ago, by which medical men were every where enjoined to report their cases instantly, under certain pains and penalties. Many of our brethren have an idea that the law does not bear out the imperative tone of the mandate ; but in this they deceive themselves. A reference to the quarantine act will shew that practitioners must obey, or expose themselves to summary punishment. Medical men cannot, however, be compelled to give their services gratuitously ; and the proclamation alluded to will probably make them more chary in doing so ; indeed, we do not see why the *doctor* should be the only professional man whose services are expected to be had for the asking, and quite as a matter of course. Regarding cholera as a contagious disease, we would suggest that an adequate number of medical men should be appointed in each district, to take charge of the sick, and that their practice for the time should be limited to that disease, otherwise how are the public ever to be safe against the constantly renewed dissemination of so diffusible a poison?—

Medical advice, too, though rendered accessible, ought not to be forced upon the poor, else we shall have them accusing the doctors of poisoning them, and then murdering their attendants, in return for their services, as they did by wholesale in several parts of Russia.

As to Sunderland, no doubt is entertained of the disease which prevails there being the same as that which has been the scourge of India and the north of Europe. This has been decided by those who have seen both. We are still left without any official details, either of the symptoms or of the treatment ; but from our own private sources we learn, that the course pursued by the disease is extremely similar to that which it followed in Russia, and that the same post-mortem appearances have presented themselves in the very few dissections which have been made. Brandy and opium have been extensively used as remedies ; indeed, a hogshead of the former, ready mixed in due proportion with laudanum, was supplied by government for the use of the poorer patients. A cholera hospital has been established ; but owing to the malicious report that the bodies of the dead were mangled, very few can be persuaded to avail themselves of it. One striking peculiarity, which is mentioned by several of the Indian writers, has been observed in the disease at Sunderland, namely, the ten-lency, in some cases, to sudden sinking after apparent recovery from the stage of collapse. Thus a man—we believe a sailor—was found with cold surface, blue skin, and imperceptible pulse ; brandy and laudanum were freely administered : in some hours after, he was found sitting up in bed, drinking gruel, and expressing great relief ; yet, from imprudent exertion, or other less obvious cause, the symptoms returned, were uncontrolled by the most powerful remedies, and he died next day. If there has been no cholera but what is

* Times, Nov. 18, 1831.

given as such in the returns, then the disease has not made much head, though the rate of mortality has been great; but if we are to regard all the cases of diarrhœa and common cholera as *the* disease, then has the rate of mortality been small. We see that they have at length ceased to return as diarrhœa those cases, some of which proved fatal in 24 hours; but their discontinuance shews that this part of the return was, to say the least of it, uncandid. As to their "common" cholera, it is evidently a very *uncommon* form of that complaint. Dr. Barry has been to Newcastle, where he found the report of the disease having broken out to be groundless: he has now returned to Sunderland.

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WHAT IS MEANT BY LIVING WELL?

By far the most popular recommendation, as a safeguard against cholera, is "to live well," by which most people understand eating somewhat more than their accustomed quantity, and taking an extra glass or two of wine after dinner. No one certainly thinks of diminishing his allowance: even the habitual tippler protests that any withdrawal of his accustomed stimulus, however slight, leads to debility. It is high time that this pernicious misapprehension should be removed. "To live well," means to live "regularly," taking due nourishment, and no more—eating and drinking to support the body, not to pamper the appetite. Viewed in this light, we would ask how many there are who do not already eat and drink enough, and more than enough? certainly not one man of a hundred, in easy circumstances. Almost the only thing which prevents people from eating and drinking too much, is being unable to afford it. Our injunction to all those who talk to us about "living well," is never to take more than they can easily digest—to regard all fortifying against the cholera, which produces feelings of oppression, as so much assistance lent to the enemy—and more especially with regard to fermented liquors, that all exhilaration must be followed by its corresponding period of depression—and that he stands the best chance of exception who so regulates his diet and regimen as to be attended with the least variation of the pulse during the twenty-four hours.

These objections to an increase of the "creature comforts," apply with equal force to persons drugging themselves with bark or quina. Where there is much screwing up, the nerves will more readily become unstrung, and then is just the moment for any prevalent disease to make its attack. We cannot, however, but commend the exertions which are every where being made to add to the comforts of the poor: let the rich be assured that this is the most effectual way of keeping the evil from their own doors: the best way for them to live *well* is to put it in the power of their less fortunate brethren to live *better*.

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VOLUNTARY DISPOSAL OF THE BODY.

DR. MACARTNEY'S document is now signed by upwards of 400 names, chiefly of professional men, who are anxious, by practical example, to remove the prejudices which stand in the way of dissection. We believe it is somewhat more than three years now since the document was prepared. Is it not time we heard of some *actual* example?—or who shall have the honour of the proto-martyrdom?

There is one obvious disadvantage attending this sort of post-obit bond in favour of anatomy: it leads the public to suppose that medical men are more interested in the matter than other people; whereas, there is no class of persons who would benefit so much by the entire prohibition of dissection as the present race of practitioners. Their profession would become a complete monopoly, and no fresh competitors could arise to "push them from their stools."

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CONTAGIOUSNESS OF CHOLERA.

It is worth observing, that M. Brierre de Boismont, the companion of Le Gallois, and the first of the French physicians who published an account of cholera after returning from Warsaw, is a decided contagionist. Nothing can exceed the scrupulous accuracy with which he seems to have made up his mind on this subject. On one point he gives the most satisfactory assurances: the Polish army was free from epidemic of any sort previous to the battle (of Iganie) on the 10th of April: the cholera made its appearance immediately afterwards: and on three distinct occasions that a

healthy division of the army came in contact with the Russians, so often did the disease regularly develop itself.

Some papers on cholera, published under the sanction of the French embassy in Russia, and attributed to M. de Mortemart, furnish abundant testimony in favour of the doctrine of contagion: it is singular, however, that the non-contagionists have laid hold of these papers as favourable to their views, though M. de M. has distinctly stated in his 13th page, (what he frequently afterwards reiterated in substance) that "*le cholera est une maladie très sensiblement expansive dans les lieux où elle est arrivée, très subitement transmissible d'un lieu à une autre par l'intermédiaire d'objets matériels, soit vivans, soit inanimés.*"

M. LE GALLOIS.

THIS gentleman sunk, under the sequelæ of typhus fever, at Landsburg, in Prussia, on his return from Warsaw. He was only in his 26th year, but already distinguished as an able experimentalist in physiology. On the mission to Warsaw, he attracted general admiration by his energy, humanity, and talent. His opinions on cholera are incorporated with those recently published by his friend, M. de Boismont.

THE FINE ARTS.

M. DALMAS, from Warsaw, has presented the French Academy of Medicine with a series of drawings descriptive of the appearance of patients in the several stages of cholera. M. D. acted both the artist and the physician to his patients; and his work is said to have been executed with great skill.

WESTMINSTER MEDICAL SOCIETY.

Saturday, Nov. 19.

MR. CHINNOCK IN THE CHAIR.

Discussion on the Question, is Cholera contagious?

THE meeting took place in the theatre, Great Windmill-street, and was very numerous attended; in fact, by far the most so that we have ever witnessed.

When the minutes had been read, Dr. GRANVILLE rose and proposed, that, after three nights' discussion, the question of contagion should be passed over; and he moved, that the society do now proceed to consider the nature and treatment of the disease. This motion was not seconded, and, in fact, could not be entertained, inasmuch as it had pre-

viously been decided and announced that the subject alluded to should come on this evening.

Dr. COPLAND thought that the number of direct and incontrovertible proofs of the infectious nature of the present disease, furnished by the speakers on his side of the question during the previous evenings, was so great as to render it unnecessary for him to go into this part of the evidence further than to advert to certain facts and considerations which had not been touched on by previous speakers. In respect to the direct proofs of the infectious nature of the pestilence observed in India, notwithstanding the representation to the contrary, they were so numerous and so strong, that two of the Medical Boards of the Presidencies had insisted on the existence of this property of the disease; and the third, after great hesitation, concluded by stating a decided opinion to the same effect. Even the non-infectionists furnished unequivocal proofs of the existence of the property against which they were contending. Of the various circumstances and phenomena of the disease there were none which in his (Dr. C.'s) mind more strongly evinced its infectious nature, and its origin from an infectious principle, than the specific and uniform character it has always presented, both in Asia and in Europe, and in every season, temperature, and locality. This specific and uniform appearance evinced a specific cause, reasoning from our knowledge of other specific diseases. Another circumstance, which he esteemed as distinctly proving its infectious nature, is the peculiar and particularly offensive odour which the disease is described, as observed, both in Asia and in Europe, to emit during life and after death,—a fact which is likewise observed in all other infectious maladies, and therefore shewing its close alliance in its infectious nature to them, particularly as the infection has been traced by the patients themselves to their having inhaled this morbid effluvia, in a very sensible manner, from the bodies of the affected.

Dr. Copland next adverted to the principal arguments which have been adduced by those who supported the opposite side of the question. They had assumed that, as the cholera of warm or temperate countries had, previously to the irruption of the present pestilence, been acknowledged not to have been infectious, this pestilence being cholera could not possess a different nature. But he insisted that the cholera of warm climates, which, in its severest forms, has been called *spasmodic cholera*, or *mort de chien*, was a totally different disease from the pestilence which has lately ravaged Asia and parts of Europe. The next argument on which the non-infectionists mainly depended, was the number of those exposed to infection who escaped it. But this was a circumstance common to this and all other infectious maladies, for all are not liable to be infected by

even the most virulent—all are not predisposed. As to the boasted non-infection from inoculation, &c. it proved nothing; for no concentrated virus being generated, it could not be thus propagated,—the materials for inoculation were wanting. Who would expect to communicate scarlet fever, or measles, by inoculation? The non-infectionists also laid great stress upon the number of those attacked, who could not trace the disease to an infectious source; but this, he conceived, is not otherwise than is to be expected, when we consider that a considerable period frequently elapses, in all infectious diseases, between the exposure to infection and the development of the disease. Even admitting only a few days to elapse, who could refer to a particular instance, or even to the several instances, of exposure to infection, either direct or indirect, which may have occurred during that time. The occurrence of epizootics was laid hold of by the non-infectionists, as a proof of the origin of the pestilence in a certain unknown state of the air, &c.; but it should be recollected that this disease had prevailed for fourteen years, and during so long a period was it not to be expected that the temporary coincidence of a more than usual mortality amongst some of the lower animals, with the prevalence of this malady, should be sometimes observed, particularly as epizootics are a frequent occurrence after very wet seasons? but the fact is, that the coincidence has been by no means frequent. As to the argument that the disease would have been kept out by quarantine if it had been infectious, the inference is devoid of weight, and entirely unfounded in fact; for, during the extension of the disease in the East, no quarantine was anywhere observed; and in the few places where it was more recently resorted to, under circumstances which could render it strict, it proved completely successful. When the infectionists argue, that this pestilence manifests a similar mode of communicating itself to that possessed by true typhus fever, one gentleman gets up and denies that this fever is infectious, and consequently that this pestilence must be non-infectious also. But as the false prophets, from whose mantle he has caught his inspiration, have not put their opinions upon record, they cannot be investigated. Every man of experience, and possessed of a sufficient range of observation, independently of the vast body of evidence upon record respecting it, proceeding from the highest and most experienced sources, knows how to deal with this objection. Without referring further to the many considerations which press a conviction of the infectious nature of the present pestilence, Dr. Copland concluded, that, although quarantine regulations could not be expected to succeed in thickly-inhabited and inland countries, they might have been successful as respects this country, on account of

its insulation, and that they had been therefore resorted to with the greatest propriety, and with reasonable expectations of excluding the pestilence.

DR. GEORGE GREGORY began by observing, that if the schoolmaster was abroad, so also was the teacher of physic, for in every society he found persons giving their opinions upon the contagiousness of cholera, and laying down the law upon one of the most difficult and abstruse doctrines in medicine—the origin and propagation of disease. Merchants, and lawyers, and men of fashion, undertake to decide, when they find that “the doctors disagree.” But do the doctors really differ? In this society, three gentlemen only have stood pre-eminently forward as anti-contagionists. One of them, Mr. Searle, avows himself, however, as still open to conviction. Dr. James Johnson acknowledges himself a *contingent* contagionist. With those gentlemen, he (Dr. Gregory) would not contend, but he was astonished beyond measure with the line of argument supported by Dr. Granville. He was at a loss to understand, why the able and conclusive arguments of Dr. Macleod and Mr. Burnett had failed to convince him, who, on former occasions, had proved himself so staunch a supporter of the orthodox doctrine. In this dilemma, he remembered that “his adversary had written a book,” and he sought, in its pages, a clue to explain the mystery. He believed he had found it. In that work (a letter on Plague and Contagion), Dr. Granville, after carefully dividing diseases into sporadic, simple epidemic, infectious or contaminating epidemic, and contagious, lays down two fundamental laws, or maxims:—1st, that epidemic diseases are never contagious; and 2dly, that contagious diseases are never epidemic. This, Dr. G. G. contended, was *prejudicing* the question. It precluded all argument, and all reference to fact. The cholera morbus was avowedly an epidemic, (that is to say, it attacked many individuals in the same place, at the same time, and in the same manner.) Now, epidemic diseases, according to the dictum of Dr. Granville, are never contagious. *Ergo*, cholera is not contagious. This was the logic of Dr. Granville. He (Dr. G. G.) protested against these *axioms*, and argued, that small-pox and plague are both epidemic and contagious. The error lay in drawing a broad line of distinction between contagion (whether mediate or immediate) and communication by a contaminated atmosphere, commonly called *infection*. Such a distinction does not exist in nature. Contagion and infection run into each other by insensible degrees. Some diseases, such as cow-pox and hydrophobia, require, for their propagation, that the miasm, or morbid matter, be applied to the *wounded* surface. Others, again, such as syphilis and tinea capitis, operate on the unbroken skin. The

effluvia arising from the lungs and skin in plague, extend a few inches from the patient's body. In the case of small-pox, the contagion extends through a chamber or ward, and even through the whole house; nor does there appear any reason for confining, even within such limits, the *sphere of contagious influence*. In some cases, it may possibly extend even across a street. What he and those with whom he was this night associated contended for was, *first*, that the cholera morbus was one of those diseases recognized by Dr. Granville as infectious or contaminating epidemics, (such, for example, as typhus fever and scarlatina); and, *secondly*, that infection or contamination of the atmosphere, by the *breath* and diseased secretions, is only a *modification of contact*. This expression, which, in point of fact, strikes at the very root of both Dr. Granville's memorable axioms, is actually to be found in that gentleman's work, (page 26.) Dr. Gregory proceeded to remark, that an absurd prejudice exists in the mind of many against acknowledging contagion (or communication from man to man) as a means of diffusing disease. It has been held up as unscientific, and as *cutting the knot* which the pathologist cannot untie. It has been called the last resource of the *ætiologist*, and only to be admitted when all other means of explaining the propagation of disease failed. He (Dr. G.) could never see the meaning of this, nor understand why a physician should entertain any scruples in acknowledging the influence of contagions, or *contaminating* exhalations from the lungs, skin, and vitiated secretions of persons labouring under disease. But, that such scruples were felt, and that very strongly too, was obvious, from the line of argument pursued by several gentlemen in the course of the debate. Mr. Searle, for instance, attributed the origin of the disease among the Polish troops to a long harassing march, and to the swampy nature of the ground on which they afterwards passed the night. When pressed, however, to say, whether the Russian prisoners, who had *not* made that same march, were yet similarly affected, his reply was, *non mi ricordo*. This silence, upon a point which would go far to settle the question at issue, was very significant. Dr. J. Johnson seemed inclined to refer the cholera to some unusual heat of the earth's surface, adducing, in support of this, the new Volcanic Island in the Mediterranean. Another gentleman strove to connect the appearance of cholera with some alleged irregularities of atmospheric phenomena; but what evidence we had of such, except the hurricane in the West Indies, he (Dr. G. G.) was unable to say. Upon the whole, he viewed these vague attempts at explaining the origin and propagation of cholera as the shifts of ingenious men, whose intellectual pride refused to acknowledge the simpler theory of *communication by contagious effluvia*.

[Either Dr. Copland or Dr. Gregory al-

luded to the fact that Mr. Orton had become a contagionist; to which Dr. Johnson objected, that little importance could be attached to the circumstance, as he was confined by illness in the north of England.]

DR. GILCHRIST having claimed the privilege of a visitor to address the society, proceeded to observe, that the evidence upon which the history of cholera is founded is unworthy of confidence. It was clear, he said, how difficult it was to come at facts, when we observed what was passing before us at Sunderland; how, then, were we to believe statements relative to Astracan? In reply to Dr. Gregory's remarks on Dr. Granville's opinions, as quoted from that gentleman's book, he (Dr. G.) begged leave to observe that Dr. Gregory also had written a book, in which the account of yellow fever was founded upon evidence the most fallacious that had ever been adduced. (The bitterness with which this gentleman spoke in some parts of his address, bore, we regret to say, but too strongly the appearance of his being actuated by personal feeling.) The Chairman begged Dr. Gilchrist to confine himself, as much as possible, to the question before the society.

MR. SEARLE offered some remarks in explanation of what had been said relative to him by Dr. Gregory; and in reply to the observations of Dr. Copland, that the pestilential cholera is a peculiar disease, different from all other diseases called by a similar name, he read a passage from (we believe) Jackson on Fever in the West Indies, in which a series of symptoms are described, the difference between which and those in some of the worst cases that he had witnessed in Poland, was to him inappreciable. This West India case, he had no doubt, was genuine cholera, though not appearing in an epidemic form.

DR. A. T. THOMSON rose to make some remarks on the character of the discussion, and spoke at considerable length on the importance of seeking truth rather than victory. The learned Professor then took an able view of the arguments on both sides of the question, and regretted that, in doing so, he was perhaps (really) obliged to repeat much that had been stated before by other members*. In conclusion, he declared himself perfectly open to conviction, and not wedded to the creed of either party, though the weight of the evidence greatly preponderated on the side of the contagionists.

DR. MACLEOD said, that when on a former evening he had addressed the meeting, unluckily for his argument, though not perhaps for the patience of his hearers the hour had expired while he was yet speaking, and he had thus been obliged to conclude abruptly, and without drawing the inferences which the facts he had mentioned appeared to him to warrant.

* It is on this account alone that we omit the details into which Dr. Thomson entered.

He would not, however, recapitulate what he had formerly said, but merely remind the society that he had dwelt upon three facts, as especially calculated to shew the manner in which the disease was propagated, viz. First, its mode of travelling from country to country, and from town to town, invariably appearing first on the frontier next to an infected district, or if on an island, always first shewing itself in a sea-port—in short, never being met with except where communication had been held with places in which it already prevailed. Secondly, the manner in which it frequently spread through the members of a family, or communicated itself to those who visited the sick, though residing in a different and distant locality. Thirdly, the immunity which had, in numerous well-authenticated instances, attended a rigid isolation, as at Peterhoff and Zarcozel0, where there were from 8 to 10,000 persons who remained unaffected, while it raged around them. But probably most men selected in their own minds particular facts from the general mass, and he would briefly mention one or two which, in his estimation, carried convincing evidence of the disease being propagated by human intercourse. In looking over the Indian reports he found that, as a rule almost universal, there was one spot in each great town which the pestilence long respected; while it decimated the adjacent district, one favoured habitation was either left untouched, or at all events was the last to suffer. This place was the jail. He doubted not (after the specimens of argument which he had heard on the other side,) that he should be told that the regular lives, and perhaps even the philosophic turn of mind, originating in their retired habits, gave to the dwellers in these secluded precincts their apparent immunity from the disease; but, unfortunately for this anticipated explanation, it happened that when once introduced it spread among the prisoners with great and relentless violence, shewing that there was no want of susceptibility, but merely a want of the necessary contamination. Another circumstance which had made a great impression on his mind, was the behaviour of the pestilence on entering Russia;—the manner in which it travelled *up* the Wolga to the north, and then, crossing over to the Don near its source, pursuing a course diametrically opposite, namely, *down* that river, to the south; thus travelling in two nearly parallel lines in contrary directions—a phenomenon perfectly intelligible if the plain straight-forward doctrine of it being carried by man were admitted, but wholly incomprehensible on any other. He had been accustomed constantly to hear, that they only thought the disease contagious who had not seen it, and this had long influenced his mind against the doctrine of contagion; but he had clearly proved, on the evening before last, that such was not the

fact—indeed, that in India and Europe, wherever Boards or Committees of medical men had been appointed to sift the evidence, they had always come to the conclusion that the disease was communicated from man to man; and here he particularly wished to correct an erroneous impression which had been made by what fell from Dr. Granville. That gentleman had stated, that when a committee of physicians was appointed by the Emperor of Russia, to decide on the contagious or non-contagious nature of the disease, they reported that goods did not convey the infection, *having previously stated that it was not communicated by man*. [Dr. Granville here exclaimed, that he had not said so, and that he would reply to this part of Dr. Macleod's observations; which intention, however, he did not carry into effect.] Such certainly was, if not the identical expression, at least precisely the meaning of Dr. Granville's statement, because he (Dr. Macleod) had at the time argued, that Dr. Granville was probably mistaken, else the fact of the report alluded to having led to the rigid exclusion of travellers by triple cordons on the roads, while merchandize was allowed to pass, became perfectly inexplicable. He alluded to the circumstance at present only to state, that he had since consulted the documents themselves, and found, as he had supposed, that although the Russian Board thought the evidence of cholera being conveyed by goods was not sufficiently strong to warrant the stopping of merchandize, they had no doubt whatever of its being conveyed by men. It appeared further, by the official papers lately published, that the Board consisted of forty physicians, of whom thirty eight held the disease to be contagious, and only two advocated the opposite doctrine. Against the numerous instances in which it evidently spread from one individual to another, the non-contagionists adduced examples of persons who had been exposed to the infection, if such it were, and escaped; but the multiplication to any extent, of instances where persons had *not* taken the disease, could never neutralize those where they had taken it. Calculations of the number who escaped might lead us to form some estimate of the proportion of mankind who are susceptible of the disease; but negative evidence can never, by any increase, disprove the positive results opposed to it. Yet this was the constant argument of the non-contagionists. Like the man tried at the Old Bailey for sheep-stealing—"Ah! my Lord," said he, "if I had known that you would have condemned me because two men swore they saw me steal the sheep, I would have brought twenty to swear they did not see me." Another advocate for non-contagion had just arrived on the field—Dr. Hammet, and the event was announced by a flourish of trumpets, the newspapers declaring that he had "several cases

which set the question at rest." Several cases to set such a question at rest! the very announcement shewed the utter ignorance of those by whom it was made of the simplest elements of medical reasoning. But let us inquire what the non-contagionists have to offer in lieu of that doctrine which renders the whole history of this malady, as to its mode of propagation, perfectly plain and simple. Why, nothing—absolutely nothing that will serve either to explain the phenomena, or even that is intelligible to others, whatever it may be to themselves. Thus we are told that cholera is produced by sol-lunar influence. In the name of common sense, what are the sun and moon about now more than they have been since the world began, that they should be accused of engendering this pestilential offspring? Others speak of subterraneous influence. Why do they not tell us what that influence is, which prevails alike in marshy jungles and on arid plains, on the summits of mountain ridges and in the depths of valleys—in every diversity of soil, climate, locality, and all that render things dissimilar. *Subterraneous* influence, not confined to the earth, but in some cases disregarding many hundred miles of intervening sea. This same subterraneous influence appeared to him to be but a subterfuge calculated to mystify rather than explain. It was in vain to grapple with things which had neither form nor body; they were too refined to comprehend, therefore too subtle and visionary for argument to impress. He could not conclude without remarking on the gradual conversion of the non-contagionists. [A loud expressive *hear!* from Mr. King.] As the gentleman had given to his exclamation such an expression of incredulity, he would adduce some instances. At these discussions those on the opposite side had begun by holding cholera not to be contagious at all; now they talked of it not being *more contagious* than typhus, or some other disease assumed to be contagious—this was notorious; and it was quite obvious that they who argued on the degree of contagion, of necessity assumed the existence of that property to some extent or other. Again, it was known to all that Dr. Russell went to Russia a non-contagionist, but that the film had fallen from his eyes, and he returned holding that doctrine no longer. Dr. Lawrie, an intelligent physician, who had seen the disease in India, and had been a non-contagionist, had just acknowledged, in the Glasgow Medical Journal, that farther evidence had changed his opinion; and though last, not least, was Mr. Orton, known as the author of a very valuable work on cholera, formerly a staunch non-contagionist, and who now said that the arguments against contagion were "as dust in the balance"—such were his very words—when weighed against those which are now opposed to them. Dr. James Johnson has made light of this, because he says Mr. Orton has not

of late seen the disease, having been confined to a sick bed. Granted; but these circumstances give additional weight to his opinions. He is now (said the speaker) removed from the scene of action, and can weigh the evidence without the prejudices of a partisan; that his recantation has taken place on a sick bed is the most convincing proof that it is—sincere. It was remarkable, that though the daily press was almost unanimously against contagion, the Reviews and Medical Journals were nearly all in its favour; and as they were better judges, this shewed where the evidence preponderated—indeed, one would think it scarcely possible that two opinions *could* exist with regard to a question where the Medical Gazette and the Lancet embraced the same side, and were seen fighting under the same banner.

Dr. STEWART begged that Dr. Copland would repeat, or more fully state, his opinion relative to the peculiar odour of cholera constituting a discriminative character of the disease.—[Dr. COPLAND explained: but we need not repeat what has been already incorporated in Dr. C.'s opening remarks.] Dr. Stewart, in resuming, observed that it appeared to him, after the very convincing arguments of the contagionists, that there could not be and there really was no difference of opinion in the society with respect to cholera; that all the members seemed to agree that the disease was contagious under certain circumstances.

Mr. BURNETT begged to be allowed a few words in explanation.

In maintaining the contagious character of cholera, he never intended to assume, indeed never did believe, that it or any other contagious disorder was always, and in every case, contracted by all who became exposed to its influence. On the contrary, he had discussed at some length the distinction between contagion and infection, and had insisted particularly on the very different degrees of communicability to be noticed in different disorders. Therefore the anti-contagionists, who, because they can cite the cases of some persons who have been exposed to the influence of cholera and have nevertheless escaped, think thus to confute the contagionists, only knock down a chimaera themselves have raised; for the contagionists have again and again referred to the truism, that many diseases which are sporadic in their origin may become epidemic and contagious—as, for example, typhus fever. Indeed (continued he) there is as much difference between a disease being *communicable* and *communicated*, as between an object being *visible* and being *seen*. An object may be, indeed many objects are, *visible*, which nevertheless are not necessarily *seen*; and a disease may be, indeed many diseases are, *communicable*, which nevertheless are not necessarily *communicated*. Moreover, the contagionists have the temerity to

attempt to prove a negative, and presume to declare that, because one, two, three, or more persons have been with patients suffering from cholera, and have escaped, therefore that cholera is not contagious. But can any conclusion be more illogical? If the cases of escape were ten times more numerous than they are—if, for every three, there were three thousand—they would avail nothing against even one well-marked and decided case of direct communication; all that they could legitimately be brought to prove would be the comparative facility of communication—the relative degree of contagiousness. Indeed, to this point do most even of the former non-contagionists seem to be veering; for one declares that it is *no more contagious than typhus*, and another that it is *contingently contagious*, Dr. Gilchrist being the only spokesman who now dares to uphold the unblenched banner of non-contagion. But what do these gentlemen mean by *contingent contagion*? Are not all contagious diseases *contingently contagious*?—and are not these *contingencies* the predisposing states of the patient's health? This, which they now so readily grant, is all that the contagionists ever have desired: yet why, when they are with us as to *facts*, are they not with us in *names* also?—for what will the public understand by their strenuously repeated affirmations that cholera is *not contagious*? Will the public understand that it is *contingently contagious*?—will they not rather suppose them to mean that it is *not communicable*?—and then, should such a dire disease be suffered unimpeded to devastate this great town, will not the anti-contagionists be rightly accused of breaking the word of promise to the sense, even if they seem to keep it to the sound.

As to the statement of the learned member, Dr. Thomson, that the speakers have appeared to him to be contending for victory rather than for truth, it may be a sufficient answer to acquaint him, that had he been present at the former meeting he would have known that the very course he himself just now pursued, to arrive at truth, was the course that the contagionists have several times journeyed over for that very purpose; that Dr. Macleod, Dr. Sigmond, Mr. North, and himself, have over and over again been obliged to prove and re-prove the facts of communication; that they had traced the progress of the disease from place to place—had shewn that as man travelled it travelled, that as man stopped it stopped; that when communication was interrupted its progress was interrupted; that as travellers journeyed round, it journeyed circuitously with them; and in continuation of these arguments, Dr. Macleod had this evening, among other points, well noticed the important fact that the last places infected with cholera had been the gaols. He might likewise have shewn, not only that it attacks last those places which are most

secluded, but also that it attacks first those which are the most public; for where does cholera first break out?—why, in sea-ports and frontier towns. Where has it first set its foot upon our own land?—why, it has been true to its general rule, and chosen a sea-port town for its invasion. Mr. B concluded by denying that the contagionists formed any party; he came open to conviction; he came, not only willing, but anxious to believe in the non-contagious nature of the disease; but he could not shut his eyes to facts, nor resist the force of evidence. He would not follow truth so far, and so far only as she concurred with prejudice, but was ready to give up all preconceived opinions, and follow the path her unerring finger should point out, well knowing, that although both Socrates and Plato are worthy friends, that the better friend is truth.

MR. KING appealed to the society if his was not the true view of the matter. All diseases were contagious. Was not lues venerea a contagious disease? Very well. And typhus? and phthisis? If nobody rose to dispute it with him, he would take silence for consent (loud laughter). He was ready to prove it, if he was to stand there on his legs till three in the morning (cries of question). With regard to cholera, he had only to say that he was proud of agreeing with Dr. James Johnson: and denied that that gentleman and he had changed their opinions as somebody had stated (laughter.)

A member proposed that a resolution to that effect should be adopted by the society; but this was very properly overruled.

After much desultory conversation, which protracted the time considerably beyond the usual hour for breaking up, it was moved by Dr. J. Jonsson, and carried unanimously, that on next Saturday (this) evening, the Nature and Treatment of Cholera should be discussed.

NOTICES.

Mr. Addison's paper has been received, but is unavoidably postponed, owing to the great press of matter on the engrossing subject of cholera.

The communication of Dr. Baron came to hand in time, but it was impossible to comply with his request by inserting it this week.

The papers of Dr. Marshall Hall, Mr. Key, Dr. Campbell, (Edinburgh), Mr. Battley, Dr. Wilkinson, a Constant Reader, Philalethes, a Chemist and Druggist, Y. Z. Mr. Copland Hutchison, a Correspondent (whose name we cannot make out), dated *Helston*, Mr. Bennett, Mr. Spence, *Non-Nemo*, Mr. Ward, Mr. Knott, Dr. Badlam, Mr. Bushell, Mr. Crowdy, Mr. Slater, Mr. Green, Physician, Dr. Stanley, Mr. Lawrance, have been received.

A Pupil (Borough) next week.

Philomelides: we are sorry that at present we cannot.

THE LONDON MEDICAL GAZETTE,

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OF

Medicine and the Collateral Sciences.

SATURDAY, DECEMBER 3, 1831.

LECTURES

ON

THE THEORY AND PRACTICE OF
MEDICINE ;

Delivered at the London University,

By DR. ELLIOTSON.

PART I.—LECTURE IX.

Inflammation (continued).—Appearances of the Blood: Buffy Coat—Coagulation.—Terminations of Inflammation: Resolution—Effusion—Suppuration.

AFTER speaking, gentlemen, in the last lecture, on the general subject of inflammation, as shewn in the solids, I began to mention that certain changes are observed likewise in the blood. I merely entered upon the consideration of these changes.

I stated, that very frequently, when a part of the body is inflamed, the blood which is drawn from a vein does not exhibit the appearance which we observe in health, but that the red particles separate from the fibrin, so that the latter remains colourless on the top, forming what is called "the inflammatory buff, or buffy coat." I stated that this was sometimes drawn into an excavated form, and then it is said to be "cupped;" so that the blood may be buffed and cupped.

I mentioned, however, that though this is frequently observed, yet you may have the most violent inflammation without any such appearance as this in the blood. If you find such an appearance, and you suspect inflammation, you will certainly have so much the more reason to believe in its existence; but if you have tolerably sufficient evidence that inflammation is present, you are not to doubt its existence because when you draw blood you find it is not buffy.

Buffy Coat.

I now proceed to the farther consideration of this subject. The buffiness of the blood is not proportionate to the danger of the disease. I mentioned, that, in the case of mere pregnancy, you sometimes have buffi-

ness; but where inflammation is present and the buffiness likewise, the latter is not necessarily in proportion to the danger; for, in acute rheumatism confined to the joints, you will often find the blood in a high degree buffy and cupped, although there is not the slightest danger in the complaint.

Many particular circumstances are necessary to be observed in regard to this phenomenon. The first cup will often be buffy, when the second, taken within a minute or two, is much less so, or perhaps not at all. When this circumstance occurs, I believe the difference generally arises from the amendment of the patient; for the beneficial effect of bleeding is sometimes very striking—it sometimes immediately checks the inflammation, and one may conceive that a certain portion of benefit in such cases takes place before half the blood we intend to withdraw is removed; and when the second cup exhibits less buffiness than the first, we may ascribe it to the actual amendment of the patient. The fibrin which forms the buffy coat is found to be more abundant in inflamed than in healthy blood; and it is actually found—at least we are told so, I have no observations of my own upon the point—that the quantity of the fibrin varies in different cups taken from the patient at the same bleeding; that the blood of the first cup shall contain actually more fibrin than the blood of the second; that the bleeding actually induces a change in the blood itself; that not merely the buffiness, but the quantity of fibrin, whether it form buff or not, is found to vary in different cups; and that generally it is most abundant in the first cup.

Coagulation.

Before, however, I proceed to the consideration of the cause of this phenomenon—buffiness—I must say a few words upon the coagulation of the blood in general. It has been contended by some, that the formation of the coagulum is to be attributed to the death of the blood; but this I cannot assent to, and

for this reason—because a coagulum will often become organized; vessels will actually form in it, not merely shoot into it, but vessels will form in it, and unite with the surrounding healthy parts. I therefore cannot ascribe coagulation to the death of the blood. Sir Everard Home declares that he has seen vascularity in a coagulum when detached, when there were no vessels at all to shoot into it, but when, by its inherent vital power, it had produced vessels. I therefore cannot believe that the coagulation of blood is the effect of its death. John Hunter, on the other hand, ascribed the coagulation of blood to its life; I cannot, however, assent to that opinion either. Freezing is known to kill eggs; and that process must be supposed, therefore, to kill blood; but if you freeze blood and then thaw it, the moment it becomes liquid it coagulates, just as though it had never been frozen. You must suppose this blood to be actually dead, and its subsequent coagulation, therefore, cannot be ascribed to its life. Sir Charles Scudamore says, that when blood coagulates, a certain quantity of carbonic acid escapes; that the blood coagulates sooner accordingly as the circumstances in which it is placed favour the escape of carbonic acid; and he consequently ascribes the coagulation of blood to the escape of carbonic acid. Now it is a fact, that the more blood is exposed—if it be allowed to trickle down the arm instead of coming immediately into the receiver—the more it is exposed in that way, or by being received into a large shallow basin, which presents a greater surface to the atmosphere, instead of into a small vessel; or if it be received into a vessel calculated to retain the heat, and consequently not allowing the blood to cool so rapidly, and more especially if it be received into a vessel actually warm; it is said that, under all these circumstances, the escape of carbonic acid being favoured, it coagulates the sooner. This may be all true; but we have very high authority, as high authority as we can wish for—namely, that of our own Professor, Dr. Turner—for stating that no carbonic acid escapes from the blood during coagulation. When we consider, that, in the same dead body, we have a quantity of blood liquid in one part and coagulated in another—and nothing is more common than to find this the case in the different cavities of the heart, and in its different large vessels—I cannot ascribe the coagulation of blood either to its life or death, or indeed to any escape of carbonic acid; I therefore beg to say that I do not know why blood coagulates. I am not satisfied with the reasons that have been given, but I am not prepared to advance another reason instead of them; and it is always a great point gained to know one's ignorance.

The buffness of the blood is certainly affected by the slowness of the coagulation; so that if the blood do not flow freely at first,

but does afterwards, the quick coagulation of the first cup may not give the red particles time to subside, and this may prevent its being buffy. Hence you see that the quick or slow coagulation of blood must affect its buffness. As blood coagulates the sooner the more it is exposed, and therefore the more slowly it trickles down the arm, a thus increased quickness of coagulation may prevent blood from being buffy which otherwise would be so; and when it comes out more freely into the second cup, it may coagulate more slowly, and then, if there be a disposition to buff, the buff will form; so that it is not at all uncommon to have no buffness in the first cup, but to find it in the second. Therefore, although a patient may have improved from bleeding, and have a disposition to form less buff in the second cup than in the first, yet the accidental circumstance of its trickling down the arm into the first cup may give rise to an opposite occurrence, and may counterbalance the operation of the benefit. You may have buffness in the first cup and none in the second, from the improvement of the patient; or you may have no buffness in the first and find it in the second, notwithstanding the patient has improved, from the accidental circumstance of the second cup coagulating less speedily than the first.

But although the quickness and slowness of coagulation can affect the formation of buff, yet they are not the cause of it; for it has been known that, of two portions of the same blood, one has continued fluid ten minutes after the other had begun to form buff, and yet has shewn no buff. When the stream of blood is exactly the same, causing the blood to flow into cups with equal rapidity, you will sometimes find the difference of buff to be exceedingly great; and you will sometimes find the buffness to be the same when one cup has coagulated very slowly, and the other very quickly. Dr. Stokes has given us some experiments upon this point, in his *Pathological Observations*. He has furnished twenty-seven experiments, in fifteen of which buff was formed, and in the other twelve it was not. Among the twelve in which no buff was formed, no coagulation took place in three of them in less than from twenty to forty minutes; and in four there was no coagulation for eight minutes. There was abundance of time, through the slowness of coagulation, for the red particles to separate from the fibrin, and yet the result was as I have stated; whereas, in the fifteen in which buff was produced, not one specimen delayed to coagulate longer than fourteen minutes, and in all but three coagulations took place in five minutes; and yet buff was produced. The real cause of the buffness of inflammatory blood, therefore, certainly is not the slowness of the coagulation; but from what it proceeds, I do not know. It may be owing to the thinness of the fibrin; for it ap-

pears to be a fact, that the fibrin of buffy blood is both thinner and more abundant than that of other blood. If it be thinner, one may conceive that the red particles will more easily separate from it, and subside to the bottom. That may be the reason, but I do not say it is. As the quantity of fibrin is known to vary during the flow of blood, so probably may the thinness; and this may be the reason why blood which shewed buffiness in the first cup exhibits none in the second—the blood being thicker, the red particles do not so easily separate. However, the buffiness of the blood is not proportionate to the slowness of the coagulation. Dr. Stokes' experiments confirmed the opinion I had formed before from observation and reading, yet I believe it is true, that generally buffy blood coagulates more slowly than other blood, although I do not ascribe the buffiness to this circumstance; and the slowness of coagulation may be in a great measure owing to the thinness of the fibrin; if the fibrin be thin, it must probably be less easily disposed to solidify. It is said that the mere celerity of the circulation does not effect any changes in the quality of the blood; that it is not because the pulse is quick in inflammation that the quality of the blood changes; for Sir Charles Scudamore says, for example, that where there has been great celerity from pure fever or exercise, and blood taken away, no buff has been observed. This, however, has been denied by others.

Although, however, inflammatory blood shewing buff generally coagulates more slowly than other blood, yet the buff coagulates, if there be strength of body, very firmly—more firmly than the crassamentum in health. However, the firmness and the cup-like form are usually proportionate to the strength of the patient; but it is to be remembered, that the firmness of the coagulum is sometimes very great, without there being any cupped appearance. Certainly the firmness of the buffy coat is usually proportionate to the strength of the patient, and the firmness and cupped form are said to be greater, though I do not know that they are, in inflammation of fibrous membranes, such as aponeuroses, ligaments, and tendons, and in the inflammation of serous membranes, than of other parts.

If it so happen that the buff of the blood be not firm, then the blood is said to be *siety*; and when the coagulum of blood is soft, whether it be buffy or not, generally I believe you will find the serum too abundant;—at least when the blood is watery you will usually find the coagulum soft, whether it be a red coagulum or a buffy one. Two writers, Trail and Gendrin, one in our own country, and one abroad, say that the serum of inflammatory blood is altered in quality—that it contains twice as much albumen as is found in health.

The cupped appearance of the blood, in-

dependently of the buffiness, is very much affected by the size of the vessel into which you draw it. If you extract the blood of a patient into a tea-cup, you have an infinitely greater chance of finding it buffy than if you draw it into a hand-basin; the larger the vessel, the less is the disposition to form the buff, and likewise to form the cup appearance. For accurate observation blood should be drawn certainly into small vessels.

These are the gross changes, if I may so speak, in the blood; but some writers say that there are still more minute changes. Dr. Hastings says that, with a microscope, he found the blood in inflamed capillaries of a deeper colour than usual, as it were broken down, homogeneous, with no appearance of red particles. The remark does not apply to the general blood in the system, but to that of the inflamed vessels themselves. He says that in three experiments there were flocculi in the inflamed vessels, which disappeared as he directed his eye toward the healthy vessels. What these flocculi were, I do not know; whether they were exceedingly minute coagulations of lymph, formed from the slow motion of the blood—for it moves more slowly in an inflamed part—and which were re-dissolved when they reached the vessels where there was a proper motion, I cannot tell.

It is a general observation, that the blood is very black in the neighbourhood of inflamed parts; and when you apply leeches it is not till the latter end of their application, and perhaps not till after several applications, that it becomes bright. This is a fact: you will find that the blood which comes away first is generally very black, whereas the blood that streams afterwards will become brighter; but it will sometimes remain black till the second or third application of leeches. I have observed this frequently, and I presume it arises from this circumstance, that the blood, from moving slowly, acquires more of a venous character. I have known the blood, times innumerable, black on the first application of leeches, and I once knew the blood, in the case of chronic inflammation of the liver, highly offensive, so that almost as soon as it was removed it went into a state of putrefaction; but as leeches were applied day after day, it became brighter and brighter, and at last regained its natural odour and nature.

It would be wrong to consider inflammation as merely affecting the solids; it affects likewise the blood itself.

Terminations.

Now inflammation may terminate in complete health of the part—the part may in a direct manner regain its former state; and when this is the case, it is said to terminate in *resolution*. But generally when there is this resolution, there is some increased secretion, either at a distant part, or if the or of affected be of a secreting character part it

itself. If it be a serous membrane, there is more or less effusion; if it be a cellular membrane, there is a more or less oedematous condition, which you can observe externally. But if these things be very slight, only temporary, perhaps no inconvenience is produced, and the termination of inflammation is then said to be in "resolution." If, however, it be a serous membrane that is attacked, and there be a copious secretion, it is said to terminate in *effusion*. If it be a cellular membrane that has been inflamed, and it does not terminate in resolution, a new fluid is continually formed, called *pus*, and the formation of this new fluid is denominated *suppuration*. If it be a mucous membrane which has been affected, you generally have a great discharge either of mucus or pus, so that you may have suppuration either from a cellular or a mucous membrane. It is actually pus that is formed continually in inflammation of a mucous membrane; nay, you likewise have pus produced from the interior of a vessel. But it is right to say, that although authors call these "terminations," yet these copious secretions are not necessarily terminations, and that when they begin, it is not a matter of course that inflammation shall end; we ought rather to say, I think, that inflammation sooner or later induces effusion—induces suppuration, because the inflammation does not always cease when they take place. Although the part may suppurate, or fall into a state of effusion, still these processes may be inflammatory—you may still have to go on with anti-inflammatory measures—you may still have heat, pain, tenderness, and all the marks of inflammation. We ought, as I have just observed, rather to say that inflammation, sooner or later, induces these changes, or that these occurrences take place in inflamed parts, the inflammation subsiding or not, just as the case may be. Inflammation may terminate, however, not in health, directly or after these processes, but in *death*, either general or local. You may have inflammation destroy life without any farther change taking place than the occurrence of inflammation itself. It is very common to see persons die of enteritis, without mortification or any other change than inflammation. It sometimes happens, however, that inflammation does not destroy general life, but the life of the individual part, and then it is said to terminate in *mortification*. But the inflammation may still continue, not in the dead part, but in those around, and extend farther till more parts die, or not, just as the case may be.

Resolution.

I will now speak particularly of these various "terminations," as they are called; the first of which—resolution—is really a termination. Now when an inflammation is resolved, all the symptoms decline, and no fresh symptoms arise; and this is essential

to the character of resolution. In this state the urine generally deposits an abundant red sediment. When the inflammation is going on, the urine is generally high-coloured, but clear; but when the inflammation subsides, there is usually a copious red sediment, and the reason of this, as given by chemists, is the following. In healthy urine there is lithate of ammonia, which is pale and white, as also a yellow colouring matter, the nature of which is not known, but which is thought to be perhaps a modification of lithic acid. If to lithate of ammonia out of the body you add what are called the *purpurates*, you immediately produce a pink substance, such as you find in hectic fever; and if the urine have its usual yellow colouring matter, a mixture of that with the pink appearance produced by the purpurates and the lithate of ammonia, gives a red colour—pink and yellow mixed together form a red colour. Now in this diseased state of things, nitric acid, as chemists tell us, is formed in excess in the urine, which acts upon the lithic acid, and converts some of it into a new acid, called *purpuric*. This purpuric acid, uniting with the salts of the urine, makes the purpurates; and these purpurates, mixing with the lithate of ammonia, which is always in the urine, produce a pink deposit, which pink deposit, united with the yellow colouring matter, becomes red. The lithate of ammonia and soda are formed in some degree of excess in the urine under inflammation, and nitric acid is produced, which, as I have said, acting upon the lithic acid, produces purpuric acid, so that the purpurates are formed. These, mixing with lithate of ammonia, produce a pink substance; and when this unites with the yellow colouring matter, the colour is changed to red. This is the reason why the deposit is red. The reason why the urine is red without sediment till the inflammation declines is this—the same substances are in the urine, but held in solution during the activity of the complaint; but when it declines, an excess of lithic acid is formed, producing super-lithates, and these are very insoluble, and therefore a great portion of them falls down in the form of a precipitate; hence you have the red sediment. This is the explanation given by one who understands these things better than any other man perhaps in Europe—I mean Dr. Prout. You find these changes in the urine: the deep-red colour, with transparency, during the activity, and the copious red deposit during the termination of the disease; and you find these very same things from the most common cold, and certainly often in the same abundance.

Effusion.

If, however, the inflammation do not terminate in resolution, but produce such an effusion as from its quantity can do harm, then, when the symptoms of the inflammation

subside, or indeed whether they subside or not, you have a fresh set of symptoms come on, from the compression of the parts. For example, if the pleura have been inflamed, and the inflammation induces at last copious effusion, after the pleuritis has subsided, you have a fresh state of symptoms, dependent upon the compression of the lung. When there is this excessive effusion, however, in a serous membrane, there is generally something more—fibrin is usually thrown out, sometimes forming layers, sometimes diffused in the serum, and forming flocculi, larger or smaller, so that the serum is turbid, and not only turbid, but you may have flakes in it. When serum is thus poured out in a serous membrane, it is at last often entirely absorbed while the fibrin remains; and frequently we find fibrin where there is no appearance of serum, the serum having been absorbed, or fibrin alone having been produced. Fibrin, so poured out, has been seen in so short a time as four hours after the commencement of inflammation. Professor Thompson, of Edinburgh, says that he has seen lymph lying on a serous membrane within four hours after he had induced the inflammation. Sir Everard Home says, that he has known an effusion of fibrin form vascular adhesions in the course of twenty-four hours. So rapid are these changes, that it appears inflammation may cause an effusion of lymph in four hours; and that within twenty-four hours from the very first, this portion of fibrin may become an adhesion, containing blood-vessels. Sometimes the effusion which occurs is not serum, is not lymph, or serum and lymph, but is pus; so that you may have the pleura, the pericardium, or the tunica vaginalis, filled with pus, and sometimes you may have a mixture of pus and serum.

Suppuration.

If the third effect of inflammation take place—not resolution, not effusion, but suppuration, then the pain indeed generally ceases, but the swelling which was present during the inflammation still remains; and if there were not much swelling before, perhaps there is a good deal now. This swelling, however, does not, as when a part is inflamed, feel hard, but is for the most part soft, and frequently you may discover fluctuation early. Sometimes you cannot discover this at first, but as the fluid increases, you discover more and more, till the fluctuation is most decided. The pain, however, does not always go away; it may decline at first, but as the fluid increases, you may have fresh pain from tension of the part; the part may become exceedingly tense from the quantity of fluid, which afterwards is absorbed, or escapes either by art or nature. There may be very considerable pain. Besides this, just as in effusion of a serous membrane, where there is suppuration of a cellu-

lar membrane, or of any organ, you may have fresh symptoms from pressure. That, however, depends upon the situation of the part; if the part in which the pus collects be one which will suffer from compression, then you may, of course, have fresh symptoms from the mere compression. But besides this, if the suppuration be extensive, you have general shivering. As soon as the matter forms, or when it is forming, or soon after it, what are called *rigors* take place; and after a time you have great heat and sweating—in fact, you have what is called *hectic fever*.

Hectic Fever.—Now this hectic fever is marked by a quick, but weak pulse, by heat of the whole surface, but particularly heat with a red flush upon the cheeks, the palms of the hands, and the soles of the feet. Sometimes the whole of these are red, but more frequently the redness is partial, forms patches, so that a portion of the cheek, the size of half-a-crown, more or less, may be intensely red, while all around is of the usual pale character. In addition to these symptoms, the patient has profuse sweating, so that as soon as he falls asleep he is almost sure to fall into a sweat, and frequently when not asleep he still sweats. These symptoms are aggravated generally at night, and frequently also at noon, and indeed at any time when food is taken. It is remarkable to notice the effect of sleep upon the sweats. If a patient fall asleep but for two minutes, although he may not know it, it is very common to find these sweats break out, and they are frequently very partial, being observed particularly about the head and chest.

In this state of things, the sediment of the urine is not red, but pink. I should have said, that when it is red it is called *latteritious*, looking like a brick. In hectic fever, when the suppuration is going on, the sediment is of a pink colour. The chemical changes are precisely the same, the purpurates acting upon the lithates, and producing this pink colour; but this pink colour is not changed to red, because it appears that the yellow colouring matter of the urine is absent, and therefore the pink of the purpurates and lithates remains unchanged.

Frequently in this state of things, the intestines pour forth a fluid as abundantly as the skin, so that you have purging, and not unfrequently the purging alternates with the sweating, so that if you have an increase of the sweating, there is a decrease of the purging, and *vice versâ*. This sweating and this purging are called *colligative*; melting the patient down, and hence the name. The alimentary canal often becomes inflamed, and when you open a patient who has laboured under this state, you find the mucous membrane red, sometimes ulcerated, and the tongue is at length red: you may see it yellowish, or of a deep brown colour, but for the most part it

is red, either at the surface, or at the tip, or at the sides and tip. The mind generally becomes very quick, the patient is excessively alive to every thing, and, at the very last, a degree of delirium takes place; and, when that occurs, it is usually the precursor of a fatal end. I need not say there is usually extreme debility and wasting of the body.

It is to be remembered, however, that these symptoms, called hectic fever, may occur without any suppuration. They will occur, I know, from mere depression of mind; they will occur frequently in women from excessive suckling—from suckling too much, or suckling too long. All these symptoms, without suppuration, will sometimes remain for a great length of time, and the patient at last, by proper management, do very well.

Abscess.—If the pus, which is formed, be collected into a mass, we term the collection an *abscess*. Supposing the formation take place in a cellular membrane, fibrin is thrown out, and the cellular membrane, exterior to the fibrin, becomes more dense and more vascular. A cavity is formed in the lymph, containing the pus, and the exterior of the cavity is formed of the condensed cellular membrane. This lymph, forming the cavity, is gradually absorbed in one part, and that part is usually one which is nearest to the surface, or some exit of the body; so that if it be nearest the surface of the body, the portion nearest the surface is absorbed; or if it be near the intestines, the portion nearest the intestines is absorbed, and so on. Wherever there is the easiest exit for the contents, there the fibrin becomes absorbed. This is a very extraordinary and beautiful provision of nature. If there be a possibility of the pus escaping into a part where it might do harm, adhesions are formed all round that situation, so as to prevent the escape of the pus into it. Nothing can be more wonderful in pathology than this—than the process of nature in giving exit to the matter in a way the most calculated to facilitate, and in taking steps the best calculated to prevent mischief.

CLINICAL LECTURES,

Delivered in the Dispensary of the London University,

By DR. ANTHONY TODD THOMSON.

LECTURE VI.—Nov. 17, 1831.

Chorea—Melancholia.

GENTLEMEN,—The case of Chorea, which I noticed in my last lecture, is still upon our books. The violence of the symptoms abated after the bowels were effectually cleared, and the Carbonate of Iron had been taken for a few days; but, owing to the neglect of the mother of the patient, the bowels became confined, and two days ago the irregular movements increased to a greater

degree than when the girl was first brought to the dispensary. The use of the purgatives has been again resorted to; and, with the view of obtaining the effect of the Carbonate of Iron in its most efficient form, it was ordered to be prepared by adding sixty minims of the tincture of Muriate of Iron to a mixture consisting of one table spoonful of a solution of two ounces of Bicarbonate of Soda in a pint of water, a dessert spoonful of a saturated solution of sea Salt, and a third of a pint of water. By swallowing this dose at the moment of mixing it, two grains and a half of pure Carbonate of Iron are taken, in a state the best adapted for securing its influence upon the nerves of the stomach, and consequently for obtaining its tonic effect; and, if the statements of Mr. Phillips respecting the quantity of Protocarbonate of Iron in the Subcarbonate, usually met with, be correct, this dose is equivalent to sixty-five grains of that preparation. The chief object of the Muriate of Soda is to keep the bowels open under the influence of the Carbonate of Iron, without the daily use of more active cathartics. I will now, gentlemen, take advantage of the opportunity, which this case affords me, of making a few remarks on Chorea.

The condition of this patient, Margaret Brady, has enabled you to form an accurate idea of the most prominent symptoms of this curious disease; the jerking movements of the legs and arms, the constant restlessness, the twitchings of the face, the agitated, irregular movement, on raising the leg in the effort to walk, the difficulty of putting the foot firmly to the ground, the partial defect in articulation, and the almost entire want of controul over the voluntary muscles. These movements vary in almost every case of the disease; and, in some instances, exhibit the most singular anomalies. Thus, in one case related by Dr. Clarke, of Nottingham, and published in the fifth volume of the Edinburgh Medical and Surgical Journal, we find symptoms in several respects resembling those of Hydrophobia; for instance, the ordinary irregular movements were accompanied with a constant snapping motion of the lower jaw, which was sometimes so violent as to require the jaw to be held; there was great susceptibility of cold air on the surface, and a dread of swallowing liquids, owing to spasms being excited the moment they touched the fauces, with a catching of the breath and convulsive respiration. In another case, detailed by Mr. Kinder Wood, in the Medico-Chirurgical Transactions, the disease strongly resembled Tarentismus; the individual had an immoderate propensity to dance, and the movements kept time to a tune which dwelt in the mind of the patient. But such anomalies are rare, and in common the external symptoms of the disease are such as you have seen them in the case under treatment.

With these more obvious symptoms there is often a very variable, sometimes ravenous appetite, a fulness and hard state of the abdomen, and a constipated state of the bowels. The vivacity and playfulness natural to the age in which the disease most frequently occurs, is lost; and, if the attack be obstinate and long-continued, the eye loses its intelligence, and the countenance becomes pale, languid, and expressive of vacancy, or the approach of fatuity. In a few cases the involuntary movements have continued during sleep; in others, as in the case before us, they cease only at that time. You will find that some writers, for instance Darwin, assert, that the will can so far controul the movements, that the patient can lie still in bed if he please; but experience is directly at variance with this assertion; and in Brady's case, although the movements cease during sleep, yet, she cannot controul them in bed when she is awake.

The greater number of the cases of Chorea terminate favourably; nevertheless, in a few instances death has occurred, but only in those of greatly weakened constitutions, in which the continuance of the convulsions and the want of rest have actually worn out the strength. But if death seldom occur in cases of what may be termed *idiopathic Chorea*, it is almost always the result of that form of the disease which sometimes accompanies rheumatic fever; in which case, post mortem dissections have displayed inflammation of the spinal meninges, softening of the cord itself, or, as I have seen in two cases, following rheumatic metastasis, inflammation of the sheaths of the phrenic nerves, and those of the nerves of the cardiac plexus. The last-mentioned variety of Chorea has not, as far as memory serves me, been noted by any writer on the disease.

It is almost unnecessary, gentlemen, to inform you that Chorea is a disease connected with, or dependant on, a highly irritable condition of the nervous system. This nervous constitution, or temperament, displays itself by an unusual degree of sensibility and susceptibility of impression; so that circumstances which, in a healthy constitution, are indifferent, produce in this inordinate actions, with or without pain. As this state is apparently sometimes congenital, it may be reasonably inquired whether it is likely to depend upon any variety of organization in the brain and nerves, which would tend to regulate and modify ordinary impressions? or whether it is merely the result of a greater quantity of blood sent to the brain, and disposing it to be more excitable than when such an excess does not exist? In reply to these queries it may be stated, that we have no evidences of peculiar organization, or structure, in the brains of individuals of a highly nervous temperament who have suddenly died, and whose bodies have been examined; and that, although an increased impetus of blood to the head, short of the

extent which would produce actual disease, may augment this nervous temperament, yet we have no demonstrative evidence of the existence of such an impetus. The pulse is not stronger, the head is not hotter, nor is there any greater redness of countenance than in the more usual condition of the habit. But whatever may be the state of the brain in this diathesis, or nervous predisposition, it is undoubtedly such as to occasion a tendency to spasms and convulsions, from a degree of impetus less excessive than that which the ordinary state of the brain would require for their production. This, therefore, may be regarded as the predisposing cause of Chorea; and, under its influence, whatever circumstances augment the action of the heart and arteries, whether these be mental or corporeal, such an impression is made as to induce those irregular catching movements which constitute the most striking feature of this disease, and which occur chiefly while the muscles are thrown into action during the process of volition. It is certain that chorea is aggravated by whatever increases sanguineous action, and that it has occasionally followed blows, and other injuries of the head likely to induce a state of chronic inflammation in the brain; but it is also as certain that it has arisen from a very opposite condition of the system. In saying this, however, I must guard you against the opinion that it is always a disease of debility; there can be no greater mistake. Nor is it less erroneous to suppose that spasm is, in every instance, a symptom of a weakened state of habit. One of the most common exciting causes, acting upon the diathesis to which I have referred the disease, is local irritation in the bowels, which is afterwards sympathetically communicated to the whole system. This irritation may be set up by improper diet, by dentition, or by worms; and whichever of these is present, the effect is forcible in proportion to the greater original delicacy of frame of the patient; and it is always fostered by confinement and by sedentary occupations.

In referring to dentition as an exciting cause of Chorea, you must not direct your attention solely to the protrusion of the first set of teeth, as an equal degree of irritation may occur during the progress of the second. In general, the convulsions attending the first dentition resemble those of Epilepsy; whilst the consequence of the irritation of the second, is more frequently Chorea. Why is this? you may naturally inquire; but the question is more easily put than answered. With regard to worms, were these a common cause of irritation producing Chorea, we should find the disease more common than it is in adults; for although the period between childhood and puberty be that in which intestinal worms are generally found, yet these parasites are present in almost every period of life, whilst Chorea

most commonly makes its attack between the eighth and the fourteenth year. No exciting cause is more frequent than crude and indigestible diet; and, in the case now under consideration, it can be assigned to no other.

The irritation connected with the uterine function, when the change which takes place at puberty is retarded, is another exciting cause of Chorea in certain habits; and is another proof how much congestion is connected with its attack. In some instances, also, the disease has evidently originated from repelled eruptions. Mental emotions, likewise, act as exciting causes of Chorea; but in these cases we may generally trace a previous low state of habit, with much sallowness of complexion, and an evident derangement of the digestive function, which may be regarded as a state not only most likely to generate local irritation, but to increase the diathesis favourable to Chorea. In this state of habit, mental emotions operating on the brain induce irregular action in the liver; and thence, in its morbid secretion, a source of local irritation is set up, perfectly adequate to the production of the disease.

The last exciting cause of Chorea which I shall mention, is one to which I have already alluded—I mean metastasis of Rheumatic Inflammation, when the inflammation is transferred to the thoracic viscera. In those cases which have come under my observation, death ensued in both instances; and, consequently, an opportunity was afforded of examining the state of the nerves. Whether any similar condition of the sheath of nerves, in a less degree, occurs in ordinary cases of Chorea, it is impossible for me to determine.

Under the impression that Chorea is a disease of debility, a course of active purgatives may appear to you to be contra-indicated; and, as a general rule, I should say that it is so; and that, if you regard the state of the bowels, and the disordered condition of the secretions, as the sole exciting cause of Chorea, and treat all cases with purgatives, I may venture to assure you that your expectations will, undoubtedly, be disappointed: nothing, believe me, has more retarded the progress of the healing art than uncontrolled and indiscriminate generalization. In numerous cases, however, purgatives must be regarded as your principal resource; and you will find, that instead of reducing still more a frame already apparently too debilitated, the strength of the patient will revive, and the command over the muscles be gradually re-established and confirmed by the steady adherence to the purgative plan. In those cases in which the alvine discharges are black, and exhale a very offensive odour, you cannot err in the frequent repetition of active cathartics. This is well exemplified in the case before us; the stools were of an unna-

tural dirty hue, and foetid; the effect of the purgatives in lessening the irregular movements was obvious; and although these still continue, yet they are considerably less violent, and rapidly abating. Where purgatives are obviously indicated, and when the result of their employment is the rapid abatement of the involuntary movements, you will usually observe that this is in the direct ratio of the improved complexion of the alvine discharges, and that as soon as these become natural, the disease is cured.

The use of purgatives in Chorea was known to Sydenham, although his treatment of the disease was founded on the absurd opinion that it depended on a humour thrown upon the nerves: he bled, employed purgatives freely, and gave tonics to strengthen the nervous system*. Few cases admit of the use of the lancet; but set this aside, and the treatment of Sydenham is exactly that which you will find most successful; that is, by the combination of the purgative with the invigorating plan of treatment. The great confidence which I repose in the work of Dr. Hamilton on Purgatives, backed by the evidence of Dr. Parr and others, led me for some time to place my sole reliance on cathartics in the treatment of Chorea; but frequent disappointments have proved to me, as they will to you, the folly of confiding too much to any one description of remedies. So greatly, however, are we governed by authority, that I have seen purgatives successively prescribed when the bowels were open, and when the evacuations presented the most natural aspect. I cannot, gentlemen, too urgently caution you to avoid yielding up your judgments to any authority, however high: the first duty of a physician is to think for himself. It is not my intention, however, to affirm that you are not to continue the use of purgatives when the symptoms remain unabated, or nearly so, merely because the evacuations have assumed a healthy character; but surely that ought to be a sufficient reason for believing, that whatever may have been the original exciting cause of the involuntary movements, when alvine irritation no longer exists, it is time to look for some other cause to explain their continuance. Nothing is more difficult, in forming our opinions of the origin of diseases, than to distinguish between cause and effect: the state of the intestinal contents may in many instances be the exciting cause, in others it is as likely to be the effect of the disease. The kind of purgatives to be employed is also of some importance. The nature of the suspected irritation will in part guide us: but besides clearing effec-

* "Indicationes eurativas primùm ad humores illos tam venæ-sectione, quam purgatione minuendos; dein ad corroborandum genus nervosum omnino dirigendus censebam."

Sydenham, *Opera Universa*. p. 495.

tually the bowels, it is to be kept in memory that purgatives may serve also the purposes of counter-irritants; and when congestions in the head are suspected, this is of more importance than the sweeping out of the intestinal canal. The stimulant effect of Calomel and Jalap, by augmenting the action of abdominal vessels, pours much fluid into the bowels, and by this method the circulating system is not merely locally, but generally unloaded.

Unless, in cases of metastasis, which even when Chorea is not the result, require the use of the lancet, the part of Sydenham's practice regarding blood-letting is not to be followed. Purgings, as long as any cause of irritation exists in the intestinal canal, should be continued, and even beyond this period, if cerebral congestion be suspected; but other means of depletion have generally proved injurious. One exception, however, to this rule must not be overlooked: when there is reason to suspect injury to the head from a fall, or a blow, previous to the appearance of the disease, then we shall find much advantage from that degree of local depletion which is always obtained from the introduction of a seton into the nape of the neck, in chronic affections of the brain, when organic mischief is going on. A proof of the advantages of a seton, in such a condition of the brain, is well illustrated by the benefit derived from it in one of the cases of Epilepsy to which I directed your attention, in my last lecture.

If purgatives have often failed when solely depended upon in curing cases of Chorea, tonics have not proved more useful when they have been administered without previously clearing the *primæ viæ*, or when they have been trusted to alone. It is evident, however, that the best method of insuring the cure after the topical acrimony has been removed, is to invigorate the constitution; and with this view various tonics, according to the fashion of the day, for there is a fashion in medicine as well as in dress, have been employed. You perceive that I have chosen the Carbonate of Iron for this purpose; and although you will find in the works of Dr. Good and others, that the preparations of iron have been found too stimulant, yet my experience does not warrant me to coincide in this opinion. The chief objection which has been offered to the use of the Carbonate is the great uncertainty of its operation, arising from the peroxidization of the Oxide of Iron, formed from the Sulphate, in the usual manner of preparing the Carbonate; and the small quantity of the real Carbonate contained in a large dose of the nominal Carbonate. By prescribing the Carbonate in the fluid form, at the moment of its formation, and in combination with the Chloride of Sodium, the tonic influence of

the Chalybeate is procured in its highest degree, whilst the Chloride aids its effect by keeping open the bowels. Some practitioners are in the habit of ordering the black Oxide of Iron, as a mild tonic, in combination with Rhubarb; but the extrication of Sulphuretted Hydrogen Gas, by the action of the Oxide on the fluid contents of the intestines, is an objection to this form of exhibiting Iron in Chorea.

In some instances, antispasmodics, particularly Camphor and Musk, have succeeded in curing Chorea; but more reliance, under ordinary circumstances, is to be placed upon a judicious course of purgatives, followed by tonics.

Melancholia.

Another case, gentlemen, connected with the nervous system—one of *Melancholia*—is at present under treatment, and well worthy of your utmost attention. The subject of it, Ann Rooke, ætatis 29, of a dry, spare habit, pale, sallow complexion, large veins, and black hair, which, as you know, are the characteristics of what is termed the melancholic temperament, was admitted a patient of this Dispensary on the 12th of November. She stated that about four months ago she suffered depression of spirits from some family matters, and has not since felt in her usual state of mind. She labours under a constant dread of some unfortunate event befalling her; and, to use her own expressions, "is utterly uncomfortable in her mind." Two months ago, there was a little abatement of this mental uneasiness, but it soon returned, and she is now truly miserable. Her bowels are irregular, and her urine is high coloured. She feels a dizziness, as if she were about to fall, a constant desire to change her place, and an acute pain in her head, which is augmented when she stoops. Her appetite is ravenous. The only medicine which she had taken, before her application at the Dispensary, was a little Camomile tea. She was ordered, by the house surgeon, to take two spoonfuls of a mixture of equal parts of the Aloetic decoction and the *Mistura Ferri*, twice a day, with four grains of Blue pill and the same quantity of extract of Henbane, every night, at bed-time.

On the 15th the blue pill was omitted, and five grains of Camphor and three of Extract of Henbane substituted for it; and on the 16th I saw her for the first time. She then complained of a burning pain at the top of her head, accompanied with the utmost depression of spirits; her sleep being short, and interrupted with frightful dreams. The bowels were confined, requiring the daily aid of purgatives; but she had not examined the aspect of the stools. The Catamenia has never been suspended; but, at the last period, the discharge was very scanty. She

was ordered to take that evening an emetic, consisting of ℞j. of Ipecacuanha powder and gr. i. of Tartrate of Antimony and Potassa; and after its operation to commence the use of the following mixture.

R Tincturæ Digitalis, f. 3j.
 Tincturæ Hyosciami, f. 3iij.
 Misturæ Camphoræ, f. ʒviijss. M.
 Sumantur cochl. iij. majora, 8va
 quâque horâ.

To-day she says that she feels considerably more calm in her mind. The emetic brought a quantity of sour matter off the stomach, and also purged her; but she has had no motion since. The stools were dark-coloured and fœtid, and the urine high-coloured. Her sleep is still imperfect and dreamy; and yesterday, from eleven o'clock in the forenoon until night, she suffered the most acute pain across the eyes. The pulse is 84, and soft; the tongue furred, and covered with red papillæ; and she has an unpleasant taste constantly present in the mouth. A fluid drachm of the Tincture of Digitalis was ordered to be added to her mixture, and she was directed to take gr. xij. of the Colocynth pill every night at bedtime.

Such is the state of this patient. I shall not, at this time, enter upon any explanation of the symptoms, but merely mention that the Foxglove is given with a view to its narcotic, not its diuretic influence.

GENERAL DISPENSARY.

Disease of the Hip, combined with Disease of the Knee; with Clinical Remarks,

By MR. COULSON.

ESTHER KEELAND, aged 21, of a fair complexion and strong constitution, was admitted at the General Dispensary, under the care of Mr. Coulson, for an affection of the left knee and hip. About a month ago, whilst sitting at work, she was seized with pain darting through the knee, and a slight swelling on either side of the joint. Pain has continued in the knee ever since, being always worse towards evening. She did not complain of the slightest pain in the hip. Suspecting, however, that the hip was affected, Mr. C. made pressure in front of the joint and behind the trochanter major. This gave her great pain, and the pain extended down the outer and front part of the thigh to the inside of the knee and insertion of the ligament of the patella. The knee is also exceedingly painful, and a quarter of an inch larger in circumference than the right one. Pressure made on the knee-joint gives pain, which extends down the shin to the front of the foot. Rotation of the femur against the acetabulum, and flexion of the

knee, produce pain. The patient limps, and walks with the toes pointing to the ground and a little turned outwards. Feels a stiffness, weariness, and numbness in the whole limb, particularly of a morning.

C. C. ad 3xvj. coxæ sinistræ. Mist. Aper.
 p. r. n.

Immediately after the cupping, she felt the numbness subside and the pain greatly diminished. She was ordered to keep on her back, and to avoid all motion of the limb. The cupping was repeated twice, and a blister applied over the hip. At the end of a fortnight from her admission she was discharged quite cured.

November 18th.—To-day Mr. Coulson made the following observations on this case, and disease of the hip in general.

Gentlemen, as disease of the hip (by which is commonly meant disease of the bones entering into the formation of the hip-joint, more particularly of the head of the femur) not unfrequently comes before our notice, I may, perhaps, advantageously draw your attention to the symptoms and treatment of this complaint. The disease commences with a stiffness of the limb, and a sense of fatigue and weakness after the slightest exertion; occasional darting pains down the thigh, and generally a peculiar pain at the knee;—sometimes there is pain in the hip itself, but frequently it is altogether wanting. The pain in the knee is often the only symptom which at first attracts the attention of the patient, and induces him to apply to a surgeon; the pain is also generally more severe during the night. The surgeon, on examination, finds the knee in most cases sound, although not invariably so; but if he presses in the neighbourhood of the hip-joint, either behind the trochanter major or in front, where the psoas magnus and iliacus internus pass over the articulation; or if he grasps the foot and rotates the head of the femur against the acetabulum, he soon ascertains that the hip is the seat of the disease. The weakness and stiffness which the patient at first experiences are soon succeeded by a limping gait, or a slight degree of lameness. But all these symptoms (and even the pain in the knee) are at first so slight as to be entirely neglected even by adults; and in young children, who have not the power of communicating their sensations, limping or lameness is the first symptom which attracts the notice of the parent, and, when observed, is too often referred to some other cause than that from which it proceeds.

It thus happens that the disease may exist for some time, both in adults and children, without being discovered. How long it may remain in this early stage, is, of course, uncertain; depending on the constitution and age of the individual, as well as a variety of other causes. The usual time,

however, is from one to six months. If the complaint be not arrested at this period, it goes into the next stage, which is marked by symptoms too evident to be mistaken or overlooked. The affected limb will be generally longer than the sound one; the trochanter major is directed more outwards than in the natural state; the nates of the affected side are flatter, the folds of the skin considerably deeper, and the whole limb, particularly the thigh, is thinner and more flabby. The surgeon may convince himself of these changes by submitting his patient to an accurate examination. In the first place, let me advise you to place your patient in the horizontal position on a flat hard substance, as the table or floor, and not on any soft yielding substance, as a bed or sofa, &c. because their yielding nature will prevent you from seeing accurately the deviation from the natural form of the parts. When you have put the person in this position, you will, in most cases, perceive the trochanter, the patella, and malleolus, lower than in the other, or sound limb. Then grasp the foot of the affected limb in your hand, and, placing the other hand on the knee, in this manner rotate the head of the femur against the acetabulum. The pain which the patient experiences from this particular mode of examination will soon apprise you of the mischief going on in the joint. Pressure in front of the joint, and just behind the trochanter, will confirm you in your opinion of the seat of the disease. After you have examined the patient in the horizontal position, direct him to stand up, and you will see that he does not rest equally on both feet. The sound limb is extended, whilst the affected one is bent; the knee being lower than that of the opposite side, and the foot generally everted, though it is occasionally turned inwards; and the nates also are flattened. The lengthening of the limb in this stage of the disease, is the most prominent symptom. Now you will be surprised when I tell you that there is scarcely no point connected with disease of the hip which has given rise to more discussion than the explanation of this symptom, some contending that there is no real lengthening, whilst others assert that the limb is itself actually longer than the other. Those who contend for the latter opinion conceive that the limb is actually lengthened, either by the expansion of the head of the bone, or by an increased secretion of the synovial fluid, or by matter pushing the limb downwards. It is certainly difficult to understand this explanation, when one considers how accurately the head of the femur is adapted to the acetabulum, and how little extension the capsular ligament is capable of allowing. Those who contend (and, I think, contend correctly) that there is no real lengthening, say, that "the apparent elongation is produced by the

position of the pelvis being altered, in such a way that the crista of one ilium is visibly depressed below the level of that of the other. It is easy to understand how this effect is produced, by observing the position in which the patient places himself when he stands erect. He supports the weight of his body on the sound limb, the hip and the knee of which are, in consequence, maintained in the state of extension; at the same time the opposite limb is inclined forward, and the foot on the side of the disease is placed on the ground, considerably anterior to the other, not for the purpose of supporting the superincumbent weight, but for that of keeping the person steady, and preserving the equilibrium. Of course this cannot be done without the pelvis on the same side being depressed. The inclination of the pelvis is necessarily attended with a lateral curvature of the spine; and hence it happens that one shoulder is higher than the other, and that the whole figure is in some degree distorted*." Sometimes in this stage of the complaint there is no lengthening of the limb; and the absence of this symptom depends, according to Rust†, on the co-existence of inflammation and caries of the acetabulum with disease of the head of the femur, so that the bones composing it will be pushed by the superincumbent weight of the body from above, and the pressure of the head of the femur from below, and in this manner the lengthening of the limb will be prevented. The surgeon, Rust says, may discover this peculiarity, which is of very seldom occurrence, by an accurate comparison of the sound with the diseased limb. The ilium of the affected side is higher, and often painfully swollen, the body inclines more to the side, and the distance of the trochanter from the upper part of the ilium, greater than in the sound joint.

The patient does not remain long in this condition; the pain at the knee, which was before slight, becomes very severe, and impedes every motion of the limb, particularly that of extension. This circumstance, combined with the fact of the pain in the hip attracting but little or no attention, occasionally deceives the practitioner, and in almost every case misleads the patient, as to the real seat of the disease. It sometimes happens that the knee is affected at the same time with the hip, as I think was the case in Esther Ketland. Ford‡ says that the knee, after the most minute examination, will be found in a perfect sound state; but this is too general an assertion, and not correct. Mr. Brodie examined a case where one half of the joints of the body were diseased. The pain of the knee, however, almost invariably

* Vide Brodie on Diseases of the Joints.

† Arthrokakologie oder über die Verrenkungen durch innere Bedingung, &c. von J. N. Rust. Vienna, 1817. P. 36.

‡ Ford on the Disease of the Hip-joint, p. 13.

attends the early stages of disease of the hip, and different explanations have been offered to account for its occurrence. It is generally believed that the pain is conveyed by the branches of the anterior crural nerve, down the thigh to the knee; but Sir C. Bell conceives that it is communicated by means of the obturator nerve. "The obturator nerve, as you know," he says, "passes through the thyroid foramen, down to the hip-joint, and after supplying the muscles is distributed upon the inner part of the knee. The nerve in its course is thus involved in the inflammation which affects the hip-joint, and the pain is referred to its extreme cutaneous branches, at a part distant from the seat of the disease." This explanation is not of itself sufficient to account for the symptom, for we very commonly find the pain extending along the middle and even outer part of the thigh, whilst the obturator nerve is distributed to the muscles on the inner side of the limb. It has struck me that, from the intimate connexion of the long head of the rectus femoris with the outer edge of the acetabulum, and with the capsular ligament, this muscle may take on the inflammatory action, and the pain in this way be conveyed down the limb to the thigh. We find something analogous to this in diseases of the shoulder-joint; the pain in these cases extends down the front of the arm to the insertion of the biceps, and the long head of the biceps, as you well know, in intimate connexion with the capsular ligament of the joint, and the glenoid ligament of the scapula. I will go no further with these speculations, but merely observe, that in all probability all the causes assigned, and others not yet discovered, have their share in producing this symptom, and that it is not to be explained by any one alone.

If the disease be not arrested at this stage, a new and most formidable set of symptoms supervene; the elongated limb becomes gradually, or what is often the case, suddenly shorter, so that on examination it is found one, two, or more inches shorter than the sound limb. This symptom generally depends on dislocation of the femur on the ilium. The head of the bone, as well as the capsular ligament, have been probably destroyed, and there being nothing to counteract the action of the extensors, the femur is drawn upwards. The nates, which were flat, or even flabby, become rounded or prominent, and swollen, and the toes are turned inwards. Sometimes the shortening occurs without dislocation of the femur having taken place. A case of this kind occurred to me some time ago in this institution. A girl of the name of Dexter, who had been for a long time under my care, died with disease of the hip in the most advanced stage; there was shortening of the limb, prominence of the nates, inversion of

the toes, and abscesses in the neighbourhood of the joint: all these symptoms led me to conclude, that there was dislocation of the head of the femur; but, on carefully examining the joint after death, I found the capsular ligament entire, and no dislocation. The head of the bone, and a great part of the neck, had been destroyed by caries, and in this way the shortening was produced without dislocation. There is shortening of the limb from another cause, to which I must call your attention, and of which the following explanation has been given by Sir C. Bell, in his *Clinical Observations on Diseases of the Hip-Joint**:—"The muscles in their action press the tendons against the inflamed parts, and hence it is that the patient cannot bear to lie with his limbs stretched straight along the bed, for this produces a tension on the front part of the joint. He inclines the body, therefore, and raises the knee upwards to the belly, bending or doubling himself in the most easy posture. In this way he twists the spine and inclines the pelvis, drawing it obliquely upwards on the affected side." This apparent will be easily distinguished from the real shortening of the limb; you should place your patient even on his back, and then bring the pelvis into a straight position, when you will find the limbs of both sides of equal length. It sometimes happens that the symptoms which have just been enumerated, as characterizing this stage of the complaint, do not present themselves, but a lengthening of the limb takes place, in consequence of the head of the bone being drawn forwards, downwards, and inwards, into the foramen ovale. When this occurs, you will find the limb three or four inches longer than the other, the knee bent, and the foot turned outwards, with the toes pointing to the ground. A prominence will be felt in the region of the groin, from the subjacent head of the thigh bone. Cases of this kind are extremely rare; I have never seen an instance of it in the living subject. In the museum of the College of Surgeons, there is a preparation, presented, I believe, by Sir C. Blicke, in which the head of the femur was dislocated, from the effects of disease, into the foramen ovale. Mr. Tyrrell mentions, in his *Surgical Course*, a case of this kind, which came under his notice; and Mr. Hicks, of Emsworth, had a case of this nature under his care, of which the following is an outline:—Master S. at nine years, in 1826, laboured under a severe affection of the hip, accompanied with great constitutional irritation. Matter formed in front of the joint, for the evacuation of which an opening was made. The wound continued to discharge for a long time, and pieces of bone occasionally came away. In the middle of August 1829, the child had a fresh attack

* Vid. *Medical Gazette*, vol. i. p. 78.

of the disease of the joint. On the 8th of October, he was examined by Mr. Hicks, who found the limb much elongated, the knee and foot turned outwards, and the head of the femur near or into the foramen ovale. By counter-irritants, rest, and attention to the general health, the complaint in the hip was arrested, and the child restored to perfect health; the deformity, of course, remains. With such extensive mischief going on in the interior of the joint, you will expect to find the neighbouring parts participating in the disease. This, however, is not invariably the case; for, in some instances, the disease will go through all its stages, and even dislocation or ankylosis occur, without any external suppuration taking place. But, in the majority of cases, the glands in the groin become enlarged and painful; matter forms near to the joint, more frequently on its outer side; and this is indicated by the tumefaction, and great pain of the part and severe rigors with which the patient is affected. If the patient be a delicate scrofulous person, the constitution suffers considerably in the advanced stage of the disease; there is great prostration of strength and emaciation of the whole body, with night sweats and hectic fever, and he not unfrequently falls a victim to the complaint. If, on the other hand, the patient is comparatively strong, the constitution suffers but little, and he recovers with no other mark of the disease than the deformity. During the progress of the disease, the motions of the limb become more and more impaired, but the power which the patient acquires of using the dislocated limb, after the disease has subsided, is very great. There is a female residing in this neighbourhood whose thigh bones are both dislocated from disease, and who can nevertheless walk about very well, without any pain and inconvenience to herself.

At our next meeting, I shall continue the subject of Disease of the Hip, and make some observations on the case of *Hæmatocele*, on which I operated yesterday.

CHOLERA MORBUS OF INDIA.

To the Editor of the London Medical Gazette.

Harwich, Nov. 12, 1831.

SIR,

HAVING had an opportunity of becoming familiar with the cholera morbus, as it prevailed at Calcutta in the year 1818, and subsequently having had frequent occasions of seeing and treating this disease during several years service in Ceylon, I presume to offer some few remarks on this malady, which I trust at the present period will not be deemed

uninteresting, or unacceptable. I shall be as concise as possible.

That the disease which has been ravaging the continent of Europe is contagious, I think admits not of a question; the manner in which it has been imported into this country at Sunderland, is a proof of this fact, unequivocal, indeed, except to those who will not be convinced; and there is every reason to believe that the cholera of Europe is identical with the cholera of India, as it was known there in 1817, and during subsequent years. Now it appears to me, that if we admit the fact, that the disease of Europe and that of India are one and the same, we must of necessity come to the conclusion that it has been brought from Hindostan to Europe by means of contagion alone; for on no other grounds can we reasonably account for the progress of this disease through every variety of climate and temperature; for, like the small-pox, the heat of the dog-days of India, and the cold of a Russian winter, are equally favourable to its existence.

After careful deliberation on the subject, and having reviewed numerous facts which occurred under my own observation, I am led to hazard the opinion that the fatal epidemic of India, which first made its appearance in 1817, was a contagious disease. By means of this quality it has pervaded almost every part of India: it has traversed Europe from east to west, and finally arrived in our own country.

The cholera morbus has long been known in India—perhaps for centuries; and in certain spots, particularly those otherwise unhealthy, it frequently proved fatal, like the ordinary autumnal cholera of this country; but whether the disease ever before appeared as an epidemic, previous to 1817, is more questionable. I am at least certain that it was a generally received opinion in Ceylon that the disease was unknown as an epidemic on the western coast of that island, and all felt secure (fallaciously, as the event proved) that it would not reach that part of Asia: and when the disease first broke out in Bengal, the medical officers considered it novel in its character, as it was more general in its progress, and more fatal in its effects. Unfortunately—I must so speak—the question of its being contagious was never duly investigated: one and all at that time attributed its origin to atmos-

pheric influence. Had more trouble been taken, and more attention paid to this subject, I cannot help thinking that we should have arrived at a very different conclusion.

To substantiate the views which I have here advanced, I would beg to relate a few facts, chiefly as connected with the appearance and progress of the disease in Ceylon.

If we trace the disease from its birth at Jessore in 1817, we shall find that it travelled frequently in opposition to a continual strong current of wind—namely, the monsoons, in the course of human footsteps, and of commercial intercourse, till at length, in the beginning of the year 1819, it reached that part of the Coromandel coast which is opposite to the island of Ceylon, from whence it was carried over to that island, and first made its appearance at Jaffnapatnam, the nearest town to the great continent of Hindostan, and the capital of a large commercial district, thickly inhabited. From Jaffnapatnam it was carried to Trincomalee, and getting on board the shipping, there is little reason to doubt but that through this means it was conveyed to the Mauritius. But to return: from Jaffna we also find it spreading southward to Colombo, the seat of government; and from thence eastward to Kandy, the old capital of the late King's dominions in Ceylon. In its progress to these places, it still observed the same laws, shewing itself only where human intercourse was constant and frequent, and where it had a succession of subjects for its propagation. At the period when the disease broke out in these towns, I was stationed at Korunegalle, distant from Colombo about sixty miles north-east, and from Kandy about thirty miles north-west. Korunegalle is the chief place in the Seven Korles, a province of from seventy to eighty miles long, and from thirty to forty miles wide, containing a population of eighty thousand souls, and garrisoned by about one hundred and fifty European, and from two hundred to three hundred (including pioneers) native troops, and is near the southern limits of the province. Within this district not a single case of cholera occurred to my knowledge, notwithstanding the disease raged along the whole line of its south and western boundary, on which human intercourse was frequent and constant, but we had little or no communication with either

Colombo or Kandy, and to that cause I now must attribute the circumstance of our escaping this complaint; separated as we were from our neighbours, by a broad belt of jungle on the west, and by a mountainous district on the south. One death only was reported to me during the prevalence of the cholera in the adjacent district. The individual was a native, and the symptoms, as related to me, were those of this disease. This man died at a place about twenty miles from Korunegalle, on the road to Colombo. The disease was at its height at Colombo during the month of April 1819, and was mostly confined to one company of the 83d regiment, forming the garrison. This company occupied a barrack near the main gate leading out of the fort into the Pettah, or native town, and separate from the barracks occupied by the rest of the regiment. The barracks at Colombo are small houses, in detached parts of the fort, each capable of containing one company only.

I could bring forward many more facts tending to confirm my argument, but the subject has been publicly and ably discussed: these which I have related are strong in favour of the opinion of the contagionists; for my own part, believing that the spasmodic cholera of India, which has now spread over nearly the whole of Europe, is a disease which, in its mode of propagation, possesses all the properties of a contagion, to maintain the contrary is wilfully to abandon the maxims of prudence, and to remain blind to the dictates of common sense.

In several cases of recovery from Indian cholera, which occurred under my own observation, I noticed that a favourable crisis in the disease was marked by the coming on of a profuse perspiration, the sweat literally pouring off the patients in streams; but I am not prepared to say whether this occurrence was only an accidental circumstance, depending upon the agency of a high degree of temperature operating on European constitutions, or whether it was one of nature's efforts for throwing off the disease. The knowledge of the fact may be useful in two ways—in directing us in the employment of remedies, and in devising the means of preservation from the disease; and explains the reason how the wearing of flannel next the skin, which was long believed

in India to be an effectual preservative against the cholera, acts in keeping off the attack of this malady. Dr. M'Whirter, who was presidency surgeon at Calcutta, and physician to the Marquis of Hastings, enjoying a most extensive private practice, first mentioned to me this fact, when the cholera at Calcutta was on the decline; stating that he had not known, up to that period, a single death to occur from the disease amongst those who had constantly worn flannel next the skin, as an article of dress; and my own observations corroborated this opinion. A different result may have attended the experience of others; but I am inclined to believe that this simple means is one of the best preservatives from this dreadful disorder.

Next to the general adoption of flannel clothing, it is desirable that the breakfast of light food should be taken immediately after rising, and before exposure to the cold morning air. We know that this is recommended as a preservative from all kinds of contagious disorders, particularly fevers of a typhoid character; and as I have known many individuals who, in India, habitually took a cup of coffee immediately on rising from bed, and who enjoyed an immunity from the cholera, I think the example well worthy of consideration in this country. I would beg to suggest that the dragoons of the army should each have a cup of coffee and a slice of bread each morning before going to early stable-duty; and I think the best effect would result. The expense would be very trifling, and, at all events, it would lessen their opportunities of indulging in intemperance, by disposing of a portion of their daily pay, which would otherwise be squandered in liquor, or some other vicious means of indulgence. Temperance cannot, at this crisis, be too strongly enjoined, nor cleanliness and ventilation too strictly enforced. Great attention should be paid to the soldiers' messing, and in their duties great discretion should be observed. Unnecessary exposure to cold and wet should, as much as possible, be avoided; and when unavoidable, the means of an early change of wet for dry clothes should, if possible, be afforded, and its necessity urgently enforced.

The high state of discipline which our army has arrived at; the care and attention of its superior officers; and the means of a strict quarantine, which

every regiment in the service possesses in a particularly favourable degree; are, I trust, sufficient to lead us to hope, that this disease will not prove so dreadful a scourge amongst our soldiers as there is but too much reason to believe it has been on the continent of Europe, particularly amongst the lower classes of society.

In presenting these remarks, hastily put together, I am actuated only by a wish to contribute to the public good, and, with this apology,

I remain, sir,

Your most obedient, humble servant,
WILLIAM KNOTT,
Surgeon, 6th Dragoons.

PATHOLOGY OF CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

M. DE JONNES, whose very interesting work on cholera is referred to in the last number of the *Quarterly Review*, gives the following summary of the results of his observations on the pathology of that disease.

As his views are clear, and his inquiries have been laborious, you may perhaps think this extract worthy of a place in your miscellany.

I am, sir,

Your very obedient servant,
C. W. CROWDY.

Brixton Hill, Nov. 16, 1831.

1st. This malady has many symptoms in common with the cholera morbus of our climates, and it has thence derived its name; but it has others peculiar to itself, and which, joined to the mode of its propagation, and the violence and rapidity of its effects, constitute a malady *sui generis*, and one of the most disastrous recorded in the history of the world.

2d. Its characters are perfectly similar, or analogous, at points two thousand leagues distant, and in countries situated under the equator, or near the polar circle—in the interior of a continent, or on the sea-shore—at the level of the ocean, or in the mid region of the atmosphere.

3d. Contrary to epidemic diseases, which depend on heat, humidity, and marsh exhalations, and appear at particular seasons, this manifests itself at all

times, although heat is most favourable to its propagation.

4th. Its symptoms are regular, successive, everywhere similar, whilst those of epidemics vary according to the energy of the agents which give rise to them, and are constantly differing in intensity, form, and rapidity, sometimes even exhibiting phenomena which constitute another species of malady.

5th. Its principal symptoms are vomitings and purgings of a fluid kind, and in prodigious quantity; cramps and violent spasms of the extremities, great pain of the epigastrium, inflammation of the stomach and intestines,—symptoms greatly resembling those of poisoning.

6th. The principle of the cholera is the same in Europe, Africa, and Asia, since it produces every where the same series of external symptoms and internal lesions—in short, the same disease, since it similarly attacks every where all persons, whatever their sex, age, or race, and since it is neither modified by difference of place, time, or individuals.

7th. At the commencement, in the middle, and at the end of each irruption, it has the same degree of virulence, since it produces the same symptoms, and destroys those attacked by it with equal rapidity and violence. Its decline is shewn only by diminished power of propagation.

8th. Its germ is the most quickly active of all the varieties of contagion, for the deadly effect is sometimes almost immediate, though generally forty-eight hours intervene between the period of infection and the appearance of the first symptoms. We know that yellow fever may remain latent twenty days, small-pox sixteen, the plague thirty, and even more, hydrophobia three months and a half, &c.

9th. It has not yet been ascertained by observation, to what period we must limit the giving out of the morbid material, whether concrete or in vapour, which produces the contagion. From the rapidity of the malady this period must be very short, but it is probable that it commences with the appearance of the first symptoms.

10th. This rapidity of the phenomena of cholera constitutes this malady an *acute contagion*; whilst the plague, yellow fever, small-pox, measles, hydrophobia, also leprosy, (lespians*?) sy-

philis, the itch, may be considered chronic contagions.

11th. Like the maladies of the first of these classes, cholera generally attacks but once the same individual; the contrary, at least, is rare, and has not been well established. This immunity of persons, who have been once infected, from contagions to which they are exposed afresh, seems to result from an alteration in the absorbent system, which, in consequence of these maladies, becomes less susceptible, or ceases to be so altogether. A similar result is produced by living in prisons, or hospitals, or the habitual use of certain poisonous substances.

12th. The degree of aptitude for receiving this malady differs infinitely according to constitution, age, sex, diet, morals, circumstances of life, which may increase or diminish, as their effects may be permanent or transient, the absorbent power of the organic tissues, with which the germ of the contagion comes in contact.

13th. From these physiological differences it happens that, out of twenty persons exposed to the cholera, one only may receive the infection. It has also been ascertained from numerous observations, that out of twenty-five individuals bitten by mad dogs, one only became hydrophobic.

14th. It follows also, that there are more chances of escape from this malady for women and children than for men, for those who are weak than for the strong, during winter than in the summer, and especially with courage and resignation rather than being under the influence of dejection and fear.

15th. We are entirely ignorant whether the germ of the malady is introduced into the body by cutaneous or pulmonary absorption, or by the organs of nutrition. Anatomical examination seems to indicate the latter channel, since it demonstrates the seat of the cholera as in the stomach and bowels; but, on the other hand, from seeing the disease propagated with unheard-of rapidity among the population of India, who are unclothed, it would seem probable that the malady is received through the medium of the skin. However, observers have admitted as a preferable conjecture, that the germ of the cholera exists in gaseous emanations from the bodies of the sick, and that, consequently,

* Probably les piens, see Aliberl, &c.—ED. GAZ.

the respiration is the channel through which it is transmitted.

16th. It is probable that this morbid gerin, which re-produces itself in the human body by the assimilative action of the vital powers, acts at first with more or less violence according to its concentration and peculiar energy, which, perhaps, is dependent on its specific quantity.

17th. No circumstance warrants the supposition that it can be transmitted, in the open air, beyond the distance of some feet; and it is certain that there is no foundation for the assertion, that it can be transported from place to place by the currents of the atmosphere.

18th. But in places where the air is stagnant, such as the mid-decks of a ship, the wards of most hospitals, in barracks, the interior of houses, especially in large towns, the germs of the cholera accumulate, become attached to persons and things, and propagate the disease through the one mode or the other.

19th. The cholera breaks out wherever its germs are carried, which is the characteristic of contagious maladies; whilst epidemics appear only in certain localities, or certain countries, where their primitive causes exist.

20th. Finally, the origin and intimate nature of these germs are entirely unknown, as are those of the contagions which have been spread over Europe from time immemorial, and which daily occur to our observation. Experience and study have taught us nothing concerning them, and they must be ranked among those mysteries of nature which science cannot develop.

CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

I SUBMIT the following considerations to your decision, whether or not they are worthy a place in your useful and excellent journal: I have been led to them from seeing the daily publication of articles about the contagious or non-contagious nature of cholera, which are in many instances quite vague, and even contradictory, in consequence of no definite and universally-received meaning being attached to the term *contagion*;

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scarcely any two writers, probably, taking precisely the same view of, or attaching the same meaning to the word. Never has this, I think, been more strikingly manifested, than in the report circulated through the daily papers of the recent discussion in the Westminster Medical Society. I defy any one to glean from it any information on the debated point; and how should they, since it is evident that different views were taken by the different speakers, of the meaning of the term which they wished to attach to, or sever from, the disease in question? One doctor denies *in toto* that the disease is contagious, but thinks "that it may be *communicated* under certain circumstances." In the published statements of Drs. Russell and Barry, these gentlemen consider "that the germs of the disease were diffused, and the disease propagated, in two ways, one of which may be called *personal*, by the dispersion over the whole city of several thousand passengers and boatmen, who had come from infected places, or been exposed to infection on the passage or on board these vessels; the other, which may be termed *atmospheric*, by emanations from the barques and their contents, suspended in and carried by currents of air to susceptible persons, *independent of direct communication*." It appears to me that here a distinction is made where no difference exists. The first way mentioned, in which the disease was propagated, was as much atmospheric as the other; the two ways were, in fact, one and the same; for from whence did the "persons" who communicated the disease in the first way, obtain its germs? and whence did "the barques and their contents," which communicated it in the second way, obtain their germs? both of them, of course, from individuals who had laboured under the disease. And was it not as much through the atmosphere that the poison was conveyed from the crew and passengers of the vessels, as from the vessels themselves and their contents. It was likewise equally "independent of direct communication" in the one case as the other, for there was the intermedium of the crew and passengers (excepting the individuals among them who actually had the disease) on the one hand, and that of the vessels and contents on the other. Both received the infection from diseased persons, and

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both communicated it through the medium of the atmosphere.

The terms contagion and infection are now, for the most part, used indiscriminately; the latter, in fact, being seldom employed. This I deprecate, for I think the distinction is useful; and the one, which I have adopted, appears to me the most classical, and least likely to occasion confusion. Whether, however, this be conceded or not, it will not affect the utility of these observations.

I would understand, then, first, by "*a contagious disease*," one which *requires*, for its propagation, that some part of the diseased person should, in one way or another, come into *actual contact* with some part of the healthy person. Examples of this are, syphilis, the itch, and other cutaneous diseases. Perhaps, hydrophobia may be ranked among them. By "*an infectious disease*," I mean one which is propagated through the medium of the atmosphere conveying exhalations, *whether directly or remotely*, whether more or less diluted, from a person or persons while labouring under that disease. I adduce some as certain, others as doubtful, examples of such diseases; doubtful, because medical men differ as to the way in which they may be propagated. The *certain* are, small-pox, measles, scarlatina, hooping-cough, the plague, &c.; the *doubtful*, typhus fever, cholera morbus, puerperal fever, erysipelas, &c.

A sporadic disease is one whose attacks are confined to one or to a few individuals, at any particular time or season; being independent of infection, and arising, for the most part, from accidental circumstances, such as exposure to cold, damp, &c. and may be instanced by a person suffering inflammation of the lungs, bowels, &c. from this cause. Under this class, I think we frequently find the diseases which I have placed as "*doubtful*" among the infectious diseases.

An epidemic disease is one which is propagated through the medium of the atmosphere, independently of the sources of infectious diseases, or of marsh miasmata.

The peculiarities attending diseases arising from marsh miasm, and other epidemics, I need not particularise.

Connected with these definitions, I would just offer one or two remarks, some of which have been recently sug-

gested to your readers in an article from Mr. Barham, of Exeter.

There seems to be no reason why two of the above sources of disease may not operate together. Suppose, for example, a sporadic case of typhus fever; as such, *per se*, not infectious: but let it occur in a thickly-populated and ill-ventilated part of a large town, with other circumstances to aggravate the disease, may not a specific poison be thus generated, which shall infect the air, and through it communicate the same disease to others?

The same may be asked with respect to an epidemic. May not a disease, which is originally an epidemic, and in its general course follows the laws of an epidemic, occurring under similar circumstances with those just supposed, be furnished with additional power by a specific animal effluvium, and thus have infection added to its means of being propagated?

With respect to cholera, it seems not impossible that the latter may be the way in which it is propagated, though I confess I feel little doubt myself that it follows exclusively the laws of infection.

One word with respect to quarantine. Though I should strenuously advocate its adoption, I am by no means prepared to allow that, (supposing the disease to be strictly infectious,) its most rigid observance would *certainly* keep the enemy at bay, if, as I see no reason to doubt, the infected air is capable of extending to any considerable distance. Allowing this, it no longer will remain matter of surprise to the advocate for infection, that this malignant disease should in some instances have spread in spite of quarantine regulations.—I remain, sir,

Your obedient, humble servant,

CHARLES P. SLATER.

Bath, Nov. 15, 1831.

CHOLERA—IS IT CONTAGIOUS?

By DAVID BADHAM, M.B. OXON.

Glasgow College, Tuesday, Nov. 15, 1831.

THE numerous facts that have been adduced by the contagionist party on the one hand, and the non-contagionists on the other, are of such a conclusive kind

in either case as to leave no doubt upon the minds of many that cholera both is and is not an infectious disease. Nothing renders this more probable to my mind than the mutual retraction of opinion that has prevailed among several medical men of observation and experience who, starting with the conviction of its contagious or non-contagious character, have subsequently seen reason for adopting a conclusion the very opposite of that with which they had set out. Now if we admit that both parties are correct in the statement of facts, how can such apparent differences in opinion be adjusted? or admitting all the statements indifferently as facts, to what circumstances is it attributable that cholera spreads in some instances with such fearful velocity from person to person, while at others it appears scarcely to take the character of a contagious disease?

The only way I can see of reconciling the dissension which at present prevails respecting the mode of propagation of this disease, is to suppose, that when cholera first makes its appearance, while it occurs as yet but in a few sporadic cases, and in different parts of a town or district, it is highly contagious and speedily diffusive, because the full effects of its noxious influence fall suddenly upon untried constitutions; but when the distemper is become epidemic, when the fomites of contagion have become very extensively multiplied, and when the very atmosphere has become charged with seeds of the disease, then it is that those who have hitherto escaped the contagion are on that very account exempted in a measure from the risk which had otherwise attended a nearer approximation to the infected; as the beginning with small doses of any poison will enable a person eventually to receive into his system even a considerable quantity with impunity. Hence the immunity of many in infected towns, whose susceptibility to the disease gradually decreases, while the air around them is continually deteriorating, till at last they can approach with impunity to the bed-side of the infected with comparatively little fear or danger. But this immunity, however general it may become from the above and other causes, does not prove the disease to be non-contagious; and, indeed, if healthy individuals with untried constitutions, and who had been living in a purer air, were

suddenly brought into close proximity with the disease, they would in all probability imbibe the contagion, and disseminate it to others similarly circumstanced to what they had been previous to exposure.

NOTE.—It is usually stated that the miasm of contagion is confined to a very limited sphere of action, varying in different diseases, but rarely extending many feet beyond the individual infected. I have, however, ventured to assume that such miasm may be greatly diffused, so as even to affect the whole atmosphere of a town, and in proportion to the malignity with which the epidemic may be raging. This appears to me probable—1, from one very presumable quality of such infecting agent which, as it must be extremely subtle, must be proportionably liable to almost indefinite dispersion, for even solid matter is almost indefinitely diffusible if it be diffusible at all in fluids: hence the operation of the finest chemical tests. 2. From the ascertained quality of a capacity in such infectious agents, that what can be concentrated till it acquires the fatal activity almost of prussic acid, may also be diluted down to harmlessness. Now the sudden deaths that have occurred during pestilences to persons in the act of unpacking merchandise, are generally admitted to arise from the over-powering activity of the concentrated form in which the miasm has made its attack. 3. Lastly, why but for this reason are persons who live in an infected town subject, as it has been remarked, to unwonted general indisposition of an indefinite character, and often so considerable and of such a kind as to need but a slight aggravation of symptoms to bring on the very epidemic that is prevailing? I conceive this to arise, because the air is to a certain extent corrupted by the diffusion of the epidemic poison, though not in all cases sufficiently so to induce the epidemic itself, but, in place of it, engendering other complaints, whose type and severity is proportioned to the susceptibility of the individual, and the dilution, so to speak, of the poison.

CHOLERA.

To the Editor of the London Medical Gazette.

Brighton, Nov. 22, 1831.

SIR,

IN consequence of Dr. Copland's allusion to my remarks on cholera, which you were pleased to insert in number 206 of your Gazette, I think it necessary to state that I did not see his article in the Foreign Quarterly Review, but that I am aware part of the treatment I proposed had been previously recommended.

The following queries having offered themselves to me, I beg leave to subjoin them:—

1. The depressing effects of the south-east wind (the scirocco of Italy) which have, probably correctly, been attributed to some electrical peculiarity, being more or less experienced in every climate, is there any connexion to be traced between this circumstance and the fact that pestilential cholera, comparing the point from whence it started with its present station, has upon the whole pursued a north-west track, accounting for the numerous deviations from it by lateral propagation by means of the various vehicles of infection?

2. Are there any data which tend either to overturn or to support the hypothesis that this epidemic is caused by a change in the electrical constitution of the atmosphere; that the expression used by some, comparing the state of the patient to that of an "exhausted Leyden jar," possibly may convey an important truth, and that electricity or galvanism, judiciously employed, may prove a most useful remedy? Galvanism would, moreover, seem suited, from the relation which exists between it and the nervous, especially the secreting power, and from the restoration of the secretions, particularly that of the kidneys, being of the first importance.

To revert once more to the question of contagion. Though we must concede to the non-contagionists, that this epidemic, as well as lues, plague, &c. not being coeval with man, the first case of those diseases which ever occurred *must* have been sporadic, and have arisen from other causes than contagion; and if this happened once, it may again at any time: still I do not see how any

one can deny that this and many other diseases may, under certain circumstances, become infectious, or doubt, at this time of day, that cholera has long since assumed that character, and at present is chiefly propagated by contagion. Since, however, ocular proof of actual transmission from one individual to another cannot be procured and conviction cannot be forced on any one, the discussion of this subject will now, I hope, give way to topics more immediately connected with the treatment of the disease.

By inserting the above, and noticing the errata* at foot, you will oblige me.

I remain, sir,

Your most obedient servant,

E. STANLEY, M.D.

ORIGIN OF THE VARIOLÆ VACCINÆ.

To the Editor of the London Medical Gazette.

SIR,

I TRUST to your sense of justice to give insertion to the following statement. All who have read your number for November the 5th, will immediately perceive the object I have in view. As my design is to be brief, I will deliver what I at present have to say in as few words as possible.

The history of the origin of the variolæ vaccinæ, divested of all ambiguity and confusion, stands thus. Dr. Jenner found that a disorder caught from horses and cows in the dairies of Gloucestershire, prevented persons so affected from being liable to small-pox. After long and patient, and successful investigation, he came to the conclusion that the disease so caught was a mild variety of the variolæ which attacked men. This belief afforded him the means of unraveling all the difficulties connected with the practice of the cow-pox inoculation, and ultimately to present to the world the most comprehensive and perfect specimen of medical investigation that has ever been witnessed. I say this without fear of contradiction, inasmuch as scarcely one fact connected

* In No. 206, p. 176, line 21, for "embrocations," read "liniments;" line 28, for "counter-indicated," read "contra-indicated;" line 30, for "venæ," read "vena;" line 40, for "oxide," read "oxyde."

with the nature or modifications of the disease has been recorded of which he was not aware.

His opinions concerning the origin of the disease necessarily rested upon a different foundation, and could not, therefore, be so conclusive as those which were based on fact and observation. He conceived that the variolæ, in all their forms, were ultimately to be traced to the horse. I need not remind your readers how much he was ridiculed for this and many of his other doctrines, but there was a foundation for them that they little dreamt of.

Matters continued in this state of uncertainty up to the period of his death, with this additional difficulty, that there was an increasing unwillingness in the minds of medical men to subscribe to many of Dr. Jenner's most important positions. Since that period, the great problem touching the origin of the variolæ vaccinæ has been solved, and the soundness of Dr. Jenner's views has received the most signal corroboration.

I am not aware that the facts that I am about to state, were ever before the period alluded to brought to bear on the origin of cow-pox, and on the identity of that disease with small-pox; but be that as it may, it has been incontestibly shewn.

First, that cows in many parts of Europe have long been known to be liable to *small-pox*.

Secondly, that this disease has spread from flock to flock in the most fatal and pestilential manner.

Thirdly, that it raged in England not much more than fifty years ago, and that the practice of inoculation was had recourse to, to subdue its violence.

Fourthly, that it is even now occasionally a violent and fatal disease among the cows, and for the most part is propagated by contagion through the whole herd, when it breaks out in a dairy.

With the exception of the last fact, I believe every one of the above-named particulars were *hid* from the profession till within these four years. At least I do not know that the slightest reference has been made to them by any writer anterior to that time, in order to demonstrate the nature and origin of the variolæ vaccinæ. Their application to this subject is clear and conclusive. Dr. Jenner finds the remains of a more vio-

lent epizootic small-pox in his neighbourhood. Without being aware of the actual history of this malady in the inferior animals, he investigates its nature with matchless success, communicates it by inoculation to man, and affirms that the disease thus communicated will afford protection against small-pox as complete as that disease itself does.

What was the ground of this confidence?—*His conviction of the identity of small-pox and cow-pox*. On what was that conviction founded?—*Analogy, and the ascertained peculiarities of the disease*. How has this conviction been strengthened and demonstrated?—*By tracing the small-pox in cows for hundreds of years*, and affording the strongest presumptive evidence that the disease was sometimes communicated to man, in its most fatal form, as it has likewise been done in the mildest form!

It is very painful for me to be compelled thus to address you, and still more that I should find it necessary at this late period to refer to a work of my own for proofs of the accuracy of these statements. The interesting fact recorded by Dr. Sonderland is demonstrative of the truth of the conclusions which have been mentioned, though some of his inferences are at variance with the best historical evidence.

I know that when an author complains he is generally supposed to be impelled by over-weening conceit or mortified self-love. I am not, I trust, one of that class. I value truth and justice above all things; and provided they are duly upheld, all personal feelings would instantly give way. On this occasion I am satisfied that I am not seeking too much from you, or from your contemporary who first published the document to which this letter refers, when I beg that you will honestly and faithfully compare it with my published work; and having done so, I would ask you, or any competent judge, whether it was right and becoming to withhold all the evidence which that work contains?—I am, sir,

—Your obedient humble servant,
JOHN BARON.

Gloucester, Nov. 21, 1831.

SMALL-POX.

To the Editor of the London Medical Gazette.

SIR,

THE spirit of your Journal seems so fair, and your desire to maintain the truth is so manifest, that I feel assured you will permit me to make a few remarks upon a paper that appeared in your last number, although it has been avowedly taken from the publication of a contemporary.

I believe it to be a mistake to affirm that the variolæ vaccinae have originated in the manner referred to in the seventh aphorism of Doctor Sonderland. It is at least certain that one of the earliest and most authentic historical documents, which by the best judges has been pronounced to be descriptive of the small-pox in man, proves that the same disease simultaneously appeared in the inferior animals. But, be this as it may, traces of eruptive epizootic diseases, in many cases communicated to man, may be found in the historians of almost every age. As we descend nearer to our own times, the descriptions are more precise and accurate; and the variolæ among cattle is a disease as clearly described, and as accurately defined as any variolæ that ever attacked man. And it is a most remarkable fact, that in the year 1690 it began with the animals, and immediately afterwards proceeded to attack the human species in the most fatal form. It is little more than fifty years since this epizootic pestilence ravaged the cows of a large portion of Europe, and especially this country; and it has been proved, as far as any thing of that kind can be proved, that the cow-pox which Dr. Jenner met with in our dairies was the local, or rather *enzootic* remains of that pestilence.

I believe, sir, it is equally a mistake that the variolæ which appears among cows is always of the mild character that it has lately assumed, as is implied in the sixth aphorism of Doctor Sonderland.

During the last epizootic among the cows in this country it was of a most virulent and contagious character; and the practice of inoculation was frequently and successfully employed, as in the human subject, to subdue its severity. It is equally true, that the dis-

ease has, within these few years, been so violent in the dairies of Gloucestershire as to destroy the life of the animal, a case of which kind has been published in the Life of Doctor Jenner, page 351. The disease, likewise, when caught by the milkers, is often a severe one; and at times there is a considerable eruption. Were I to detail all the facts by which these statements may be proved, I should more than fill your number. They, and a great many others, have been examined, and arranged for the express purpose of proving the identity of small-pox and cow-pox, and thereby explaining the protecting virtues of the latter. The whole will be found in the fifth, and other chapters of the volume above cited. I have examined the evidence therein contained with great attention; and it appears to me as conclusive as any thing of the kind can be. The propagation of the small-pox to the cow, in the manner described by Doctor Sonderland, is an interesting and important fact, and affords the strongest confirmation of the accuracy of the doctrines contained in Dr. Baron's work.

I am, Sir,

Your obedient servant,

NON NEMO.

November 10, 1831.

PHYSIOLOGICAL CONJECTURES.

No. II.

BY PHILEATHES.

To the Editor of the London Medical Gazette.

SIR,

I.

IT has long been an interesting question to determine, what is the precise state of the circulation and of the respiration, during the state of torpor in hibernating animals.

I think this may be done, in regard to the circulation, by submitting the web of the frog, the wing of the bat, the ear of the dormouse, to be viewed in the microscope.

I think the quantity of respiration may be readily and accurately determined, in the manner described in my former communication, in the Medical Gazette, No. 185, page 373.

It will be interesting to associate with these circumstances the state of the temperature, taken by a fine and delicate thermometer, in the axilla pressed by the wings of the bat, or the anterior extremity of the dormouse.

It appears probable that one character of hybernation is, in fact, that of a reduced state of existence, the animal being deprived, by the influence of cold, of the energy of part of the nervous system, and so brought lower down in the zoological scale.

Might it not, under these circumstances, bear to have these parts of the nervous system, viz. the brain, medulla oblongata, and medulla spinalis, removed, without the destruction of the function of circulation, as is observed, under peculiar circumstances, in the batrachia?

In fine, is not the case of the animal, in the state of hybernation, identical with that of an animal deprived of brain and spinal marrow? Does not cold slowly destroy the functions of these organs, whilst the experiment removes the organs themselves?—and does not the difference between the two cases arise from the different degrees of slowness or rapidity with which the influence of the brain and medullæ is removed?

A comparison between the remaining functions seems to confirm this conjecture.

II.

It appears probable that the peculiarities of the fœtal life consist also in those of a lower order of existence.

The blood of the fœtus is not only different from that of the parent, but, according to MM. Prevost and Dumas, its globules are larger—that is, it bears the character of that of an animal of a lower order.

There is also a remarkable deficiency in the function of respiration. The changes effected in the placenta, the organs of fœtal respiration (see Phil. Trans. for 1810, p. 213—217), are far less marked than those afterwards induced in the lungs.

Lastly, the fœtus bears to be deprived of the influence of the brain and spinal marrow without experiencing any impediment to growth and development. These organs may actually be destroyed, in the chick, on the third day of incubation, according to M. Serres, without arresting the growth of the animal.

Both in the hybernating and fœtus states, the peculiarity probably consists in a good circulation, with its secretions, and an independence of the large nervous masses and of respiration, in a greater or less degree; and both may thus be compared to that of an animal deprived of brain and the medullæ, whilst both probably bear to be deprived of these organs.

The general principle is that of a lower grade of animal existence.

PHILALETHES.

14, M—S—, Nov. 14, 1831.

PROVINCIAL SCHOOLS OF MEDICINE AND SURGERY.

To the Editor of the London Medical Gazette.

SIR,

AMONG the schools instituted for the promulgation of medical and surgical information, the provincial establishments, in county and other large towns, do not seem either to have attracted that degree of attention in their respective districts, to which their means of furnishing information entitle them, or to have acquired, in the eyes of the profession, an importance proportioned to their real utility in promoting the education of the junior branches of the profession.

The subject has been enforced on my attention by a recent visit to one of our large provincial towns, Kingston upon Hull, in Yorkshire, where an Anatomical and Medical School has lately been established under the promising auspices of Dr. Alderson and his able colleagues. At present, the number of attending pupils is limited, owing, probably, to that temporary opposition which a newly-established institution generally meets with; but its ultimate success is ensured by the enterprise, experience, and talent, that are combined in the professors of the respective departments. Mr. Craven did me the honour to give a lecture on anatomy, that I might judge of the mode in which the anatomical department was conducted; and, if this school may be allowed to afford a fair criterion of other provincial institutions of the same kind, their object of giving a sound elementary education to the

young student cannot fail to be attained, and their utility in forwarding the means of education cannot be questioned.

The advantages which they present are numerous; not only to the student, who may be desirous of obtaining information, but to all concerned, whether in the business of medical instruction, or in the management of the hospitals and infirmaries which most of our large towns possess.

The labour of teaching is not confined to the mere act of delivering a lecture at stated periods; but his duties entail upon a professor the necessity of preparing himself by obtaining an intimate knowledge of his subject. The physician or surgeon, who is engaged in teaching, is compelled to instruct himself before he can instruct others; the advantage, therefore, derived from teaching is not confined to the pupil, but extends itself to the teacher, by holding out an additional motive to labour and study. With this view, his mind must be constantly directed to the cases of disease which he witnesses—his memory must be stored with valuable facts and observations—and he is obliged to keep pace with the progress of science, by reading the best works as they issue from the press. A man with his whole mind thus directed to the duties of his profession, becomes an acquisition of no ordinary importance to the town in which he resides; the whole population feels the effect of his information and experience; the poor in the infirmary have the benefit of his superior skill and judgment; and the lustre, which he has been enabled to throw around his own character, is thus reflected in the more solid advantages, which his medical and surgical skill confer on his fellow townsmen. It becomes, therefore, the interest, no less than it is the duty, of the governors of provincial hospitals and infirmaries to foster with care these rising institutions, which have difficulties enough to encounter in the hitherto insurmountable prejudices of the vulgar, without meeting with opposition from those whom they naturally look up to for support.

Indeed, in giving encouragement to provincial schools of anatomy, the governors of medical institutions are incessantly calling into action talents which might otherwise have been dormant, and are bringing forward as teachers men of industry and ability, who, from

neglect and want of opportunity, might for ever have remained unknown and unnoticed. From among these young men, the governors have the power of selecting the most distinguished and the best informed for the medical officers of these hospitals; thus rewarding merit and encouraging rising talent, instead of conferring the appointments on persons whose character is wholly unknown, except to a few immediate and interested friends of the candidate. The best talent, which the town affords, will be thus secured to the public institutions for the sick poor; or, if it be not secured, at least no plan can be well devised, that affords a fairer chance of advancement to the successful and industrious teacher of his profession, or that more effectually directs patronage to its legitimate object—the protection and advancement of merit.

Much, however, as all classes are benefited by such establishments, none participate in their advantages so deeply and so immediately as the students themselves. The regulations for study, now imposed on a medical student, by the recent enactments of the College of Surgeons and the Apothecaries' Hall, render it no easy task for a pupil, however diligent he may be, to follow, with any lasting effect, the course of study prescribed, in the short period allotted for their residence in town.

It is far from my intention to impugn the propriety, much less the motives, of their regulations; for, I believe, that these legislative bodies are impressed with a sincere conviction of the necessity of improving and extending the system of medical education, and that they are conscientiously endeavouring to attain this end by the enforcement of certain laws. But it is equally the duty of these bodies to have some consideration for the student, and to be aware, what an immense mass of labour they entail upon a pupil, who is desirous of fulfilling his duty to them, and his duty to himself, (duties not always identified in a pupil's mind,) by compelling him to attend all the lectures, not within the space of two years, but of two winter seasons. As it is some time since the pupillage of the Examiners of Apothecaries' Hall elapsed, in order to refresh their memories, as to what a pupil is enjoined to attend, and to give the latter some idea of the extent of pursuits he has to encounter,

when he comes to town, I subjoin a copy of our printed list of the hours of attendance on the various lectures, &c. delivered every week at Guy's Hospital:

	<i>Hours.</i>	M	T	W	Th	F	S
Lectures on Midwifery	$\frac{1}{4}$ before 8	—	—	—	—	—	—
— Chemistry	$\frac{1}{4}$ before 10	—	—	—	—	—	—
Examination of Patients for Admission	10	—	—	—	—	—	—
Demonstrations in Anatomy ..	11	—	—	—	—	—	—
Dressing casual Patients at Surgery	11	—	—	—	—	—	—
Prescribing for Out-patients—Assistant Physician } and Assistant Surgeon	11	—	—	—	—	—	—
Accidents, &c. attended by the Surgeons	at all hours	—	—	—	—	—	—
Surgeon for the Week attends in the Wards	12	—	—	—	—	—	—
Patients visited in the Wards by Surgeons	12	—	—	—	—	—	—
Physicians	12	—	—	—	—	—	—
Eye Infirmary	12	—	—	—	—	—	—
Surgeon Dentist visits	$\frac{1}{2}$ past 12	—	—	—	—	—	—
Demonstrations in Morbid Anatomy	1	—	—	—	—	—	—
Consulting Surgeon visits	—	—	—	—	—	—	—
Dressing the Patients finished at	2	—	—	—	—	—	—
Lecture on Anatomy	2	—	—	—	—	—	—
— Theory and Practice of Medicine	$\frac{1}{2}$ past 3	—	—	—	—	—	—
— Medical Jurisprudence	do.	—	—	—	—	—	—
— Botany and Entomology	do.	—	—	—	—	—	—
— Experimental Philosophy ..	6	—	—	—	—	—	—
— Comparative Anatomy	6	—	—	—	—	—	—
— Physiology	$\frac{1}{2}$ past 6	—	—	—	—	—	—
— Materia Medica	7	—	—	—	—	—	—
Physical Society	7	—	—	—	—	—	—
Lecture on Surgery	8	—	—	—	—	—	—

To follow with zeal so extensive a range of pursuits as is comprised in the above list, requires not only a spirit, but a frame of constitution, rarely possessed by a youth of twenty years of age; and it unfortunately happens, that a season rarely elapses without one or more of our most hardworking young men falling a sacrifice to their close application. I would, then, take this occasion of submitting to the Examiners for the Apothecaries' diploma, the expediency of extending some of their prescribed courses of lectures into the summer season,—an arrangement that would much lighten the winter season, and virtually lengthen the period of study, by compelling young men to remain in town during a part of the summer, instead of passing that season in other than intellectual pursuits. For example, the lectures on *Materia Medica*, on *Medical Jurisprudence*, and on *Botany*, might, (if it met with the concurrence of the respective lecturers on these subjects,) with advantage both to pupil and to teacher, be deferred to the three summer months of June, July, and August. Such an arrangement

would permit the pupils to pass a month (May) in the country, at the conclusion of the winter season, and a second month (September) at the termination of the summer course. I say, with the concurrence of the lecturers; for, in making regulations of this nature, it would savour a little of courtesy, if the Court of Examiners would consult the lecturers before passing regulations, by which the latter are principally affected. The medical chairs in the schools of this metropolis are, with few exceptions, held by physicians, who are entitled at least to a little courtesy, if not to deference, as to the line of study that is chalked out for their pupils; and although, by the accidental operation of the law, the Examiners of the Apothecaries' Company are enabled to issue laws establishing professorships, and dictating the order of attendance upon lectures, yet, an appeal to the opinion of men, who have received a somewhat more extensive education, and therefore are, at least, equally qualified to direct their pupils' course of study, might enable them to arrive at a more correct conclusion, as to what is fitting and

expedient in conducting medical education.

I would also take the liberty of suggesting to the Court of Examiners, and I do so with a pretty extensive experience of medical pupils, that the character of the medical student is much improved within the last ten years; and, that the order in which the lectures on *Materia Medica* and *Medicine* should be attended, might, with safety, be left to the discretion of the students themselves. Regulations that are not absolutely necessary, only encumber the student, and hamper him in the distribution of the limited time he has to bestow on his multifarious pursuits. Few arrive in town, especially since the establishment of provincial schools, without some knowledge of chemistry, botany, and *materia medica*, and a considerable share of practical experience in medicine; and, I venture to affirm, that, if left to themselves, as to the order of attending lectures, they would be able to parcel out their time to more advantage, and obtain upon the whole more extensive acquaintance with the different subjects, than any compulsory clauses or quarantine laws of education can enable them to acquire.

After perusing the above routine of medical and surgical education, as now conducted at our principal metropolitan hospitals, it needs but little argument to convince the student of the utter impossibility of effectively bestowing his attention on all these important branches of his education, unless he is able to lighten his labour, by having previously obtained an extensive acquaintance with anatomy, before he commences his studies in town. Provincial schools afford him, during his pupillage, the means of gaining this solid foundation: it matters not on how limited a scale their operations may be conducted; ability and zeal on the part of the lecturers, and industry on the part of the student, will stand in the place of a more extensive field of operations.

The objections so frequently urged against the long period of apprenticeship, as required by the existing laws, vanish, when the instruction received by occasional attendance on lectures during apprenticeship is taken into consideration. The mind of a surgeon's pupil, who possesses these advantages, becomes early drilled and ini-

tiated, not only, as heretofore, in the mechanical art of dispensing medicines, but in the interesting walks of science. He is insensibly led to form a just estimate of the profession in which he is embarked, and to take early delight in those pursuits which are to form his principal study. At the termination of his apprenticeship, with mind well prepared by previous habits of study, and a disposition to take advantage of every opportunity that offers of obtaining information, he comes to a large hospital, where he soon begins to find the value of his previously acquired knowledge. Instead of being obliged to devote his time to the acquisition of the elementary principles of science, he is able to see and understand their application in practice; instead of exclusively passing his valuable time in the ordinary occupation of a dissecting-room, which the pupil uninformed in anatomy is compelled to do, he can select his subjects for dissection, and more accurately inform himself in those parts that are concerned in operations and practical surgery. He is able to spare more time for observation of disease at the bedside, aided by the remarks of the physician and surgeon; his previous information divests his pursuits of that irksomeness that must attend a course of study directed solely with a view to pass an examination; and he feels and regards his studies rather in the light of a pleasant pursuit, than as an arduous and laborious task.

It were endless to attempt to enumerate or to describe the advantages and influence of previous study on a young man, before he comes to a large scene of disease for the completion of his education. But there is one, in which is comprised so much that is good, that it deserves to be especially pointed out—I mean the utter extinction of that pernicious system, technically termed, "grinding": the demands made on a student's time have hitherto rendered this process to a certain extent unavoidable; a young man must have a retentive memory, indeed, who can pursue science as it ought to be followed, and at the same time retain in his mind, at the end of two years, those points which an examination calls for. The student, who conducts his studies solely intent upon his examination, will charge his memory with the details of his subjects,

to be forgotten as soon as they cease to be subservient to the purpose for which they were acquired.

Strongly impressed with the necessity of medical pupils of the present day coming to town with some knowledge of the subjects of their future studies, I have thought it right to impress on their minds the immense advantages which the schools in large towns hold out to them, assuring them that they will sensibly feel the benefits arising from them during the whole of their professional career. Those who pass their apprenticeships in towns too small for the existence of a regular school, I would recommend to form associations, with the assistance of one or two well-informed surgeons, for the purposes of regular instruction.

With apologies for the length of this letter, I have the honour to remain, sir,

Your obedient servant,

C. ASTON KEY.

St. Helen's-Place,
Nov. 20, 1831.

HOSPITAL PUPILS.

To the Editor of the London Medical Gazette.

SIR,

I TRUST you will pardon my addressing you on a subject of deep importance to the pupils who are attending the surgical practice of the united hospitals of Guy and St. Thomas. I have, sir, to complain of the liberality (I *must* be allowed to say ill-judged) of the officers who are connected with those useful institutions, which are so deservedly renowned for the great advantages, and the wide and extensive field of observation, which their well-filled wards present to the diligent and industrious student. The persons (very many of whom are not pupils) who accompany the surgeons round the wards, are so numerous that it is impossible for more than one patient in three to be seen; and consequently much interesting clinical instruction is lost, by many of those who are actually pupils, from the distance at which they are necessarily placed from the surgeon and the patient. If those who have not entered to the hospital at all, or who do not even attend a course of lectures there, be allowed to see the practice at the expense

of the exclusion of those who *have* paid for admission, of course there can be no possible advantage in paying the admission fee of 26*l.* 6*s.* This, sir, certainly ought not to be, and I feel assured that Messrs. Key, Morgan, and Cooper, whose kindness, attention, and anxiety to afford instruction to their pupils, are so generally acknowledged, will use every means in their power to correct the important abuse, if it were sufficiently made known to them, and I earnestly trust some means will be taken immediately for the exclusion of those who have no right to intrude themselves into the wards, to the great disadvantage of those gentlemen who have paid for their admission.

If, sir, the all-absorbing and important subject of cholera, upon which you are now so philanthropically engaged, will allow you to find space for the insertion of these humble observations, you will confer an obligation on

Your obedient servant,

And constant reader,

A PUPIL.

Borough, Nov. 15, 1831.

ANALYSES & NOTICES OF BOOKS.

L'Auteur se tne à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Observations on the Nature and Treatment of the Cholera Morbus, now prevailing epidemically in St. Petersburg. By GEORGE WM. LEFEBVRE, M.D. Member of the Royal College of Physicians of London, of the Royal Medical Society of Edinburgh, and Physician to the British Embassy, St. Petersburg. 8vo. pp. 96.

THERE is a pleasant sketchiness about Dr. Lefevre's manner of treating his subject, which is well calculated to win the reader's attention; and we take leave to think, that this is no small recommendation for the work before us, now that the public are beginning to tire of controversy about the nature and causes of cholera, and to look for something practical and decided as to its history and treatment, be the same of what authority it may. Steady, sober

narrative, and logical argument, even upon this all-important subject, are, we perceive, growing wearisome and unfashionable—at least, if we may judge from various remarks appearing daily in the non-medical publications, betraying but too often a bitter spirit of ill nature towards the members of our profession, simply because we cannot supply the sapient grumblers with understanding enough to sift what they read—to recollect that they peruse hastily the opinions of individuals—and that there is no one possible topic of controversy, upon which there will not be almost as many different opinions as there are thinking individuals employed to think about it. Dr. Lefevre's book, however, may be read by these non-contents as a diversion—not that they will find it a whit more decisive in its tenets about the theory of cholera, or more practical in its precepts, than many works which have already appeared; but there is a lightness in its whole construction which renders it rather refreshing in the perusal. The doctor is a non-contagionist; we are sorry for it; though, we fancy, he is so far from being decidedly so, as that were he among us just now we could easily persuade him to confess himself “a *contingent* contagionist” at least, which is the most newly-adopted and saving title of the party with which he seems rather inclined to identify himself. At all events, Dr. Lefevre, who finished his book at St. Petersburg in September last, acknowledges himself a non-contagionist merely for want of *more facts*; and that is as fair a reason as we can expect any man in similar circumstances to give. We wish all those who profess to belong to the same creed of scepticism, were equally candid in their acknowledgments.

Dr. Lefevre derives his experience of cholera from his personal attendance on “a large though not very populous district” of the Russian capital, during the prevalence of the late epidemic; and the “Observations” now before us are the substance of the reports which he then and there made to the chief inspector of his district. “In fulfilling this task (says Dr. L.) I was induced to enter more at large into the subject, and as I imagine that the observations of an individual, or rather a comparison between the observations of different individuals during the late malady, cannot be wholly without

interest, I have been induced to send the following to England for publication.”

The author's account of the manner in which cholera invades a country, or a town, is curious.

“Among the most striking features in the propagation of this disorder, is what may be styled *its eccentric motion*;—and, true to its eastern character, it presented this feature here, not only in its passage through the country, but even in its dissemination over the different parts of a town which it invaded. In its progress from Tiflis to Moscow, it was *observed to move in a zig-zag direction*, rather than in a regular line of march. It would pass by a town which lay immediately in its path, to appear in another, which it must have reached by a very circuitous route. When it invaded a town, it followed the same law; touching at a point to *fly off at a tangent*, and appear at a widely separated part from that where it first commenced, leaving the *intermediate spaces uncontaminated*.”

This is, to be sure, not a little fanciful; the doctor thinks himself a non-contagionist, yet, according to the preceding *eccentric* account, the disease is progressive, *moving*—that is, as we conceive, attacking individuals—in a *zig-zag direction, flying off at a tangent*; which, if we can understand our author, means that new seizures are observed along a line of route situated laterally to its recent line of progress; whilst it appears that it is only somewhere at the extremity of the tangent that new cases occur, the *intermediate spaces* (that is, of course, the tangential line) being left *uncontaminated*. Now, pray, how does Dr. Lefevre know that the disease follows the direction of a tangent, if it only appears at the two extremities of the supposed line, and never manifests itself between them? Perhaps it jumps, or takes flight like a bee; or, more soberly, what is to prevent us from supposing that it is carried in a coach, or goes in the company of some traveller? Dr. Lefevre thinks, however, that there is something in it much more mysterious than this; and if ever he hears of its passing by a town in the course of its progress, and appearing beyond it, takes it to be a voluntary act on the part of the cholera, or only attributable to its eccentric propensities—not seeming to care for in-

quiring whether it has been excluded unceremoniously from the said town or not.

“ ‘The disease,’ says Dr. Kennedy, ‘would sometimes take a complete circle round a village, and leaving it untouched pass on, as if it were about wholly to depart from the district. Then, after a lapse of weeks, or even months, it would suddenly return, and, scarcely reappearing in the parts which had already undergone its ravages, would nearly depopulate the spot which had so lately congratulated itself upon its escape.’ ”

“ ‘This whole passage (says Dr. Lefevre) is applicable to St. Petersburg at the present moment; for though last autumn we were surrounded on all sides by the cholera, still we escaped during the winter and succeeding spring. Seven months had elapsed since its appearance in places not far distant from the capital, and we began to think that the imperial city would escape its ravages.’ ”

“ ‘There is a striking resemblance,’ observes Dr. Kennedy, ‘between the propagation of the plague, as described by Procopius, and that of cholera. It always spread from the sea-coast to the inland country; the places which had escaped the fury of its first passage, were alone exposed to the contagion the second year.’ ”

“ ‘People (at St. Petersburg) resumed their usual avocations and their ordinary diet; they began to regret the privations they had voluntarily undergone in abstaining from various articles of food which they considered dangerous. The profusion of fruit which pours into Petersburg from all the southern provinces, but particularly the grapes and water-melons from Astracan, had, the preceding season, been allowed to rot in the hampers in which they were imported. The supposed preservative means were now generally laid aside, and the houses were no longer redolent with the fumes of chlore, tar, and juniper, which poisoned the antechambers. The family receipt-book was no longer conned over by day and meditated upon by night, and Buchan and Thomas had already been replaced by Fairy Tales and Travels in the East. The medicine chests and prescriptions of each confidential physician were altogether discarded; castor-oil and opium fell again to a moderate price;

and, indeed, the confidence in the faculty was much shaken, when it was ascertained that the means employed by the common people were equally successful with those adopted by the profession itself. Some few, however, continued to use the same precautions as heretofore, and even took medicine by anticipation. Some even fell victims to such an absurdity. In the midst, however, of this almost general oblivion of the past, the *hydra-headed monster* was upon his route; for the evil day had only been postponed.”

And with what deadly effect the “hydra-headed monster” came at last into the Imperial city—and what a panic he created there by his actual presence—are too generally known to need a repetition from us. We have thought it right to give the preceding passage, however, as one pregnant with a moral not inapplicable to present circumstances among ourselves.

In his account of the symptoms of cholera, Dr. Lefevre is not so alarming as certain other writers: we give a specimen. Speaking of the great majority of ordinary cases, he says:—

“ ‘The affection of the head was almost universal, but it was of different kinds. Sometimes the patient complained of a sudden lancinating pain, which was of momentary duration; at other times the feeling that is produced by fainting, dizziness before the eyes, dimness of vision, *muscæ volitantes*, preceded the attack.’ ”

“ ‘The following case illustrates a peculiar feeling in the head immediately preceding an attack of cholera. The person was seized in my presence, whilst giving directions regarding the arrangements of a large hospital, over which he presided. He was sixty years of age, and apparently in perfect health: whilst in the midst of conversation he suddenly put his hand to his head, and complained of a sharp pain passing through the temples, which lasted but for a few seconds; he ascribed it immediately to having taken a pinch of snuff from a neighbour’s box, which being stronger than that which he habitually took, it had got into his head. The sensation passed off, and he resumed his conversation. This took place at nine o’clock, P.M.; at eleven he was seized with other symptoms of cholera, and died of the disease after five days’ illness.’ ”

“ ‘Though I have almost universally

found an attack of cholera preceded by something bordering upon vertigo, yet, in one rapidly fatal case, no such feeling was present till it was produced by the weakness caused by the evacuations."

The vomiting in cholera, Dr. Lefevre describes as very similar to what is seen in sea-sickness: while lying in a horizontal position, the patients do not experience much nausea or desire to vomit; but upon raising their heads, they commence vomiting immediately, and with that kind of straining which we witness in sea-sickness.

In his account of treatment, our author is very copious: he divides the subject into the external and internal treatment; and under each head examines the various classes of remedies which are commonly employed. Some of the remarks in this portion of the volume are extremely well worthy of perusal.

A maplike table, indicating the progress of the cholera at St. Petersburg, is prefixed to the observations: it is an ingenious mode of exhibiting the numbers on the sick-list at each successive date; but for want of a little explanation, we have seen some readers sadly puzzled to make out what it meant.

Cholera, its Nature, Cause, Treatment, and Prevention, clearly and concisely explained: with an Appendix, containing practical Remarks on Fever and Dysentery, with which Cholera is intimately connected and frequently combined; being the Substance of Reports made to the late Government of Poland. By CHARLES SEARLE, Esq. of the Hon. East India Company's Madras Establishment, and lately in charge of the principal Cholera Hospital at Warsaw. Second Edition, revised and enlarged.

THIS little brochure is a second and enlarged edition of the one we recently noticed: it contains some useful hints, and has the advantage over many others, of giving the results of actual experience and observation at the bed-side of the sick. Its chief fault is a want of arrangement and a defective perspicuity. There is nothing absolutely new in Mr. Searle's present recommendations: an emetic of salt in warm water; small bleedings, local or general; calomel almost always, and purgatives occasionally; heat, rubefacients, and

frictions externally;—such are the chief means of cure suggested. Mr. Searle is a non-contagionist, but he must forgive us for saying, that, looking only on what was within the range of his own vision, and magnifying its importance like what is seen through a microscope, he loses sight of the general bearings of the question as a whole. The pamphlet, however, with all its defects, has more claim to attention than many of the productions on the same subject, under which the press at present groans: the quantity of catch-penny trash constantly handed to us is amazing.

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Reports of Medical Cases. By DR. BRIGHT. Price 9l. 9s.

[Continued from page 201.]

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Apoplexy of the Spinal Cord.

HAVING concluded his remarks on pressure of the brain occasioned by sanguineous effusion, and on apoplexy generally, Dr. Bright presents us with an excellent case of apoplexy of the spinal cord, for which he acknowledges himself indebted to Dr. W. Stroud. The following abstract is taken from Mr. Stroud's own words:—

Mr. Benjamin B., aged 48, was engaged during the greater part of his life in the merchant service at sea, performed many voyages to India and China, and in general enjoyed good health. In 1812 he received a violent chill at St. Helena, which brought on a rheumatism, with which he laboured for many years. The muscles of the trunk both before and behind were occasionally affected; his bowels were habitually costive, and the digestive organs generally out of order. This was his last voyage. He engaged in commercial business at home, but, not proving successful, his health suffered through care and anxiety; he fell into a declining state, attended with nervousness, increased flow of urine, and some degree of emaciation. On the night of April 20, 1826, he sustained a slight paralytic attack, by which his mind and speech were a little disordered, and all the muscles on the left side were weakened. From the effects of this attack he never recovered; neither his articulation, however, nor his faculties were sensibly impaired; and, from being at first hemiplegic, he gradually became paraplegic,

in which state he died April 16, 1827. During the two months preceding his death, the palsy having decidedly seized the trunk, the use of the catheter became necessary. There was also sloughing about the nates, of the fetor from which his olfactory nerves were sufficiently acute to render him conscious. The act of death may be said to have occupied thirty-six hours; the powers of evacuation, speech, deglutition, the mental faculties, and the remaining muscular force, were successively extinguished. The respiratory function ceased the last. "As he sat up in the bed supported by pillows, the head and body vacillated from side to side; the breathing became laborious, and sometimes moaning and stertorous; the eyes were half-closed, and had a wild and vacant expression; but as long as the power of communication continued, he signified that he was free from pain and distress."

Upon examining the head and spine, the following appearances were observed. The brain was sound, with the exception of a little serous fluid between the membranes, and some congestion in the veins of the pia mater. The disease had been apparently seated in the coverings of the spinal cord, which exhibited evident marks of inflammatory action, although the cord itself presented no visible derangement. Serous fluid, in considerable quantity, was found between its membranes, and the arteries of the pia mater were much injected. "Within the upper dorsal vertebræ, on the left side, rather more than an inch in length, in the direction of the spinal axis, and about half an inch in a transverse direction, was an apoplectic cell, containing the red and broken remains of a coagulum; and lower down in the spinal canal, the internal ligament was to some extent deeply marked by ecchymosis, as if in progress towards the formation of another similar effusion. The extravasation appeared to have been wholly external to the cord, which was in consequence compressed; but, as far as could be perceived, it was neither disorganized nor inflamed. The coagulum seemed to be subjacent to the pia mater, and to be enclosed in a false or adventitious membrane."

Here, then, was clearly hemiplegia of the left side, arising from an apoplectic effusion on the same side of the spinal cord; although the right side of the

brain, it should be observed, did certainly exhibit an undue degree of vascularity. This, however, agreed perfectly with the symptoms, and more especially with the fact that the complaint was purely paralytic, and eventually assumed the form of paraplegia, while the senses and the mental faculties were not obviously impaired.

It is justly observed, by Dr. Stroud, that we may derive several useful and interesting conclusions from the history of the foregoing case. For example, it would appear that conditions purely nervous, and originating in long-continued mental disturbance, may, according to predisposition, induce an inflammatory or hemorrhagic state of the membranes investing the brain, or the spinal cord; and that this state, operating through the medium of the nerves, may exert an extensive influence both on the solids and on the fluids of the body. Under the exacerbations which repeatedly occurred in this case, the intensity of the secondary irritation progressively advanced, from itching and smarting to severe rheumatic pain, chiefly between the shoulders and on the affected side. The blood became buffy; the bile dark-coloured and concentrated; the alimentary canal abounded with acidity. Without any apparent disorder of the stomach and bowels, the tongue used to become red, or foul, or parched; and all this obviously from the state of the nervous system. The excitement which proceeded from the left side of the spinal cord to the right side of the brain, furnishes an interesting illustration of an occurrence which usually takes place in the opposite order of succession, and affords additional evidence that the corresponding halves of the central mass of the nervous system decussate in the medulla oblongata.

PRESSURE OF THE BRAIN resumed.

Pressure from Tumors.—The first case with which our author illustrates this morbid condition of the brain, is so very curious and interesting that we are sure our readers will be in no small degree obliged to us for giving them a pretty copious abstract of it. It is a case in which a large tumor was found attached to the dura mater, and descending deeply into the anterior lobe of the cerebrum.

A. Z., a gentleman of about fifty

years of age, had been for a considerable period subject to severe attacks of headache, which always began at the forehead. He had had a small encysted tumor removed from that part fifteen years previously, but never had any suspicion of the possibility of a connexion between his unpleasant symptoms and the almost forgotten tumor, until questioned strictly, during his illness, with respect to past occurrences serving to throw light on his perplexing case. He was subject to gout also; which, however, only seemed to render him more attentive to his diet. But all this never created any uneasiness among his friends: it was not till the summer of 1824 that they began to be seriously sensible that all was not right, from the circumstance of his newly-acquired listlessness and indolence, so much at variance with his former habits.

"In January 1825," says Dr. Bright, "I first saw this gentleman as a patient; at that time the long continuance of his bodily weakness, and his mental failure, and their gradual increase, had begun to inspire his medical attendants with a fear that chronic organic disease might be taking place: he was perfectly collected, and able to answer questions, but he was unwilling to do so; would sit on the sofa and read the newspaper without paying any attention to it; said that he felt occasionally, as he sat, a tendency to be drawn to the left side, or rather to give way on that side: he could walk across the room quite alone, but sometimes could not direct his steps, and sometimes rather ran than walked; he could still play at cards with his family for amusement in the evenings. Occasionally he had headache, and now and then became very drowsy for a day or two together, pointing, when asked, to his forehead as the seat of pain. At this period he would sometimes become very irritable, and almost irrational on some points—more particularly, he would often call for his great-coat and insist on going to the theatre, and it was scarcely possible to prevent his going. It was obvious that from month to month he became more infirm, although at times the drowsy fits passing away, or other casual ailments subsiding, seemed to give hope that a favourable change was taking place. It was remarked that he became more and more helpless, and more absent every day; for many months before, he had some-

times been so absent as to stop in the street looking at indifferent objects for a long time together, and by this means probably arrive an hour after the appointed time at a dinner-party; at other times he would sit after dinner, taking his wine with one or two familiar friends, without speaking a word, which was quite contrary to his usual habits. He would now sit for hours together totally unoccupied, and without saying a word; and he not only rose very late, but would frequently spend two or three hours in dressing."

In the course of the summer, he had to be wheeled about in a chair; the sphincter of the bladder lost its power, and his drowsiness increased to an alarming extent. A blister to the scalp produced erysipelas of the face, stranguary, and a violent spasmodic shaking of the left hand. Some months after this, a helplessness and a lethargic condition came upon him, that rendered him a mere infant; though he was certainly conscious of those around, and very thankful for the care taken of him. He was frequently incoherent in his ideas, fancying that he had seen certain persons of distinction, and public characters, who had never been near the place, and constantly saying that he was going out to dine with some one. Strabismus was now observable, and slight distortion of the features; deglutition difficult—sometimes alarmingly so. The tongue, which for years had generally been foul, and often had a thick stripe of dark fur towards the base, became loaded in a most extraordinary manner with a thick olive-coloured coating. The sphincters now lost their power irrecoverably. The left hand and leg seemed partially paralysed. The action of the skin was changed: profuse perspirations would occur for weeks together. Every feasible plan of treatment was tried, but to no effect. A severe epileptic fit carried off the sufferer at length, in July 1826. The following we think well worthy of being extracted.

"*Sectio Cadaveris.*—On removing the skull, the vessels of the dura mater were rather turgid; and raising the membrane it was found to adhere to the parts below on the anterior portion of the right hemisphere of the cerebrum, more particularly about an inch from the front near the longitudinal sinus, where the adhesion was perfect, with an appearance of vessels radiating over the hemisphere. The surface of what appeared to be brain at that part was obviously changed,

and lobulated in its structure; and it would appear that the great aggravation of symptoms leading to the fatal termination, was connected with a softening and a watery infiltration of the brain in the neighbourhood of the tumor.

Two cases are next given of tumors formed by disorganization of the brain, causing hemiplegia: both terminated fatally. We have then an interesting account of a case in which there was stupor and other symptoms of pressure without paralysis; but in which, after death, there were found two or three hard tumors in the anterior lobe of the cerebrum. After this comes a case of scrofulous tubercles in the brain, producing paralysis and great irritation: some of the particulars we must extract.

Amelia Humphreys, aged eleven, was admitted under Dr. Bright's care in November 1829. Five months previously, up to which period she had been a healthy child, though delicate, she had a fit at school, and never completely recovered after. By leeching and blistering she was enabled, in about a month after the fit, to feed herself and walk with but little assistance. She relapsed, however; and upon admission at Guy's presented the following symptoms:—The left leg incapable of motion—stiff, cold, shrunk, and firmly extended; the foot drawn inwards. The right leg almost similarly affected; but both legs are sensible to the touch. The use of the upper extremities impaired; the back stiff and rigid; the feces and urine pass involuntarily; she screams occasionally, and complains of her head. Ordered to have her head shaved, and the common embrocation applied. *Habeat Pil. Galban. comp. et Pil. Aloes c. Myrrha aa. gr. v. omni nocte.*

A seton was applied to the nape next day, and her bowels being relaxed she was ordered *Hydrar. c. Creta, gr. ij. bis die, et Inf. Cascariæ cum Soda, ter die.* Beef-tea, Arrow-root, &c. &c. The appetite good.

" 11th.—Abdomen greatly swollen last night; the breathing shorter and more laborious, effected chiefly by the diaphragm. Hiccup came on early in the morning, and has since subsided; the respiration and pulse were then exceedingly rapid; they are this morning 40 and 160 respectively: the irritable habit and fretfulness have subsided,

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and there has supervened a state of oppression without coma or delirium; indeed she is remarkably distinct in her answers, recollecting circumstances regarding her health which occurred long ago; the appetite, which was great, is almost gone; the countenance pale, but the cheeks are suffused from time to time with a purple flush; the abdomen is rather tumid, the feces always passed involuntarily; the right arm is bent over the body, and firmly contracted, stiff, and inflexible; the spastic action of the muscular fibre may be felt by laying hold of the right arm or the left leg; the eyes are directed to the left, and she does not move them when requested, or when spoken to: there was observed, after taking nourishment this day, a violent spasmodic action of the muscles of the left side of the face, particularly the mouth.

" Nine o'clock, P.M.—Comatose and insensible; the eye-balls, which before were fixed, are now constantly rolling; the mouth is drawn to the left side, not permanently, but spasmodically; the mouth is nearly closed, and frothy saliva rests upon it. Pulse 176, not faulting; the extremities and head warm; hiccup renewed occasionally; abdomen tumid; the muscles of the extremities are constantly quivering and twitching; she has taken nothing since five o'clock.

" 12th.—Dissolution seems to be approaching; the breathing sonorous, with mucous tracheal rattle; a miliary eruption, forming small transparent vesicles, has appeared over the whole chest; eyes dim and fixed; pupils contracted; pulse flags during inspiration, and then beats three or four hurried strokes during expiration; forehead and extremities very warm. Expired at half-past four o'clock.

MEDICAL GAZETTE.

Saturday, December 3, 1831.

" Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

AN APOLOGY FOR HUMAN DISSECTION.

THERE are three questions which seem to us to comprehend the whole matter

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at issue between the profession and the public with regard to the application of the dead to the use of the living. They are questions to which all those delicate interrogatories may be reduced which for the last two or three years have been asked us, and which we never remember to have heard urged with more interest or earnestness than they now are, in almost every company that we happen to frequent. 1. Is it possible that a sufficient supply of bodies cannot be obtained for the schools without a temptation being held out to wretches to commit the crimes of exhumation and murder? 2. Cannot medical men do without dead bodies, and be content with artificial contrivances in their stead? And, 3. Would it not be better to be without these cruel cold-blooded surgeons altogether than countenance them in the profanations which they commit? This last question, however, we take leave to get rid of summarily, before we go farther: we never reason seriously with any one who proposes it, for we generally find such person to be a wag, a fanatic, or a fool: at all events, we have always found it most convenient to direct such people to the miracle-mongers of Regent-Square, or the humane practitioner in Harley-Street. We therefore conceive ourselves as having to deal with but two sets of inquirers—persons not unwilling to take for granted that there will always exist a necessity for physicians and surgeons in the world, and that if these said surgeons and physicians may not be permitted to learn their profession, so much the worse for their patients—quacks will abound, and manslaughterers must be winked at. Our postulate is simple, but not superfluous; and we can safely recommend it, from experience, as the best possible *shibboleth* by which reasoners on this subject may test the value of their opponents.

Now with regard to the first question—which comes, we presume, from those who think burking and unburying su-

pererogatory iniquities wantonly and unnecessarily perpetrated by monsters who are blots upon humanity—we admit the full force of their impressions of the crimes and criminals engendered by the present modes of supplying the schools; but unfortunately, constituted as society in this country is—such, we mean, is the state of the laws by which we are governed—the wants of the profession, or of the community rather, afford no slender apology for the existence of such crimes and criminals amongst us. In plain words, we reply, it is impossible that in the present state of things the schools can have any thing like a sufficient supply of subjects without exhumation and murder: nay, what is worse, even *with* the aid of these atrocious enormities, the supply is exceedingly scanty, and must every day become more and more so, by reason of the increased vigilance of the authorities employed in the suppression of crime. But in desperate emergencies, proportionate efforts of desperation will always be made; nor will miscreants be found wanting as long as their services are required. And surely the time is now come, when, if ever, the danger of ministering to the wants of anatomical science is at the highest. Never was the risk of life more obviously to be incurred, or the temptation to risk it more alluring; yet have we no hesitation in saying, that unless the strong hand of power be exerted to put down the schools altogether, they will still be supplied (however scantily); nor will desperadoes be deficient to attempt that supply, at whatever hazard.

It was thought at one time that nothing could go beyond the following picture of iniquity. Two officers of police are examined before the Anatomical Committee:—

“ You pretty well know who the men are in London who pursue this trade [that of body-snatching]?”

“ Yes, we cannot help knowing

them : we are out all hours, and *running against them*. * * * I have recovered between 50 and 100 bodies for persons whose houses have been broken open, and the body stolen out of the coffin prior to their being buried."

"Do you think many bodies are obtained by breaking into houses and stealing the bodies previous to burying them?"

"There have been *a great many*. * * * I have known many cases in which the different parties of resurrection-men have quarrelled, and have broken into one another's houses and stolen the bodies before they could be conveyed to the theatres ; and they have been so violent that they have *cut a body into pieces*, and carried it to the opposite party's house, and raised a mob there."

"Have you heard, that to destroy the work of another, they go to the churchyard, and leave the coffins standing upright?"

"Yes, I have known them *fight in the graves*."

"Then you probably think the resurrection-men, with some few exceptions, the most worthless class in the community?"

"*The most dreadful*."

Yet these were only a sample of the resurrection-men who flourished before the iron age of Burke and his successors was revealed ; these were never once suspected to be capable of the *coup de grace* which promises to render the present æra infamous in the annals of crime. But enough of this odious topic, on which we fear we have digressed unreasonably : to return, then, our answer to the first question is briefly this—there is *no* possibility of getting any supply for the anatomical schools without an infringement of the law, by exhumation at least ; and with this, under the most favourable circumstances, the supply is but scanty. The inference, of course, is, either that some effective expedient must be adopted by the legislature, and sanctioned by general consent, whereby the schools may proceed in their honourable pursuit uninterruptedly, or, exhumation must at

least be connived at, and the present precarious supply continued. The latter alternative, we need scarcely remind the reader, perpetuates and encourages the dangerous order of miscreants whose deeds are beginning to cry aloud for vengeance.

It is full time now to come to the second question—and an important one it is, as it forms the strong hold and refuge of most of the puny babblers in society. They have learnt how to shift it about, and to present it in a variety of shapes. It is a broad question of substitutive expedients, which, one by one, demand a sufficient answer : there may be fifty of them, with an *et cetera* at the end, and the challengers may fly from one to another with consummate skill and untiring pertinacity. But we shall try to draw a moderate circle round them, and reduce them to a limited number of their ingenious devices. "Why," says one, "will not the teachers of anatomy and surgery avail themselves of the elaborate and learned works of their predecessors ? there they will find ample plates and eloquent descriptions ; surely these should suffice, without mangling and hacking up the recent subject?" "Why," says another, "will not the dissection of animals, of cats, dogs, sheep, monkeys, &c. answer every purpose, without inflicting indignities on the human dead body?" "Why not employ wax models?" says a third : "nature can with these be imitated in the nicest point, and they are beautiful to look upon." "Why," says a fourth, "are not drawings multiplied in profusion, and made to represent every possible part of the human frame?" And, lastly, "is there no machinery whereby the human body may be so represented in wood, leather, cloth, silk, and so forth, as that operations may be learnt upon it without practising on the actual flesh?" These, we believe, are the principal subordinate

queries into which the proposers of the second question usually resolve it. To each of them separately we shall offer a brief remark or two; but first we shall take the liberty of suggesting that they are all confessedly intended to point out mere substitutes for a genuine commodity. We could, if we were at all inclined unceremoniously to cut short the argument, readily do so by one plain practical question. In the event of some serious accident, or acute bodily suffering, befalling one of our nearest connexions, and that we conceived life or death to depend on the choice we made of the practitioner whom we should call in, would we hesitate a moment, *ceteris paribus*, between him who had laid the foundation of his professional knowledge by the practice of human anatomy, and him who had obtained never so high a character by his operations on machinery, and his contemplations of lithographs and wax-works? But we will not press for a reply; but rather, for argument sake, say something distinctively on the make-shifts.

With respect to the "elaborate and learned" works of the elder anatomists—with all due regard for those eminent authorities—we submit that there has been no art so slowly progressive—none in which the principles and facts have been laid down with more uncertainty by the early writers—whether owing to the precarious opportunities of observation which they enjoyed, the rage for paradox and discovery which sometimes possessed them, or the imperfection of the arts of engraving and wood-cutting at the times in which they wrote; added to which we may mention, that even if they were at all valuable as substitutes for the thing itself which they profess to describe, and not so full of mistakes as they really are, they still are very costly, scarce, and inaccessible. No books are more obsolete than old ones on descriptive anatomy—no books more

curious for their fanciful but dangerous errors. Every modern lecturer and demonstrator on the human body is well aware of this, and finds it his duty to expose the fact to his pupils—to guard them against the speculations of the old observers.

It is to the modern systems of anatomy that the student must have recourse for guidance and assistance in his labours: they will serve to direct his pursuits, but that is all: they can no more make him a practical anatomist or surgeon than reading all the books that were ever written on chemistry could make a dextrous experimentalist of one acquainted with apparatus only in engravings and wood-cuts.

The proposal to substitute comparative for human anatomy, is absurd upon the very face of it. It is true that the dissection of animals can throw much light upon the structure and functions of the human body; but only collaterally and by comparison, as the very name given to the study implies. It was by a fatal, though perhaps unavoidable proceeding of this sort, that many glaring errors have been set down in the works of the fathers of physic. We should like to know how the matter would be mended if the rising generation of anatomists were to be put off with a supply of monkeys for men—though the substitution of "monkey" for "human shambles," might serve, perhaps, to stop old Cobbett's mouth. What a spectacle for Europe and America, to behold cargoes of monkeys from the Brazils and from Africa, to be killed and dissected for the education of British anatomists! Why it would be a sort of murderous slave-trade revived.

Then, as for wax-works, they are certainly, as we have seen them in some modern specimens, "very beautiful to the eye," and, being truly modelled after nature, very useful as a help to the memory of those who have already dis-

sected: but the pieces must be innumerable, if we wish with them to supply the place of any required dissection; and for this the expense must be most enormous. For colleges, and the museums of wealthy individuals, a set of wax models must be a most desirable collection; but it is ridiculous, in point of genuine utility, to compare them with dissections of the recent subject, executed with the student's own hand.

The same objections which belong to wax-works apply also to drawings or engravings—which *must* be multiplied, and therefore become intolerably expensive to the great majority of medical students. And, by the way, we have never heard how surgical operations are to be taught on either drawings or wax models.

But these, forsooth, may be taught on machinery: an artificial *man* of some sort is to be constructed, and the operations thus to be demonstrated. Preposterous! Why, surely, these people must think that Frankenstein is no fable, and that Prometheus really stole fire from heaven. Nothing comes amiss to their credulous maw. We really feel more than half inclined to send these notable speculators to the same limbo of vanity to which we adjudged the propounders of the third question.

And now that we have fairly met every objection, bearing upon its front the show of reason, against that side of the argument which we have adopted, we think we may venture to close this part of our remarks. Our object was popularly to point out the folly and ignorance of those who raise a cry against dissection, on the supposed ground of its being not indispensable—as if medical men took up the knife “to mangle the dead,” out of some inhuman propensity to commit a wanton outrage. We have shown, we hope satisfactorily, that none of the *humane* expedients proposed by the opposers of

dissection, have the least value in comparison with the actual human dissection itself—and that, therefore, the latter being indispensable, unless medical science be to be thrown back into primitive barbarism, we are bound to forward its interests as far as in our power lies. In our next, we hope to have an opportunity of stating more particularly what we think the friends of anatomy ought to do. It so happens that certain recent occurrences, however shocking to humanity, seem favourable to the prospects of the medical art: and, if they are, they must be almost as directly subservient to the well being of the community at large. None, we are sure, but the most “obstinate malignant,” will presume to impute selfishness to the stir we now make; but, however that may be, no influence of so sinister a description shall ever have the least weight with us in deterring us from our purpose.

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REMEDIES TRIED AT SUNDERLAND IN CHOLERA — OTHERS SUGGESTED.

WE mentioned last week that a hog's-head of brandy, already mixed with laudanum, in due proportion, had been furnished by government for the use of the cholera patients at Sunderland. We regret to say, that little benefit has been derived as yet from this, the volatile oils, or any other method of treatment which has been tried; nor has any thing occurred to throw any additional light either on the nature or treatment of the disease. The inhalation of oxygen has been tried in several cases without any apparent advantage; the pulse, indeed, rose a little during the inhalation of the gas, but immediately on leaving it off, the sinking of the pulse was found to be greater than before its use. Some nitrous oxide was in preparation a few days ago, and ere this, we doubt

not, has been tried. We must confess, however, that we entertain little hope from this class of remedies, because the lungs do not act upon that portion of oxygen which the air naturally contains, it having been long ago ascertained by Dr. John Davy in India that only from one-fourth to one-third of the proper quantity of carbonic acid was found in the air expired by those labouring under cholera. A galvanic battery has been ordered to be sent to Sunderland, and the effects of this most powerful agent are to be ascertained. The experiments of the late Mr. Finlayson, at Ceylon, though too limited to warrant any general inference, were certainly such as to justify farther trial: in one of his patients, who was "moribund," a galvanic current from a small battery was passed through the chest, the man immediately revived, and ultimately recovered. The same gentleman also states, that in two out of three cases, the functions of the lungs appeared to be restored by stimulating them with ammonia, volatilized so as to impregnate the atmosphere with its fumes.

We earnestly recommend a trial of injecting medicated solutions into the veins, particularly some of the neutral salts, as muriate of soda. No one who ever saw a leech disgorge its black blood upon salt can fail to have observed its instant conversion into a brilliant scarlet. That the change of colour carries with it a corresponding change of character, we do not venture to assert; but it is at least worth ascertaining; and we think that the views of Dr. Stevens, as to the effect of salts on the blood, to which we have repeatedly called attention, ought to be put to the test as speedily as possible. It affords no mean claim to the investigation of these doctrines, that a man of Dr. Prout's high character should have declared the essay containing them, and which was published in this journal, to be one "which, spite of

some imperfections, seems to contain the germs of discoveries of the last importance to mankind."

CONTAGIOUSNESS OF CHOLERA.

SOME of the gentlemen at Sunderland, who doubted the contagious nature of cholera, do so no longer. One of them informs us, "that the contact of the body after death is more powerfully infecting than during life."

CHOLERA RETURN.

THE total number of cases of cholera at Sunderland, from the 6th of October to the 30th of November, was 319; of whom have died 97; being, in fact, almost all of those in whom the disease assumed malignant characters.

CHOLERA *versus* LIFE ASSURANCE.

WE alluded, now some months ago, to the curious circumstance, that while the cholera appeared in Europe, though yet at a distance, the number of persons in this country who insured their lives underwent a sensible diminution, a circumstance which we attributed to the apprehension, that if the malady should prevail extensively here, the contracting parties would be unable to meet their engagements. That the value of human life will be altered to a greater or less extent, is quite obvious; but we understand that some of the Assurance Companies regard themselves as secured by a bye-law, which provides, that in the event of any plague or pestilence prevailing in this country, the heirs of persons dying of such malady shall be entitled to receive only a certain proportion of the sum assured. We have been informed, that at a meeting of one of the most opulent of these societies, held a few days ago, the existence of such provision was alluded to:—if so, we crave leave to ask, how far a bar-

gain will be held binding in law, to which both assurer and assured were not parties? We remember that Lord Eldon, at the time of the bubble mania, when so many new Companies were started, distinctly stated, that they might make what bye-laws they pleased for their own internal regulation, but that nothing involving the interests of the assured would be held good in a court of law, except what appeared on the face of the policy. The question may become a very important one.

CHOLERA AT HAMBURGH.

WE have been favoured with the perusal of an account of the irruption of cholera at Hamburgh, by Dr. Fricke, a physician of some repute in that place, which has been transmitted to the authorities in this country.

The writer professes that his object is to shew that the disease was not imported, but originated in local causes, not of a very obvious nature, nor easily admitting of description. As a consequence of this position, it follows that the malady cannot be exported, and therefore that "the onerous quarantine" ought to be abandoned, more especially as regards the admission of Hamburgh vessels into foreign ports. Such are the declared objects of the writer; how far his inferences are warranted by the facts detailed, we shall leave the reader to decide.

No cases were known to exist nearer than 30 miles from the town, till it appeared within its walls—on the 5th October. Reference, however, is made to a case anterior to this, which deserves to be mentioned. A sailor belonging to a vessel performing quarantine died of a bowel complaint, into the history of which a medical committee was appointed to examine. They found that the individual had been received on board at Wittenberg on the 7th of September, at which time he had a looseness, but not to such an extent as to prevent him from attending to his business. On the 23d he became much worse, and died on the 2d of October; the case, however, was declared to have been one of "chronic diarrhœa." The vessels alluded to was discharged from quarantine, and admitted into the harbour among

the rest of the shipping on the 5th of October. The first person who is stated to have been attacked with cholera was one Peter Petersen, a drunken pauper, who lived partly by begging, and partly by selling things about the streets. He was in the habit of remaining away from his lodging during several days and nights together, and was absent on one of his wanderings on the 20th and 21st of September, during which time it is first stated that he had visited none of the shipping, but subsequently an exception is made with regard to certain barges laden with fish, potatoes, and cabbage, the latter having evidently come down the river from the interior. This person was taken ill on the 5th of October, and died next day. He lived in a cellar in Nicholas-Street, a narrow lane, within eight hundred feet of the harbour. The house appears, from the account, to have been a large old building, the lower part of which, under the level of the street, was occupied by a little colony of vagabonds of both sexes, who lived promiscuously; there were forty-one of them in all, and of these eight were foreigners.

The second case occurred in the person of a female, residing in the same cellar, in which, by the way, seven persons slept. She had not been out for several weeks, till the 5th, when she went to assist in digging potatoes: on her return, Petersen being ill, she attended him as nurse. She was seized with cholera on the evening of the 7th, and died on the 8th. The manner in which the obvious communication of the disease in this case is attempted to be got over, is a curious illustration of the extent to which men will sometimes shut their eyes, that they may not see what would otherwise be most conspicuous. The woman, we are gravely told, had no connexion with Petersen, "with the exception that she attended him as nurse during his short and rapid illness"—(als dass sie solchen bei seinem kurzen Krankenlager gewartet hatte.)

The third case was that of a waterman, named Somers, the particular friend and companion of Petersen, who had accompanied him in his wanderings: he was seized on the 7th, and died on the 10th. This patient, "on the night on which Petersen was taken ill, had slept with him, and subsequently attended upon him during his illness."

On the 8th of October, two

more cases occurred in the same cellar. From this time cases began to appear in other individuals, dwelling in the same neighbourhood. Thus, on the 8th, a person named Neuer was taken ill; he had worked in the dock-yard on the two preceding days. On the same day one of the crew of the *Atlas*, a vessel lying in the harbour, was also seized with the malady; and three other cases occurred in the vessel within ten days. On the 9th of October the presence of the disease at Hamburg was declared in an official document signed by the principal medical men of the place; and on the 14th a proclamation was issued by the magistrates, calling upon the people to bear the visitation with fortitude, and not to aggravate the evil by any attempt at concealment: "a mere denial of the disease being among us (say they), cannot alter the views of foreigners as to its existence, but may facilitate the spreading of the malady, and prevent that immediate assistance which the sick require from being rendered." Let the good people at Sunderland take the hint.

Upon the whole, we have never met with a more curious document. The facts are interesting, and seem to be trust-worthy, inasmuch as they make directly against the purpose of the writer; so that he cannot be supposed to have admitted them otherwise than because they were too notorious to be denied. The chain of connexion between the first case and those which immediately followed, is so obvious and unequivocal, that, unless we had seen Dr. Fricke's account, we could not have believed it possible that such facts could have been adduced by any man, to prove that the disease was merely dependent upon local causes—had not been communicated to the Hamburgers from without, and could not be transmitted by them to others;—yet, so it is.

A PRETENDER AT SUNDERLAND.

THE good people of Sunderland are, some of them at least, very odd personages. A correspondent of the *Times*, in Monday's paper, begins his epistle in this inflated way:—

"This is an age of pretenders, and to guard against them I think it proper to bring to your recollection, that *I* was the first observer who discovered, or at least made the fact publicly known, that

the different forms of cholera at present spreading in Sunderland were one and the same disease—that disease being the Indian cholera!" Prodigious! the discovery is worthy of a Jenner, or a John Hunter; but what, in the name of common sense, is the meaning of the first part of this pompous announcement? That as *pretenders* (this is our conception of it) *are abroad*, our best preservative against them is, *to recollect* that the writer in question was *the first who discovered* that the cholera at Sunderland was the Indian cholera! If we are right, then, once more we say prodigious!—but evidently the sentence, with that signification, is somewhat elliptical: it will be but a lame and impotent recollection, if we do not please to remember at the same time, that he who discovered the disease may also have discovered the remedy.

But proceed we to another sentence. Our great discoverer, after thus warning us of the present existence of pretenders, and jogging the Editor's memory as to the great claims of a certain person, presumes to make a thrust at Dr. Barry and *his pretensions*. "Dr. Barry, who has been here for some days, did not, *it is said*, entertain any suspicion of the circumstance, until he heard of *my opinions* yesterday, and I am informed that *they excited his surprise*." So well they might, notable mare's nest as they are! But, of course, in this gentleman's estimation, Dr. Barry is no more than one of the many pretenders who are abroad; and this brings us to issue with our letter-writer on the matter of fact. Did Dr. Barry really entertain doubts about the identity of the disease at Sunderland and what he saw at St. Petersburg, until surprised by our discoverer's opinions? We cannot take credit to ourselves for being enabled to set this matter right by an exclusive statement, for the learned physician's letter on the subject is now before the public; but we *know* that Dr. B. had not seen above two or three cases of the Sunderland cholera when he was convinced of its genuine character, and immediately apprised the authorities here to that effect: his words were, "it is identical with the disease of which Dr. Russell and myself saw so much at St. Petersburg." That the sapient correspondent of the *Times* was not acquainted with Dr. Barry's convictions, we can readily conceive; but the insinuation conveyed in the epistle is grossly

presumptuous and absurd, even though it is conveyed cautiously under the form of an *on dit*.

After some more conceited nonsense of the same sort, our notable discoverer favours us with the announcement, that he thinks, with certain philosophers, that "every thing has two handles—a right one and a left:" we apprehend that the gentleman has taken the trouble of going down to Sunderland to catch hold of the wrong handle—at least, in the present instance, he has acted with consummate awkwardness; and his disguise drops off but too egregiously before he leaves us. In the last paragraph he slips in a piece of vile puffery about a "Mr. Kennedy, a London practitioner, the author of a work on cholera;" and this we take to be the key to the whole production. That the person here alluded to is disappointed in consequence of not being employed by the Board of Health, is by no means impossible, as we find him still *unattached* in Sunderland. But what in the world, even though Mr. K. were "a London practitioner," have his opinions *in that capacity* to do with the question one way or other?

DR. BARRY ON CHOLERA.

SHORTLY after the arrival of Dr. Barry in Sunderland, he requested a meeting of all the medical gentlemen of the town, and in consequence a most numerous assembly was convened in the Exchange rooms. The high reputation which Dr. Barry had so justly obtained was not lost upon the faculty of the town, and at the meeting much valuable information, particularly as regards cholera, was accordingly elicited.

Dr. Barry having detailed the symptoms of the Russian cholera in a very lucid and graphic manner, (viz. the vertigo, the prostration of strength, the peculiar evacuations from the stomach and bowels, the spasms, the collapsed countenance, the whispering voice, the blue skin, the cold tongue and respiration, and the arrest of the circulation) declared that the Sunderland epidemic and the Russian cholera were the same disease, the diagnostic marks being so prominent as to render it, when once seen, not easily to be mistaken.

Dr. Barry, when speaking of the pathology of the disease, stated that in

examinations after death he had generally found congestion of the great vessels, and very frequently effusion of blood in patches beneath the mucous membrane of the stomach and intestines; the gall-bladder always distended with bile; the urinary bladder contracted, and containing either nothing, or a minute quantity of whitish fluid; and the intestines void of any feculent substance; but the most remarkable morbid appearance, and that to which he would attach the most importance, was the phlogosed and softened state of the spinal marrow, which in a greater or less degree was always present.

With respect to the treatment, he would recommend full vomiting, in order to emulge the gall-bladder, which might be produced by a strong solution of common salt in water, given in doses of six ounces. Warm, dry applications to the skin, and especially patient and continued friction, he had found very beneficial; whilst moist warmth, all kinds of vapour and water baths, he considered worse than useless. Bleeding, as well as large doses of either opium or stimulating liquors, were dangerous in the collapsed state, when the powers of life were reduced to the lowest ebb, and consequently easily annihilated; but when re-action was beginning to take place, or previous to the state of profound collapse, he believed that blood-letting would be of benefit to the patient. A practice which he would strongly recommend in this country (should the disease unfortunately progress,) but which circumstances would not allow him to use on the continent, was the application of the actual cautery to the back, opposite the lower dorsal and upper lumbar vertebræ: this had been done with success by a celebrated continental physician, and he (Dr. Barry) thought it a mode of treatment highly worthy of trial.

In answer to a question put to him, Dr. Barry stated, "that he had seen no case of *common* cholera at Sunderland."

TEMPERATURE OF THE HUMAN BODY.

SOME years ago, Dr. John Davy made a set of experiments with a view of ascertaining the changes, if any, which the temperature of the body undergoes

in different parts of the globe, and the result was, that some, though but a slight elevation, occurs in warm climates, and a proportional decrease in cold countries. These experiments were lately repeated by M. Regnaud, of Paris, during a voyage in the eastern hemisphere; and as the thermometers used were compared by M. Arago with those of the Observatory in Paris, their accuracy cannot be questioned. Four sets of observation were made in the torrid zone, and three in temperate climates; while all were made on the same twelve individuals, robust and healthy men, fed, clothed, and lodged, in the same manner. In $10^{\circ} 4'$ north latitude, the air being at 79° of Fahrenheit, the average temperature of all the twelve was $99\frac{1}{3}$ degrees; in $36^{\circ} 10'$ south latitude, the atmosphere being $62\frac{1}{2}^{\circ}$, the average temperature of the men was $98\frac{2}{3}$ degrees; under the line, and with an atmospheric heat of 86° , the animal temperature was 100 degrees. In latitude $7^{\circ} 1'$ south, the air being at 86° , the body was $99\frac{1}{2}$ degrees; and, again, the air being at $62^{\circ}\frac{1}{2}$, the body was at 99 degrees, and the atmosphere rising to 79° , the body rose to $99\frac{1}{2}$ degs. Thus the atmospheric range was from $53\frac{1}{2}^{\circ}$ to 86° , which produced a variation in the animal heat of only one degree and a half.

WESTMINSTER MEDICAL SOCIETY.

Saturday, Nov. 26, 1831.

DR. SIGMOND IN THE CHAIR.

Nature and Treatment of Cholera.

DR. JAMES JOHNSON said, that with a view of economizing the time of the society, of introducing a systematic mode of discussing the subject, and more particularly with a view of avoiding the necessity of repeated explanations, and preventing misconception, he had taken the liberty of throwing his observations into a series of short propositions, distinct from each other, by which any member, or members of the society, would be enabled to discuss any one particular point without diverging into irrelevant matter. The propositions, which the doctor read from a written copy, were as follows:—

I.—That in epidemic cholera, as in most other epidemics, a poisonous or sedative principle, whether originating in the earth, or emanating from animal or vegetable substances, strikes a predisposed individual, and

after an uncertain period of incubation, produces a train of phenomena forming the subsequent disease. In sporadic cholera the diffusive power is absent; but when the common exciting causes are strong, and the subject highly predisposed, severe or fatal cases will occur which cannot be distinguished from those of epidemic cholera.

II.—The effects of the choleric poison exhibit a great analogy to those produced by the virulent contagion of typhus, and the concentrated miasmatic exhalations that give rise to malignant fevers, remittent and intermittent, such as we see in Batavia and other highly malarious places.

III.—This poison shews its effects, according to the evidence of our senses, first on the nervous system, as evinced by the prostration of strength, by the affection of the head, by the absence of the secretions; and, in fact, by a depression of the whole of the sensorial functions as well as those of organic life.

IV.—The secondary effects of the choleric poison are shewn in the vascular system; the circulation recedes from the surface, the blood accumulates in the internal organs, the decarbonization and calorification cease or are greatly diminished, the temperature of the body falls to that of the surrounding inanimate substances, paleness is changed to blueness, and the functions of the ganglionic system of nerves seems to be suspended, if not annihilated.

V.—It is at this period that nature appears to make violent, but too often unsuccessful, efforts to restore the balance of the circulation, and to re-establish the secretions by sickness and purging, the ejected fluids being exudations rather than secretions.

VI.—If nature do not succeed by the above-mentioned efforts in restoring the circulation and secretions, and consequently oxygenation and calorification, these efforts themselves prove auxiliaries to the choleric poison in destroying life.

VII.—We are not in our present state of knowledge certain whether the spasms be merely the effects of the poison on the nervous system, or an effort of nature to resist it; but they, like the sickness and purging, if not immediate, tend ultimately to excite the powers of life.

VIII.—If nature, by which I mean the constitution, whether with or without aid, be able to resist the first or depressive shock of the poison, and institute a re-action of the system, that re-action, in a great majority of cases, becomes a fever, exhibiting a new train of phenomena, and demanding a different treatment. If this view be correct, it would lead to the inference, that what is understood by cholera sometimes constitutes the first stage of choleric fever.

IX.—If re-action, if restoration of the circulation and secretion, and oxygenation,

do not take place, the patient dies in a state of asphyxia, the intellectual powers often remaining but little impaired till the last glimmer of the lamp of life is extinguished. This has been often witnessed in concentrated miasmatic fevers within and without the tropics.

X.—Pathology. All the changes which present themselves in the dead body are, in my opinion, effects, not causes of the disease, with the exception of the congestion of black blood in the internal organs, which is almost the only phenomena observable when cholera terminates fatally in a few hours. The traces of inflammation in various organs after death are connected with the reactive fever rather than the cholera which precedes the fever.

XI.—Treatment. As we have no means of expelling or neutralizing the poison, we can only endeavour to counteract its effects, and assist nature in her movements.

XII.—The primary or essential indication is to restore the equilibrium of the circulation. If that equilibrium be effectually restored, secretion, calorification, and oxygenation follow.

XIII.—The balance of the circulation is to be restored partly by internal, partly by external means, but always by several means simultaneously employed at the very earliest period of the disease.

XIV.—Venesection may appear a desperate remedy, but we have a desperate disease to combat. I proposed this measure many years before the epidemic broke out, and it has been adopted to a very considerable extent both in Asia and in Europe; and I proposed, and would still propose, venesection with a two-fold view; first, to relieve the heart and internal organs from a portion of that deluge of black blood in which they may be said figuratively to be drowned; secondly, to turn, as it were, the tide of the circulation from the centre to the surface of the body. This measure I would chiefly confine to the young, the robust, and the previously healthy, and in them contemporaneously with, or soon after the measure which forms my next proposition.

XV.—The first internal remedy which I propose, both in aid and in imitation of nature, is a stimulant emetic, as an infusion of mustard-seed, or, what perhaps would be better, sulphate of zinc. I propose this from a conviction, founded on observation, that of all the means which nature or art can bring into operation, the act of full vomiting is the most powerful, driving the blood from the internal organs to the surface of the body, and it is also the most efficacious means of exciting the secretion in every glandular structure of the living machine. Nausea and retching are quite different in their effects from the operation of full vomiting. Nausea and retching depress

the powers of the heart and nervous system, and prevent the blood from flowing to the surface. Full vomiting impels the circulation with great force into the superficial vessels, for it is extremely difficult to stop the flow of blood from the orifice of a vein during vomiting. I have seen the blood come from a vein under such circumstances with all the characters, or at least all the appearance, of arterial blood. This proposition is well exemplified by sea-sickness, of which I have had painful personal experience. An unfortunate landsman bears a close resemblance during the first storm he experiences at sea to a man with cholera. He staggers about the deck, striving to keep down the rebellious heavings of his stomach, but he cannot succeed, up it comes; and during the first vomiting, I have seen the blood gush from the mouth, nose, and eyes of the sea-sick sufferer. From being actually blue with nausea, his face becomes red with vomiting; but the cause of the sickness still continuing, he ultimately becomes pale and exhausted. He is like a man who takes a fresh dose of emetic tartar after each paroxysm of vomiting, and the wonder is that he does not die: I have known some die of it. It is curious that Cælius Aurelianus, who gives the best ancient account of cholera morbus, places sea-vomiting among the causes of that disease. It is but justice to state that Mr. Boyle proposed full vomiting in epidemic cholera ten years ago; and I have the best authority for recommending this, because when it was pursued on the continent, it was eminently beneficial.

XVI.—As soon as vomiting has produced its salutary effect on the circulation, or has failed to produce that effect after a fair trial, I would propose diffusible stimuli, with calomel and opium, but not in immoderate doses. Brandy and laudanum, as being most readily procured, and the least likely to be loathed, are perhaps the best; but the choice of stimuli must be left to the practitioner, and the danger of inducing subsequent inflammation should be constantly borne in mind. Calomel alone would probably be the most successful after the emetic.

XVII.—The remedies above mentioned, in moderate but efficient doses, seem to impart vigour to the heart and nervous system through the medium of the stomach. While they mitigate the spasms, and restrain the injurious exudation of fluids from the intestinal canal, the mercury changing these exudations into secretions.—*Qu.* Would not the inhalation of oxygen gas be beneficial?

XVIII.—The external remedies are three—heat, friction, and counter-irritation. These three means should be employed, not only simultaneously with respect to each other, but contemporaneously with the internal remedies. They should always be so employed, that the patient may not be suf-

ferred to throw a single voluntary muscle into action : every muscular movement, even that of sitting up in bed, is prejudicial, or absolutely dangerous during the exhausting orgasm of cholera. An apparatus, permitting the application of heat, friction, and counter-irritation, without the necessity of muscular exertion on the part of the patient, will be shewn to the society. As the cramps and spasms are chiefly confined to the extremities, would not firm compression by bandages mitigate them?

XIX.—Prophylaxis. Considering myself, as well as every member of this society, bound to abstain from all discussion on contagion, I shall consider this subject in four words—temperance, cleanliness, ventilation, and fearlessness: in fine, the presence of all those things which tend to preserve general health, and the avoiding of all those causes which predispose to common diseases.

He (Dr. Johnson) had thus endeavoured to throw into a series of propositions the chief points for discussion. He solemnly protested he would not defend a single proposition for the sake of argument or victory, but renounce all, or each of them, the moment he was convinced of their or its instability. The learned doctor then exhibited the apparatus of Mr. La Baume, for applying hot air or vapour to the surface of the body.

Mr. BOYLE had visited India; and remaining there for some time, he had an opportunity of seeing a great deal of cholera. He had great opportunities of making post-mortem examinations, in the course of which he endeavoured to ascertain the cause of the absence of bile. The general appearances discovered by him were already on record, and consequently he would now merely remark, that, in all fatal cases of cholera, no bile appeared, whereas in every favourable case it did. He sought for the cause of that phenomenon in the biliary ducts, and frequently found them so much obstructed as to experience a difficulty in passing a probe. The gall-ducts were distended with dark-coloured bile sufficient to obstruct them, independently of the spasmodic action which there was reason to suppose existed contemporaneously. The unsuccessful application of the various remedial agents that were recommended, together with the facts he had just stated, induced him to believe that, if a remedy could be administered which would produce bile, the appearance of which was always salutary, and the absence of which always indicated a fatal termination, it would be the best agent that could be employed. He also found, contrary to general opinion, that there was not such a degree of vomiting and purging as was generally imagined in the *worst* cases of epidemic cholera; on the contrary, that where those

symptoms occurred, together with violent spasms, the patients generally recovered. If the patients were very bad, there was a great collapse; the powers of life seemed to be destroyed, or paralysed, at the moment of attack. If patients merely made unsuccessful efforts either to vomit or to unload the alimentary canal, they generally died. If a patient, when interrogated, merely said that he felt unwell, but could not describe the nature of his case, fatal consequences most probably ensued; whereas, if, on the contrary, violent action occurred, accompanied with violent spasm and serious complaints, and sufficient power remained to enable him to discharge the diseased matters, that patient generally recovered. He thought those observations worthy of remark, because he presumed that, if a gentleman who had not previously seen the disease were called to enter a room in which two patients were lying in the circumstances he (Mr. B.) had just described, he would most likely pronounce an inaccurate prognosis. It was from those circumstances that he was led to exhibit emetics, and in every case where he had an opportunity of doing so, they proved successful. He did not wish to attach more importance to that fact than it deserved, but he understood, from good authority, that emetics had proved highly serviceable on the Continent; certainly not in every case, but they had been found to be the most successful remedies. He was first led to introduce the practice from analogy, and few cases which he had treated by emetics and other auxiliary means proved fatal. Having thus stated the reasons why he adopted emetics, he would now offer a few remarks upon other remedies. [Mr. Boyle here made some remarks on bleeding, and afterwards on frictions and other external applications.] As to the internal remedies, in addition to an emetic, he thought that benefit might be derived from calomel, opium, brandy, and ammonia and aromatic confection. It was not wonderful that calomel should have been brought under the notice of the profession as an important remedy in cholera, in consequence of its efficacy in the treatment of diseases generally in tropical climates. In bad cases of cholera, however, he had never seen calomel produce any effect whatever, and, in post-mortem examinations, he had found it adhering to the mucous membrane of the stomach and the duodenum unaltered. Ammonia he looked upon as the most important stimulus with which he was acquainted in the depressive stages of all diseases, whether acute or chronic. Compound spirits of lavender he considered an excellent stimulant, combined with brandy punch. It was impossible for him to follow Dr. Johnson's propositions in detail, but there were two or three points to which he would allude, and the first related to cramps. Having seen a

number of patients at the moment of being taken ill, without cramps, he did not think that they were a cause, but a symptom of the disease. He did not believe that all the appearances observed in post-mortem examinations were causes, but that some of them were consequences of the affection. The distention of all the interior vessels he believed to be consequences; but the obstruction of the bile ducts, and the dark or green colour of the bile, he looked upon as the immediate causes of the affection.

DR. GREGORY remarked, that he thought it would contribute to a clearer understanding of the subject, if the *nature and treatment* of the disease were discussed separately; and he recommended, that the society should devote itself, in the first instance, to the question of the *identity of the common with the Asiatic cholera*. It was admitted on all hands, that the disease now prevailing at Sunderland was identical with that which had prevailed in Russia and in India subsequent to 1817; but it was still a matter of controversy, whether it was the same disease as that described by *Sydenham* under the title of cholera, or as that which had occurred in India prior to 1817. To determine these points was not a question of idle speculation, for upon their decision it depended, how far we could benefit by the experience of former times, in regulating our treatment of the present epidemic. For himself, he had already avowed his conviction, that the Asiatic cholera was truly a *nova pestis*, unlike any disease hitherto described under the same name. Still he thought the question open to investigation, and peculiarly fitted for discussion in this society. For the present, he merely threw out this suggestion with the view of guiding the society into a profitable path, reserving, to a future occasion, a more extended examination of the question.

MR. SEARLE said, that cases of the disease which had occurred at Sunderland had been submitted to his inspection, and he felt no hesitation in stating that it was the same as that which he had seen at Warsaw. His observation was not founded upon any of the symptoms, but upon post-mortem examinations.

DR. GILCHRIST made some observations.

DR. COPLAND remarked that Dr. Johnson had taken for granted, in his first and second propositions, that the present pestilence was identical with the severe forms of cholera frequently met with in warm climates, or in warm and moist seasons in temperate countries,—an inference which the symptoms of both diseases, and the experience of many able observers, did not warrant. He stated that he had, on the previous evening, when called upon, referred to the preliminary symptoms, the symptoms observed in its course, and to the consecutive phenomena and terminations of this pestilence, as fur-

nishing decided evidence that it is altogether distinct from the disease called common and spasmodic cholera, and the marks of distinction he was still ready to show and maintain. As to the pathology and treatment of the disease, as stated by Dr. Johnson, he had but little to remark, and scarcely any thing to object. He would only observe that the pathology, even as laid down by Dr. Johnson, furnished striking evidence of the difference existing between this pestilence and common or spasmodic cholera. His views respecting the propriety of combining the means of cure, and directing them simultaneously into action, as well as his recommendation of emetics and bloodletting, very nearly met those entertained by Dr. C. himself, but that much would depend upon circumstances as to the extent to which bloodletting should be carried, and occasionally as to the propriety of omitting it altogether. He conceived that the different grades of the disease, as well as its different periods or stages, required a modified treatment, and in this, he stated, that he was borne out by the experience of Dr. Keir, of Moscow, as detailed in a valuable letter, the perusal of which had been granted him by a friend (Mr. Craigie, Dr. C. informed us) now present. According to the experience of this gentleman, the most intense grade and advanced stage of the disease did not admit of bloodletting; whilst, in the less severe cases, and in the more intense cases when they came immediately under treatment, the advantage derived from the practice was decided. As to the superiority of warm or hot air over the warm or vapour bath, he expressed his belief; and he could recommend, from an extensive experience of the practice in analogous states of the disease to the present, the turpentine fomentations, and the other external measures which he had detailed in a previous number of this journal. The probable advantages of inhaling stimulating and oxygenated gases, he remarked, had been recommended by various writers, and, as the question in which the exciting cause of the disease made its invasion of the frame was evidently the lungs, and, as this organ was paralyzed by its impression, the propriety of trying this mode of medication was evident. Respecting the use of calomel in the disease, he remarked that its utility was undoubted, and that it should be remembered that, when the powers of life were reduced very low, it had no effect unless combined with stimulants, particularly camphor and ammonia.

DR. WEBSTER thought that the question of Dr. Gregory was of great importance, because if they could establish the identity between the disease as it occurred in India, and was now prevailing in Sunderland, and as was frequently seen prevailing in this country, he considered that a great step

would be gained towards arriving at a knowledge of its proper treatment. He had not seen the disease in Asia, but he had seen cholera as it frequently prevailed in England, and as it had prevailed in the southern parts of Europe, and to him it appeared to be the same disease, though it was no doubt modified by the peculiarities of climate in which it occurred, as also by the habits of the individual affected. The ague, which occurred in the fens of Lincolnshire, was the same complaint as that which affected the inhabitants of the Pontine Marshes, or the community of more southern regions, varied, no doubt, by particular circumstances. As facts, however, were of greater importance than declamation on this subject, he would refer the meeting to a description of spasmodic cholera as it occurred in India, and the document to which he would call their attention was a paper contained in the fourth volume of the Medical Transactions of Calcutta, by Mr. Wood. [Dr. Webster here, amid some interruption, read two cases, which terminated favourably, and contrasted them with others, which terminated unfavourably.] His object in stating those particulars was to prove, that the disease, described as cholera spasmodica, was a complaint which had always existed in this country, and had been well described by Sydenham, and as this was the case, he trusted the public anxiety would be relieved. He had seen a letter from Vienna within a few days, describing the disease as it occurred there, and the account given was, that, when the disease first broke out, many fatal cases occurred, because the treatment was not understood, but afterwards public fear was allayed. Gentle emetics were given in the first instance, and the vomiting was directed to be kept up by camomile tea. Whenever that was followed by perspiration, and by a copious discharge of the contents of the stomach, particularly with bile, the patients generally recovered. He (Dr. W.) thought it right to state that fact, in corroboration of what had been advanced by Dr. Johnson and Mr. Boyle. Salt soup was also recommended; ipecacuanha emetics were found to be the best.

Dr. Thomson could not accord with the opinion delivered by Dr. Webster, that the cases detailed by him, and others of a similar nature, were identical with the disease which had lately reached the British shores. As far as he could form an opinion, from the perusal of the Indian reports, and other writings of those who have seen the disease as it appeared in India, there seems to exist a decided difference between it and the most virulent cases of the spasmodic cholera known in this country. Even in the description of Sydenham the distinction is obvious. In one particular, the presence or absence of bile, the diagnosis of the disease now spreading, is well marked. The most striking feature of the present malady, its communicable

character, is evidently one which had never been observed in the ordinary Indian cholera; and, consequently, when the present disease broke out at Jessore, the inhabitants saw sufficient in it to alarm them, and rapidly fled from it. It is, in his opinion, impossible to confound the two diseases; and probably much of the diversity of opinion which has unfortunately prevailed in the profession, may have arisen from the name imposed upon the cholera, and its similarity in some of its features to the ordinary Indian cholera. With regard to the symptoms, the most important, the state of collapse, is that which almost exclusively demands attention. There seems to be a complete paralysis of the nervous system, which, as it were, arrests the function of circulation, and consequently prevents that change from taking place in the blood which results from its passage through the lungs. The blood is accumulated on the heart and large vessels, owing to the vital energy of the circulating agents being nearly destroyed; and this readily explains the pale, cold state of the surface; the absence of bile in the alimentary canal; the coldness of the breath, and many other symptoms. The effect closely resembles that which follows the bite of some of the most venomous serpents, or the action of powerful sedative, vegetable poisons, such as the *upas antiar*, on the living system, that Dr. Thomson could not help believing that, in whatever manner the disease originated, it was now produced by the exhalation of a most powerfully sedative virus from the bodies of the sick, which, applied to those of broken-down constitutions, or otherwise predisposed to receive it, operates by an immediate impression on the nervous system. It is, in his opinion, this paralysis of the nerves which induces all the other symptoms; and, therefore, the first object of the practitioner should be, to sustain the habit under its impression, and to endeavour to rouse the powers of the circulation. The question is, how is this to be accomplished? Dr. Thomson deprecated the use of the lancet for this reason, that the congestion is a consequence of the paralysis of the nervous energy, not its cause; and he could not comprehend, even if blood could be obtained in such a state of the habit, what advantage would be derived from it, as long as the paralysed state remained. If the heart were struggling to expel its contents, and could not accomplish it, then he would accord with the propriety of blood-letting, but not otherwise. He believed, that in those cases which had been benefited by it the bleeding had preceded the state of actual collapse. He strongly recommended stimulants in this state, such as ammonia and volatile oils; and effects had been recorded of the power of the former, in the experience of some of the Indian practitioners, which were almost marvellous. Dr. Thomson concluded by remarking, that any thing which he could

offer to the Society on this subject was of little value, as he had never seen the disease: let him once see it, and then his mind would be soon made up as to the manner in which it ought to be treated.

DR. WHIRING, in conjunction with all his medical brethren, had had several cases of cholera during the last year, but he had not had one fatal case. He had seen cases very severe—he had seen cases lying in a state of collapse, almost in a moribund condition, but these patients recovered. That was not the case, however, with the malignant form of the disease, as it occurred abroad and at Sunderland; there the patients died, and consequently he saw in that circumstance a certain degree of difference in the affection. If that were sufficient to establish a distinction in the two cases, then the question inevitably arising was, "Is there any difference in the mode of treatment to be adopted?" If that question received a negative reply, then where was the necessity of making a distinction between the diseases? He might be allowed to say that he believed they were obliged, in this disease, to be guided by principle; for they did not know wherein the essential nature of cholera consisted. It had been remarked by Dr. Johnson, that there was an impression made on the nervous system; but what that was they knew not; they only knew that the effect produced by the impression was subsequently manifested principally through the medium of the circulation. If, then, that were the case, they must prescribe for the symptoms of the disease; for at present they had no specific for it. Now, what were the symptoms? They were such as were seen in many cases, not only of cholera, but of typhus fever, and occasionally in other diseases where there was a great collapse of the system, an overloaded condition of the heart and large blood-vessels, and a deficiency in the capillary circulation. If such were the case, why should they make a difference in the treatment? They acknowledged they did not know the nature of the disease—they acknowledged they had no specific for it—and therefore let them take the symptoms and prescribe for them, whether they occurred in typhus fever or in cholera. It was in that way that judicious medical men were now looking at the treatment of cholera—it was in that way that they had treated all sporadic cases—it was in that way they had treated typhus fever, and it was that way Dr. Mackintosh had been treating severe cases of ague in the cold stage, when that stage proceeded to an extreme degree. He must acknowledge, however, that he did not accord with some sentiments which had been expressed respecting blood-letting. It had been said, that, in the severest cases, they ought not to bleed. He would only argue analogically from what he had seen in severe cases of typhus fever, where the system was in a state of congestion. It was in

the severest cases that the remedy was the most efficacious. It struck him that, in opening a vein in the severest cases, they were doing more good than in those instances where the disease assumed a milder character. He would prefer the external jugular. The external application of heat ought clearly to be had recourse to; but he could not think that the internal exhibition of stimulants was always allowable; for two things were greatly to be feared as the result of the means employed—namely, inflammation of the stomach and bowels, and general excitement, in some very severe cases. If mercury were recommended, he should be glad to know on what principle? If it were small doses, to excite the liver, it might be of service in time, but he thought not immediately. If it were exhibited for any other purpose, then he should like to know how it acted.

MR. HUNT made some observations with regard to the disease at Sunderland.

DR. O'SHAUGHNESSY moved that the debate be adjourned, and gave notice that, at the next meeting, he should take the liberty of stating to the Society the results of some chemical investigations he had made, connected with this disease.

The resolution having been seconded and carried, the meeting was about to disperse, when

DR. JAMES JOHNSON rose, and spoke at some length on the question of identity. It had been stated, and argued, that the cholera morbus which broke out at Jessore in 1817, and not there only, but at several other places contemporaneously, was not identically the cholera as it appeared previously to that period. The general argument was, that it had never been epidemic before, that it was only sporadic. Now the fact was, that, at Surat and the neighbourhood, sixty thousand persons were carried off by one visitation in the year 1760. That was certainly a curious specimen of sporadic cholera. The symptoms which then occurred were precisely the same as those manifested at Jessore in 1817. Within the last week a case had occurred, which was actually submitted to government, and caused considerable alarm in their minds. [The doctor here detailed the particulars of a very severe case, with cramps of the limbs and blueness of the surface, which had occurred at the St. James's Workhouse.] There was something in the epidemic that made the cases more severe, more numerous, more fatal, and consequently characterized it, but the individual features of the cases were the same; and at the time he did not feel justified in stating that the patient, to whom he had just alluded, did not labour under Indian cholera.

A desultory conversation then ensued, several gentlemen complaining that Dr. Johnson had been allowed to address the meeting after Dr. O'Shaughnessy's motion was carried; in the midst of which the meeting

was adjourned until next Saturday, on the understanding that the debate on the nature and treatment of the disease was to be introduced by the mover of the adjournment.

PORTRAITS OF PROFESSIONAL MEN.

ENGRAVINGS from two cabinet portraits of Mr. Abernethy and Sir Astley Cooper have been sent us. The likenesses and execution in both are excellent; and the pupils of these distinguished teachers will recognise their old masters in their happiest aspects.

PHYSICIANS TO MARYLEBONE INFIRMARY.

DR. HOOPER resigned the Marylebone Infirmary some weeks ago, and is said to have recommended Dr. Ager as his successor. Dr. Hooper had a salary of 200*l.* per annum; and as the appointment was otherwise looked upon as a good one, half the physicians in the parish became candidates for it. The ardour of the competitors, and the extreme disinterestedness evinced by some of them, led the guardians of the poor to discontinue the gratuity altogether, and to elect two physicians instead of one. Such is the injurious effect upon our profession of competition carried to excess—the physician is *not* thought worthy of his hire. The choice fell upon Dr. Hope and Dr. Sims.

DR. GILCHRIST.

WE have received a letter from this gentleman in which he strongly denies having been influenced by any personal feeling in the remarks he made at the Westminster Medical Society on the 19th instant, relative to the disputed point of the contagiousness of the Yellow Fever at Gibraltar.

DR. ELLIOTSON'S LECTURES.

A CONTEMPORARY, jealous—and not without cause—of this journal, has inserted a notice to the effect that Dr. Elliotson's lectures cannot be completed "for two or three years," and that *his* plan is to make the volumes of each year contain complete sets of lectures. We have to inform our readers that *our* plan is in this respect precisely the same, and that, as heretofore, the volumes of each year shall be made complete in themselves, and independent of the others.

HOT AIR AND VAPOUR BATHS.

NUMEROUS methods of applying hot air and vapour baths to the surface of the body have recently been submitted to us. Most of them are on the principle of Dr. Gower's Sudatorium, more or less modified. Among these, Mr. La Baume's, and Mr. Green's baths, are particularly deserving of attention, being portable, and easy of application. Nor must

we omit to mention the bath for hot air or vapour made by Mr. Rippon, of Castle-street, East; it goes into an extremely small compass, is equally efficient, and much cheaper than any of the others we have seen.

TRAVELLING HOSPITALS FOR CHOLERA.

AMONG the measures proposed in Paris for managing the cholera when it reaches that city, is the establishment of *ambulances* in each quarter. There will be a body of physicians, apothecaries, and nurses, ready to give the speediest succour. "It will be so ordered," says the proposer, "that the disease, the doctor, and the remedy, may enter together, or close on each other's heels;" "and that *death* also may probably be of the party"—adds one of the wicked wits.

"BURKING."

WE have just time and space to mention that Bishop and his accomplices, who were this day (Friday) tried at the Old Bailey, for the murder of the Italian boy, have been found *Guilty*.

NOTICES.

WE are greatly indebted to our correspondents (more numerous than we have space individually to answer) for their communications. That as little delay as possible might take place, we have this week given an extra sheet. Some on questions of a less temporary nature are postponed till the next and following Numbers.

BOOKS RECEIVED FOR REVIEW.

Researches to establish the truth of the Linnæan Doctrine of Animate Contagions. By Adam Neale, M.D.

An Inaugural Dissertation on the Congenital Malformations of the Heart. By John Paget, M.D.

A Memorial presented to the Medical and Surgical Officers of the Worcester, Salop, Birmingham, Gloucester, and Hereford Infirmaries, on the Abuses existing in the Public Hospitals; with an Appendix, containing the Answers received from them.

A Critical and Experimental Essay on the Circulation of the Blood. By Marshall Hall, M.D.

Lectures on Anatomy. Vol. III. By B. B. Cooper, F.R.S.

Practical Remarks on the Nature and Treatment of Fractures of the Trunk and Extremities. By Joseph Amesbury.

The Principles of Surgery. By James Syme, F.R.S.E.

ERRATA.

In our last No. p. 267, last line but one, for "color," read "calor," and in p. 269, for "3ij." read "ʒij."

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SATURDAY, DECEMBER 10, 1831.

LECTURES

ON

THE THEORY AND PRACTICE OF MEDICINE;

Delivered at the London University,

By DR. ELLIOTSON.

PART I.—LECTURE X.

I MENTIONED, gentlemen, in the last lecture, that I did not think the circumstance of the coagulation of healthy blood—not to say the circumstance of any of the diseased appearances—was at all explained; that I was not satisfied with any of the explanations which had hitherto been given. I forgot to mention, that I think I put together all the arguments—every thing that I can say to you here—in fact, every thing I know upon the subject, in one of the notes on the blood, in Blumenbach's Physiology. I feel a little delicacy in referring to a book of my own, but I trust you will excuse me if I should have occasion to do so in the course of these lectures.

I do not know that we have any sufficient reason given either, for the cause of the buffiness of inflamed blood. Perhaps it is owing to the thinness of the fibrin. It is said that the fibrin is thinner in inflamed than in healthy blood; and if this be the case, we may imagine that the red particles will subside more easily than if the fibrin were of its healthy consistence.

I spoke, at the end of the lecture, of what are called the various terminations of inflammation. I considered resolution and effusion, and was particularly considering, at the close of the lecture, suppuration, that effect of inflammation in which a new fluid is produced, called *pus*. I was stating, that when this new fluid is collected together within a cup, or at least when it is collected together and not diffused, an abscess is

said to exist. I mentioned that the structure of an abscess, in general, is this—lymph is thrown out, pus is formed in the middle of it, and the cellular membrane immediately exterior to the lymph is condensed, becomes firmer than the rest of the cellular membrane, so as to back the cup of fibrin. When the pus is fully collected and ready for exit, the lymph is gradually absorbed in one part, and that part is nearest to the exterior of the body, or nearest to some natural outlet, so that the pus can escape by the surface, or by the intestines, the trachea, the bronchiæ, the stomach, the bladder, or any canal which runs to the surface. If there be any danger of mischief arising from the circumstance of the walls of the abscess becoming thin, and letting out the pus into a cavity, or part of the body where serious consequences would result—as, for instance, into the peritoneum—then a great deal of additional lymph is frequently thrown out, glueing the various parts together, in order to prevent as much as possible the liability of the escape of the pus in an improper direction. Thus you see that nature usually thins the walls of the abscess at that point which is most eligible for the escape of the pus, and thickens them where it would be dangerous for the pus to escape. I stated that nothing could be more beautiful in nature than this provision.

Pointing.

The absorption at this point, wherever it may be, goes on. If it be near the surface we see the chief swelling at one particular part, and the collection is said to *point*. At length a minute opening is formed, and the matter escapes. When the latter occurrence takes place, and the cavity is thus opened, minute red granules are seen within, highly vascular, soft, covered with pus, and which, in fact, secrete the pus. Sometimes the opening at which the matter escapes, although small at first, enlarges, absorption takes place to a great extent, ulceration runs on to a considerable distance, and perhaps lays bare the cavity very much.

Granulation.

Those red granules which secrete the pus grow larger and larger, and advance forwards till the cavity is filled up. This process is called *granulation*. When the cavity is entirely filled up, portions of skin form upon its surface, and portions of the surrounding skin prolongate into them, so that you have a double source of new skin. Fresh skin appears in insulated points, and the healthy skin around shoots prolongations into it. In this way the surface of the granulations becomes covered with skin, and is healed.

But from the loss of cellular substance which takes place in the formation of the abscess—for the cellular membrane is more or less destroyed; and from the contraction of the new parts—for these granules, which were very vascular at first, become less and less so; from the loss of the cellular membrane, and the contraction of the new parts, when all is healed, we see a depression. If it be a very minute abscess, such as is only called a *pock*, as in small-pox—then you have merely a little depression, a pit; but if it be a more extensive formation of matter that has taken place, then you have *scars*. They are exactly the same thing, only on a different scale. The contraction of the new parts, and the loss of the cellular membrane, are sometimes such that the joints cannot be extended, and the arm becomes bent to the body, or the fingers bent down to the hand.

Diffused Suppuration.

It is said that the pus is sometimes collected in this way in an abscess, without any lining of lymph; but when the latter is absent the pus is almost always diffused. You frequently have pus formed in the cellular membrane in a diffused manner, so that it will run to a great extent and do very serious mischief, producing, wherever it spreads, new inflammation and the most frightful devastation. This is observed in that form of erysipelas styled *erysipelas phlegmonoides*. Where suppuration takes place in the cellular membrane, uncontrolled by a cup of lymph, it often runs on extensively, and sometimes a whole arm, or a whole thigh, is covered by a sheet of matter in this way.

You will, however, sometimes have a formation of pus, and an absorption of the skin and cellular membrane, without any collection of matter. This is seen every day in the case of the throat, and sometimes upon the skin itself, but more frequently upon a mucous membrane. If a mucous membrane be attacked by a slight inflammation, the epithelium may be absorbed, together with a small portion of the mucous membrane—in fact, you may have an ulcer, and that ulcer will secrete pus without there being a collection of matter underneath. In an abscess the matter is formed in the cellular membrane, or a solid structure, and tends out-

wards; but frequently in a mucous membrane—nothing is more common than to see this in the throat—the process goes on inwards. The surface, which is a little inflamed, becomes abraded, matter is formed upon it, and in that way you have an ulcer without the previous existence of an abscess. If the part heal, it is afterwards filled up in the same way as if the matter had been collected in a cup in the cellular membrane.

Qualities of Pus.

Pus, if healthy, is an opaque fluid, yellow or white, homogeneous, short, and creamy; so that, if you take a drop between your fingers and separate them, it breaks; the drop divides into two—does not hang in strings. It is bland generally to the taste, and without smell;—some say it is rather *marckish*—but one never thinks of tasting it, except in the case of a gum-boil, or other suppuration in one's own mouth. If it be perfectly healthy it is inodorous and bland, or at the utmost there is only a *marckish* taste. It is said to be neither acid nor alkaline, although, by exposure to the atmosphere, it turns sour. It is also insoluble in water. It contains globules; and hence it is distinguished from pure mucus by being placed between two bits of glass, and then looked through at the sun. From the existence of the globules you have an iridescent appearance, the colours of the rainbow. This is not the case with mucus; for mucus contains no globules. Mucus, again, is not short, like pus. If you take a portion of mucus between the finger and thumb, and separate them, you find that it drags out into threads; it does not break off short, like pus. I believe it is known by those who have had much to do with gonorrhœa, that, when the disease is violent, the secretion from the mucous membrane is purulent—it regularly breaks off between the fingers; whereas, when the disease declines, and pure mucus is again secreted, a person has a full conviction that he is getting better, by finding that the discharge will drag out between the finger and the thumb. When, however, pus is first secreted, it is said not to be globular, and that these globules do not appear in it till it has been poured forth from the vessels for fifteen minutes.

Persons have endeavoured to discover the difference between pus and mucus by chemical tests. John Hunter says that he found pus easily coagulate by a solution of muriate of ammonia, but that mucus would not. Dr. Darwin, jun., says that a solution of caustic potass does not dissolve pus, but makes it a viscid semi-fluid substance, stringy, like mucus, not dissolvable in water, but resembling the diseased mucus of the bladder. When that viscus is in a state of chronic inflammation—when the mucous membrane is in a state of cystorrhœa, and there is a great

discharge of mucus from the surface—its mucus is very ropy, and may be dragged out to a great length; and this is the appearance which pus exhibits if it be mixed with a solution of caustic potass. Dr. Darwin, jun., also says that the same effect is produced by adding to the pus sulphuric acid; whereas, if you add these things to healthy mucus, small flakes are produced, and not a stringy semipellucid substance. On the other hand, two modern French writers, Andral and Baumes, deny this distinction. Then you find another chemist say (Grasmeyer) that if pus be triturated with an equal quantity of water and then this combined fluid is mixed with an equal quantity of carbonate of potass, a jelly is produced in a few hours; but that this is not the case with mucus. Pus in general sinks in water, but mucus generally does not; and if pus be diffused in water, the water becomes uniformly white.

However, as to all these distinctions, the fact is, that pus and mucus run into each other. Mucus may be diseased, so as to be something like pus—puriform; and they will run into each other by insensible degrees. In disease of the lungs, the distinction between pus and mucus has been thought most important, as tending to shew whether ulceration exists; but here the two secretions are continually mixed together. When there is an abscess of the lungs, or a number of abscesses, and matter is discharged from them, you will have at the same time a quantity of mucus secreted from the bronchial tubes, so that the pus and the mucus are expectorated together; and in proportion as the former surpasses in quantity the latter, or the latter exceeds the quantity of the former, you have the character of the one or the other. If the quantity of mucus be very considerable, the pus may be borne up by it in water—not allowed to sink. Besides this, there may be an abscess in the lungs, and there may be also an excavation, and this not secreting pus, but really mucus. The lining membrane of the cavity in the lungs, at least if fresh ulceration have not taken place, may secrete merely mucus. Again, every mucous membrane which is inflamed may produce absolute pus; so that you may see pus discharged from the lungs when there is no ulceration at all. On the other hand, there may be an ulceration of the lungs, but the lining membrane of the abscess may have become of such a mucous character that it secretes really mucus, and nothing else. In addition to this, you may have mucus from an abscess in the lungs and pus from the bronchial tubes, in consequence of these being in a state of irritation, combined together in various proportions; so that the distinction between the two is not of the importance which was once imagined.

With regard to the constitution of the pus,

Schwilgué, who is one of the last that has analyzed it, says that it consists of albumen—of extractive matter, fatty matter, soda, muriate of soda, phosphate of lime, and some other salts. When pus is scrofulous, it is said to have more soda and muriate of soda than healthy pus. Pus was once imagined to consist of the dissolved solids; seeing that a cavity existed, it was imagined that the solids were melted down and turned into pus. But then you frequently have pus, as I just now stated, when there is no ulceration at all, and the formation of pus is not in proportion to the loss of the solids. You may have, from a small abscess, an immense secretion of pus, if by chance the abscess do not heal, and yet it does not extend. The truth is, the pus is a new secretion. Whether globules are formed in it—whether it is perfect or not, immediately—whether fifteen minutes are required to produce the globules and fully form the pus—is of no consideration; the fluid which constitutes the pus is, to all intents and purposes, a secretion. Yet I believe pus will dissolve dead solids; although it is not dissolved solids, yet it will itself dissolve them, making a solution of them if they be dead. John Hunter found dead flesh dissolve very readily in pus. He put an equal quantity of dead flesh into pus, into jelly, and into water; and he found that portion put into pus dissolve very readily, while the portions put into jelly and water did not. Hence it would appear that pus is useful in dissolving the solids when they die; and the cellular membrane continually dies in phlegmon. When an abscess is formed, the pus, no doubt, dissolves a portion of the cellular membrane, but still it does not dissolve it all; for when you open an abscess, you continually see portions of dead cellular membrane, which are called by the common people the *core*, escape.

Healthy pus is called by the old writers *laudable*. I recollect, when a student, hearing good pus, instead of being termed healthy pus, commonly called *laudable* pus—*praiseworthy* pus. Healthy pus should abound with globules. I may mention that one writer (M. Gendrin) says that these globules differ from the globules of the blood only in being larger, and of an opaque yellow colour. Some say that the term globule is improper—that these particles are not exactly globular, but irregular in size and shape—but still all agree that there are certain particles in the pus, as there are in the blood.

Healthy pus should abound with globules; but if they be deficient, and it watery, then we have what is called *ichorous* pus; but there may be other differences in pus besides a deficiency of globules. When pus is thin, like water, it is called *ichorous*; if it be thin and bloody, then it is called *sanious*; and it is called *scrofulous* if there

be little curds in it, which very possibly are shreds of lymph. Pus sometimes very much resembles mucus, does not abound sufficiently in globules, and is glairy or muciform, and will hang out in little slight strings. When mucus, however, becomes something like pus, we call it puriform. Although healthy pus is inodorous, and said to be only mawkish to the taste, it frequently becomes fetid. This particularly occurs if a bone be diseased; but in suppuration of the tonsils it is not uncommon for the pus which escapes, or is let out by a lancet, to be most horridly fetid; generally, however, when pus comes from a bone, or the immediate neighbourhood of a bone, it assumes this fetid character.

Early Formation of Pus.

I mentioned in the last lecture in how short a time lymph and fibrin were seen effused in a serous membrane; and pus also has been seen to form in a few hours: it has been seen produced in a mucous membrane in a few hours after inflammation has been set up; and when an abscess has been produced, matter has been formed in so short a time as from twelve to eighteen hours from the commencement of the inflammation.

Proud Flesh.

I should have mentioned, when speaking of granulations, that the smaller they are the better; for if they be large they are generally spongy, and the common people then denominate them *proud flesh*.

Mortification.

The next termination of which I will speak is *mortification*. This is shewn by the part becoming of a darker red than before. When a part is about to mortify, it grows of a dingy red; from red it becomes purple, and then black; and when it is once dead, from the decomposition of the matter, it changes to a green colour and various other hues. The pain ceases, the part becomes cold, and, if it be the surface, we often see vesicles, which, although they are frequently filled with a pale fluid, yet are more frequently filled with a bloody serum. If it be the skin which is mortified, it becomes variegated, and perhaps the part crackles, too, under the finger. From the extravasation of air the swelling increases; and the part has often a doughy feel. These are the local symptoms, but general symptoms take place. There is extreme prostration of strength and extreme restlessness. The face becomes ghastly; the pulse becomes rapid and exceedingly weak, perhaps intermitting and irregular. There is a general fall of the temperature of the body, and a cold clammy sweat takes place. The tongue becomes brown or black, the manner becomes exceedingly quick, and frequently a little delirium takes place. The cessation of the

local and all the violent general symptoms will take place just before death from inflammation, when that inflammation does not cause mortification, or any other local change—although, when mortification occurs to any extent, you will have the very same thing, that is to say, the cessation of pain, the ghastly countenance, the rapid, feeble pulse, the extreme sense of debility, restlessness, and at last death, without any mortification—without any other local change whatever. I believe I mentioned before, that frequently from the symptoms of enteritis, you would imagine that mortification of the intestines had taken place, but on opening the body you may find no mortification, nothing but intense inflammation.

When the parts inflamed become of a purple colour, and cold, and fall into incipient mortification, restoration is not impossible: parts will recover that have become purple; and lost a portion of their temperature. Even parts which have been congealed will recover by good management; but this, I believe, takes place in man and the other warm-blooded, more than in cold-blooded animals; and Professor Thomson, of Edinburgh, says, if a *whole* limb is frozen it is never restored. A still higher degree of mortification is called *gangrene*; and the complete death of a part—when it is dead, gone irrecoverably—is termed *sphacelus*. In the latter case, the part is cold, senseless, black; and putrefaction follows. There are three degrees, therefore, of mortification:—the one where the parts are merely purple and cold; a second where things are more advanced, but the part is not absolutely dead; and the third, in which the part is really dead,—mortification being the generic term for all the three stages. When a part is dead, and putrefaction is begun, the progress of the putrefaction depends very much upon external circumstances. The warmer the weather, of course the more readily will putrefaction take place.

Separation.

If the part, which is in a state of sphacelus, is not important to life, it may be separated by nature from the body. The process of nature here is exceedingly wonderful. The large vessels, which go to the part, become plugged up—a coagulum of blood forms in them, or adhesive fibrin is poured out from the inner surface, so that in one way or other the progress of the blood is arrested; the consequence of which is, that it coagulates to some extent above the plug, and the plug thereby becomes increased. Dr. Thompson, of Edinburgh, says, that a coagulum never extends higher than the first communicating branch, and that one would suppose. The blood coagulates behind the

plug, nearest to the heart, and you cannot, therefore, expect a plug to extend beyond the first communicating branch. He says that sometimes he has seen a vessel entirely connected together, completely obstructed, without the formation of any plug at all, the whole obstruction being produced by an effusion of lymph, lymph which has become organized, and formed an internal adhesion. Nay, he says that sometimes he has seen an artery completely obstructed without any lymph, without any plug, but simply from its becoming closely contracted, so contracted that it would not admit a bristle. The contraction has formed as complete an obstruction to the blood as the existence of a coagulum, or a quantity of fibrin would have done. He says that he has noticed the same circumstance in an amputation where no ligature had been employed. The vessels have become so contracted as not to admit of the introduction of a bristle, and have thus prevented all hæmorrhage. In whatever way it is done, whether by a coagulum of blood, whether by a plug of lymph forming an adhesion, or by the mere contraction of the vessels, or by any two of these, such is the fact, that nature generally succeeds in preventing hæmorrhage from a mortified part, by obstructing the course of the blood in the larger vessels leading to it.

Then the process of separation is accomplished in this manner: a red line appears at the boundary of the healthy part which is nearest to that which is dead. This red line, of course, is a line of inflammation, and in it ulceration takes place, which goes down in the healthy part lower and lower, till a complete separation takes place; and in this way a whole limb is sometimes separated from the body.

Necrosis.

Of course I have been speaking of soft parts; but if a long bone die, which is a very common thing, without any death, or any especial disease, of the surrounding parts, a new bone is formed around it. The process of nature, therefore, is quite different here from that which is employed to separate a dead soft part. In this case a new bone is formed under the periosteum, around the dead portion; and in this way what is exterior to the new bone, namely, the periosteum, the ligaments, and the cartilages, all become attached to it. When the old bone within the new one is detached, completely loosened, then an opening is formed in the new bone, ulceration takes place in it at one spot, granulations occur on the inner surface; and these granulations push the old bone through the ulcerated opening in the new one—at least they tend to do so. The aid of a surgeon is frequently of use in assisting to draw out the old bone, and enlarge the openings in the new; but this is the process adopted by nature when a bone dies.

[The learned professor here exhibited a beautiful specimen illustrative of these remarks.]

This process is called *necrosis*; and the old bone within will frequently rattle like the kernel of a dry nut. The old bone is called *sequestra*.

Exfoliation.

If, however, it be not a long bone which is dead, but merely a plate, or a flat bone, this is separated by ulceration. Granulations form under the old bone and push it off. This process is designated *exfoliation*, because the bone comes off like a leaf.

Terminations.

These are the "terminations," as they are called, of inflammation; but I should rather say, they were the *effects* of inflammation than terminations, because inflammation does not necessarily terminate when they occur. Inflammation frequently goes on after these effects take place, and therefore we ought to say, strictly speaking, that these are the effects of inflammation, or circumstances that occur from inflammation.

You will find that authors enumerate among the terminations of inflammation, *scirrhus*; but if by that they mean the *scirrhus* which afterwards becomes cancer, they are certainly wrong, because *scirrhus* will occur without marks of inflammation: a part without these will gradually become cancerous—seems a specific disease. If, however, by *scirrhus* these writers mean true *scirrhus*, mere induration, then they may be right, because one of the effects of inflammation is to harden. When a part has been inflamed for a length of time, it frequently becomes indurated, so that *scirrhus*, in that sense, may be said to be the effect of inflammation, but not a termination of it, because the inflammation will go on, and the part become more and more indurated. Induration, however, is only one of such effects of inflammation which they ought to enumerate, for it has as great a tendency to cause parts to become softened as hardened. Induration is rather the effect of chronic inflammation, while acute rather produces softening.

Duration of Inflammation.

Having spoken of the effects of inflammation, I now proceed to consider its duration, and this is very various, much depending upon its intensity. If an inflammation be exceedingly violent, it cannot last long; but if that be not the case, it may last a great length of time. Inflammation may exist for only a very few hours, and may certainly destroy life in the course of a day: it is sometimes so rapid that a person shall be well in the afternoon, and die from it before the next morning is much advanced. I recollect myself seeing last winter a case of

pericarditis, which occurred suddenly in the evening, about seven o'clock, from a lady having exposed herself to cold by walking about the house half-undressed, and she was dead before eight the next morning. Inflammation may be so acute as to destroy life in a few hours without any mortification, but simply from its own intensity. Inflammation may be even almost momentary, but that will depend upon the use you choose to make of words. Certainly a child is sometimes seized with a violent rush of blood to the head, its eyes will become red, —in adults the same thing will happen—its face flushed, its head distracted with violent pain, it screams, and death ensues in the space of one or two minutes. After death a great quantity of blood will be found within the cranium, and externally to it. If this state be inflammatory, it certainly proves that inflammation may be almost momentary, and that it will destroy life in this sudden manner.

But inflammation, if it be not violent, may last for many months, and even years; for if it remit, it may be tolerably violent often, and yet continue for a very long time, its severity not remaining the same during the whole of the period. If it once degenerate into the chronic form, it may last indefinitely, and when it does so, it generally induces various changes of structure, which I will hereafter mention. When a part has been once inflamed, the redness will occasionally remain some years without any pain or any heat. You may see this frequently after burns, and after cutaneous diseases. A portion of a scar will remain exceedingly red, the vessels would appear to be in a state of passive dilatation; there is no pain, no heat, no tenderness, but extreme redness; and this, after it has existed a year or two, or perhaps longer, will sometimes decline; the vessels will gradually at last recover their natural tone. After ulceration the skin will frequently remain of a red colour, perhaps of a very dingy red, and almost of brown colour. In the case of a person who has had a sore leg, you will see that the portions corresponding with the spots of ulceration are frequently very red, brown, variously discoloured, or perhaps nearly black.

Such, gentlemen, are the usual phenomena of inflammation, such are the chief symptoms, such are the general effects or terminations, and such is its course. The next point which I will consider is the variety in all these symptoms, and the variety in its course, occasioned by the nature of the structure which it attacks. As, however, this is a new subject, I had better defer its consideration until the commencement of the next lecture.

CLINICAL LECTURE,

Delivered at St. Bartholomew's Hospital, Nov. 9,

By H. EARLE, F.R.S.

Tracheotomy; especially as applicable in cases where Children have drunk boiling water.

EDWARD ELMORE, æt. 2½, was admitted into Queen's ward on the night of the 4th of November.

Six hours before, he had drank some water from the spout of a tea-kettle, which had been recently boiling. Upon admission, the respiration was very laboured, and accompanied by a sound evidently arising from some obstruction in the larynx. She seemed in pain, but could speak with tolerable plainness, and had swallowed fluids recently. Some leeches were directly applied, and seemed to relieve the child much. After a while, however, she again became worse; the leeches were repeated, and an aperient powder ordered.

About 1 o'clock next day, (18 hours after the occurrence of the accident) Mr. Earle saw the child. At this time, the air was evidently propelled with great difficulty through the larynx, and the chest was but incompletely filled at each inspiration, although, on applying the ear, some air was heard to enter, accompanied by a crepitating sound. The child lay back motionless upon the bed, although she was perfectly conscious, spoke to her mother once, and coughed now and then. The lips were livid, and her countenance anxious.

It occurred to Mr. Earle, that in such cases as the present, the operation of tracheotomy might be of service; and although the little sufferer, in this instance, was too far gone for any sanguine hope of relieving her being entertained, yet, as the only chance of saving her life, he proposed it. Mr. Lawrence agreed that the case perfectly justified the performance of the operation.

In the attempt to remove a portion of one of the rings of the trachea, one of the thyroid veins was wounded. As this bled rather freely, the operation was deferred for about half an hour. Eventually, an opening was made of sufficient size to permit the introduction of a portion of a large-sized gum catheter, through which the air freely passed.

Shortly afterwards, the child opened her eyes wide, her countenance brightened up, and she looked much revived, as all present remarked. This improvement was not, however, progressive; the child never rallied to any extent, and died without a struggle in the course of the afternoon, having lived about 24 hours after swallowing the water.

After the tube was introduced, constant attention was required to remove the mucus, which every now and then blocked it up, producing, by its obstructing the passage of air through the tube, all the former difficulty

of breathing, which was immediately relieved by removing this mucus.

Upon examination some hours afterwards, the mischief was found to be chiefly confined to the glottis, and parts immediately around it. Considerable thickening of the arytenoid cartilages; a shrivelled, corrugated state of the epiglottis, which was also superficially ulcerated, its edges being at the same time in a sloughy state.

Some inflammatory appearances, likewise, were found along the trachea and bronchi, which, however, did not seem to arise from continuity of surface, as the superior portion of the tube, where the opening had been made, was not so much inflamed as the lower.

The lungs were loaded with serum and blood; and, in some considerable portions, were so much consolidated, as must have prevented the entrance of air at these points.

Mr. Earle, in his clinical lecture, made the following remarks upon this case:—

He observed, that cases of this description were not very uncommon in their occurrence among the children of the poor. Some fatal cases have been published by Mr. Stanley and Dr. Marshall Hall, in the *Medico-Chirurgical Transactions*. It occurred to Mr. Earle, some time back, that it would be proper to perform tracheotomy in such cases, but this was the first case that had occurred in his practice. He was not aware until after the death of this child that the same idea had suggested itself to, and had been acted upon, by Dr. Hall, at Nottingham, who published a paper detailing some cases. In only one of these an operation was performed. This was done twelve hours after the accident had occurred, and the child lived thirty-six hours; whereas, in all the other cases he relates, death took place much more rapidly.

The epiglottis and surrounding parts are, by such accidents, injured in such a degree as to become unfit to perform their functions; added to which there is much spasmodic contraction in the rima glottidis, similar to what occurs in acute laryngitis, in which disease tracheotomy affords the best chance of recovery.

In a part of such vital importance as the larynx, it is very doubtful whether we can trust to mere antiphlogistic measures, although employed at an early period, and with activity, whether such affection be dependent on any injury, or be an idiopathic inflammatory attack. It is not merely the affection of the organ itself that is of importance, but through it other important parts suffer. From the injury done to this one portion, the whole of the respiratory apparatus becomes deranged. Even should we succeed in arresting the inflammatory action, yet we must recollect that inflammation is a process, and that the local actions already commenced may destroy a patient, although

the general inflammatory state may be arrested.

From a mere consideration of post-mortem appearances, we do not see the full extent of the affection. This part is endowed with the highest degree of sensibility and irritability, consequently inflammation in its neighbourhood produces the most violent spasmodic action.

The extreme sensibility of the larynx, as contrasted with the minor degree which exists in the trachea, is well demonstrated by what occurs upon the introduction of a foreign body. A foreign body, upon being introduced into the larynx, excites the greatest possible irritation; but on descending into the trachea, so little does it cause, that a practitioner, if not aware of this circumstance, might doubt the presence of such body until the patient by coughing, or other violent exertion, forces it up to the larynx, and thus reproduces the dreadful spasmodic irritation. This high degree of sensibility is no doubt provided for the wisest purposes, namely, to prevent the ingress of any foreign body during the act of deglutition.

In consequence of its obstructing the free egress and ingress of air, an inflammatory affection of the larynx exerts a rapidly-destructive influence upon the lungs; consequently no operations, unless performed prior to the occurrence of such changes, can be of any avail.

In the present case the lungs were loaded with serum, and in a great measure consolidated; the blood sent to them could be only half oxygenated, and in such a state must exert a deleterious effect upon the sensorium and nervous system. This causes a diminution of nervous energy, which again will react upon the lungs.

Dr. Philip relates, in his experiments upon animals, that where he has cut off the supply of nervous influence the lungs have become speedily loaded and hepatized. These experiments in some measure explain the very rapid changes effected in the lungs in acute laryngitis, and cases like the present, where severe injury has been inflicted on the glottis and larynx. The operation in the present case could scarcely, after so long a delay, have been expected to succeed; and, therefore, its want of success should not militate against the future performance of it in other cases at an earlier period.

There are two operations by which the object may be effected:—1. The removal of a portion of one or more of the rings of the trachea. 2. The forming an aperture between the thyroid and cricoid cartilages.

The first of these is difficult of performance in a young child, or a deep-necked female, and dangerous from the great number of veins which ramify about this part, the wounding of one of which arrested the operation in the present case. The mobility of

the larynx is also very great indeed, rendering it difficult to fix it for a short time, in order to remove a portion of it; and, from the nature of its texture, the knife will slip over it rather than penetrate it, unless it be held firm. This object may in some degree be effected by thrusting a tenaculum into it, and thus in some measure restraining its motions—which expedient was resorted to in the present case. Another danger arises from the irregular distribution of the carotid arteries, which sometimes occurs.

The other mode of operating is far more easy, and in the present case would have sufficed, as the inflammatory action had not proceeded far down the larynx, although it was impossible to be certain of this beforehand. But in acute laryngitis the first of these operations must be performed, as the whole larynx often participates in the inflammation, and therefore the farther from the seat of disease the operation be performed, the more chance of success. Mr. Earle referred to the case of a patient of Dr. Latham's, in Mary's ward, upon whom he performed tracheotomy last summer, when she was upon the eve of death, for acute laryngitis. In this case he could not but attribute the happy issue of the case to his having performed the operation at a point sufficiently remote from the disease. There are some points of difference in acute laryngitis, and the affection resulting from local injury to this part. In the former the greatest difficulty attends an attempt at deglutition, and there is also great tenderness evinced upon pressure being made in the region of the larynx.

In a subsequent lecture Mr. Earle observed that at the close of his last lecture a gentleman related a case to him, in which the operation had been successful, which had occurred at the Salisbury Infirmary. He had since furnished him with the details of the case, which Mr. Earle read to the class.

The child was sixteen months old, and the operation was performed two hours after the accident had occurred, and succeeded perfectly. This case, of course, proves in the strongest manner the propriety and utility of performing the operation before the disorganizing process had commenced in the lungs. In the progress of both this case, and that before alluded to, as detailed by Dr. Hall, portions of the food were occasionally observed to pass out at the external wound. Now as the action of the boiling water is so immediately upon the epiglottis, corrugating it, and unfitting it from closing perfectly the aperture into the larynx, this circumstance would seem to go far in determining the true use of this part. Magendie considers the epiglottis as an organ of the voice, and not of deglutition. In experiments upon animals, or in accidents in battle, it has been removed with the effect of impairing the

voice much by its loss, yet such subjects have been enabled to swallow after a time.

When we consider the admirable adaptation of the epiglottis to the part it covers, we cannot but coincide with the old opinion that it is (at least in part) an organ of deglutition; and in reference to the deductions we draw from experiments and accidents, we should always bear in mind the very great compensating and reparative efforts of nature upon these and all similar occasions. During sleep, when all parts are in a more or less torpid state, if it were not for some such contrivance, the saliva so copiously secreted in catarrh and in salivation, would pass into the larynx, exciting distressing cough. There is a man in Pitcairn's ward, of the name of Burgess, who has now come in with a phagadenic ulcer of the leg, who was a patient in 1828. He then had an extensive sloughing ulcer of his throat, which extended to, and destroyed the epiglottis. For a long time after this occurrence, whenever he took food or drink, he had a most distressing fear of suffocation. Now, however, he has recovered in some measure his voice, and has lost all the dread of swallowing he once possessed. This man entertained the fear of suffocation as long as the parts remained in their unprotected state, but has since entirely lost it.

Mr. Earle next alluded to the following case, which, from the symptoms, was supposed to be one of chronic laryngitis.

Godfrey Wolmer, æt. 60, gold refiner, admitted into Powell's ward, November 4th. Countenance anxious, sallow, and aged; respiration very difficult, and accompanied by a wheezing sound heard at a considerable distance. The difficulty seems, however, to be entirely confined to the larynx. Occasionally it is aggravated by severe paroxysms, which oblige him, if he be lying down, to start up, from fear of being suffocated. All exertion increases his distress much. Occasional cough, accompanied by the expectoration of a viscid mucus, and often purulent matter. This cough, however, does not seem to affect his chest, but merely acts, as he says, by clearing his throat of the mucus, which being expelled with a very peculiar noise, affords him great temporary ease. Voice husky, and sometimes almost inaudible. He says his deglutition is very arduous, but those about him state that it is not very difficult. He, however, complains of his food lodging for a time at the upper part of the throat. Very little tenderness is excited by pressing the larynx. Dr. Latham examined his chest. Percussion was every where good. The respiration was scarcely audible, owing to the small quantity of air admitted to the lungs, but no unnatural sound was detected. The old man, on admission, was in an enfeebled state, and seemed only half alive.

The history he gave of his complaint was vague; he referred it to no distinct origin, and even gave different accounts as to its duration. It seems, however, it certainly had existed nine months, and had become much worse during the last three. He has been long out of employ, and living in a very indifferent manner; and, indeed, latterly the severity of his sufferings would have incapacitated him from undertaking any, which required exertion.

Active counter-irritation was applied to the throat, but he continued going on much the same. The house-surgeon was several times called to him when in paroxysms of dyspnoea, which seemed to threaten his life. Opium given internally, and inhaled, relieved him much of this spasmodic irritation.

It was thought that this man laboured under chronic laryngitis; and although, from the long duration of the disease, irreparable destructive changes in the part had probably occurred, yet, as perfect rest of the parts affected afforded the *only* chance of their reparation, and as the man's life, during these paroxysms, was in infinite danger, Mr. Earle considered it a proper case for the performance of tracheotomy; as a means, at all events, of prolonging the patient's life, and in no way adding to his danger.

In this view of the case, Dr. Latham and Mr. Lawrence coincided, and it was agreed to perform the operation the next day.

On the evening before, however, he was seized with another paroxysm; the house-surgeon was sent for, and performed the operation: it was too late—he died directly after. Upon examining his body, the tunica arachnoides was found very much thickened indeed, and beneath it was most copious effusion. The ventricles of the brain were also distended by fluid. The epiglottis was nearly natural in its texture and appearance, slightly thickened, and turned to one side by a portion of the tumor about to be described; each arytenoid cartilage was to be converted into a large mass of cartilaginous hardness and irregular form, nearly closing the rima glottidis. The œsophagus, at its origin, was much contracted, and becoming converted into the same indurated substance as the arytenoid cartilages. The lungs were loaded with a sanguineo-mucous fluid, the lower lobes being much condensed.

Mr. Earle observed, that although, from the inveterate nature of the affection, cure would have been out of the question, yet, had the operation been performed sooner, this man's life might have been prolonged.

OBSERVATIONS ON THE CHOLERA MORBUS OF INDIA.

By J. SPENCE,

Assistant Surgeon, 52d Regiment.

THE mystery which has hitherto enveloped the nature of cholera morbus, and the fatality with which it has almost universally been attended, are circumstances of themselves sufficiently urgent imperatively to call upon individuals having experience in the disease to make public the result of their observations; which, being done with candour, cannot fail to be interesting.

Formerly, spasmodic cholera was a disease which professional men, not destined for service in India, were somewhat careless of, looking upon it as one which they were never more than nominally to know. Now, however, since it has passed the boundary of the tropics—since it has committed such devastation on the Continent of Europe—all are eager to gain information regarding it; the attention of all classes is directed towards it; in fact, cholera is the lion of the day; and therefore I hesitate not to offer to the public some brief remarks, which, having been collected at the bed-side of a very great number of patients, may perhaps be acceptable.

The cause of cholera morbus is necessarily a most important matter, involving, as it does, not only our vital, but mercantile interests, and therefore should not too hastily be dismissed from our consideration. But, to arrive at a correct conclusion, the profession ought to enter upon the investigation perfectly unbiassed by any notions previously entertained, regarding its contagious or non-contagious qualities; with reference to which I cannot but lament that the opinions I have adopted are opposed to those acted upon by the highly-distinguished individuals appointed by the government for the elucidation of this subject. In forming these opinions, however, I have been actuated only by the desire of attaining the truth, and believe the conclusions arrived at to be the fair inference from the facts presented.

I have seen very many cases of cholera morbus in the Presidency of Bombay, and a few at Colombo, in Ceylon. Once only when it occurred as an epidemic. At other times the cases were separate

from each other by a greater or less interval of time; which latter fact is one of much weight in the argument, for if it can be proved that a genuine case of cholera can occur, and the patient be treated and dies while in the midst of persons, none of whom become infected, I think it certainly a proof that the disease could have no power of propagating itself. To bear me out in the fact that such instances are very frequently met with, I would call for the testimony of every practitioner who has been even six months in India. One instance might occur, and the individuals in the same ward, or in attendance upon the sick person, might not be predisposed; but is it likely that it should be observed a hundred times with the same result?—and I feel confident that I could, with the assistance of my friends, adduce a greater number than I have mentioned. When the disease, then, attacks individuals thus sporadically (say one in a month), I think I may safely assume that it neither had its origin in contagion nor did it manifest the property of a disease of that class. I then, from this, am led to inquire whether there be any difference perceptible in the symptoms, or signs after death, between a case of cholera under the above circumstances and one occurring when the disease is epidemic? To this I can most confidently answer in the negative; admitting them both to have the symptom which forms the grand diagnostic of the spasmodic Indian cholera—namely, the evacuations per anum resembling rice water: this being present, I repeat that there is no distinction whatever. I have seen cases, and, so far as I remember, all those which occurred whilst I was at Ceylon, were of the same character, in which there was violent vomiting and purging, with cramps and great exhaustion; but the stools were not white, and evidently contained bilious matter; the patients all recovered. But this is a very different form of disease to that which I shall presently describe.

It often happens that detachments marching in India, on arriving at certain encampments, become attacked with cholera; and it is then the universal custom immediately to strike the camp, and the disease usually disappears. Fortunately, the persons in authority, never thinking of contagion, remove the sick with them, if possible. Wretched,

indeed, would be the situation of an infected person, did those around believe him capable of contaminating them: to propagate such a doctrine would cause the helpless sufferer to die without attendance. One gentleman, with whom I am acquainted, arrived in Bombay harbour, from New South Wales, with a detachment of recruits; they immediately proceeded on the march to Poonah; but on the second day, having arrived at an encampment situated at the foot of the mountains, or ghauts, as they are called, cholera broke out, and, in the course of a few hours, he lost a number of men, and became affected himself, having, with the rest, been an hour previously in excellent health. As soon as possible they proceeded up the ghauts, and it was my friend's good luck to recover, although, during the march (being moved in a palanquin), his dissolution was greatly feared. On arriving at Kandallap, a distance of about five miles from Campoolie, but at least 3000 feet above its level, the disease disappeared. Between the native inhabitants of these two villages there exists the most unlimited communication; moreover, there are constantly whole hordes of native travellers passing through them, along the road between Panwell and Poonah. Surely if the disease were contagious it would exist in both at the same time, and to move from one to the other would be of little avail; however, such is the fact—the detachment moved, and not one other person was infected. I was informed how many died, but I cannot at present remember the exact number. The deaths are sufficient to shew the nature of the disease. Its local origin, and sudden disappearance on the change of situation, notwithstanding that at least such of the sick as were able to be conveyed followed in the rear, are, I think, satisfactory evidence of its possessing no contagious property. If the heads of the departments in the country had any, even the most remote, reason to believe that cholera was a contagious disease, surely they would, in a case like that I have just adduced, establish a quarantine, and not have allowed this infected body to join the head-quarters, at least till a clean bill of health could be produced; but no—they continue their march, arrive at Poonah, and hear no more of cholera, at least for a time. This is by no means a singular case, and

I doubt not, that, on reference to the reports of the army medical officers, many similar examples may be found. Cholera has made its appearance in transport, merchant, and other vessels, some time after their leaving the harbours of India; they proceed on their voyage, often, indeed, losing many men, but I will venture to assert there never was an instance where the ship, touching at any port, communicated the disease to a single individual. I am aware of the popular opinion that the *Topaze* frigate, touching at the Mauritius*, infected persons on that island; but we are informed by Dr. James Johnson, in his book on the "Diseases of Tropical Climates," that the fact was entirely disbelieved by the principal medical officers and others who were there at the time, and must necessarily be most entitled to credit.

For the last ten years, or more, an immense number of vessels have been trading between India and the different ports of England, many of which have left the different presidencies at times when the disease has been most rife; often has it happened that the labourers employed in shipping the cargo have fallen immediate victims, and still we have as yet escaped the pestilence: why, then, is it likely that there should be a difference in vessels coming from the Baltic? Certainly it would not be easy to answer this question. It is a common practice among the ships of war on the Indian station, immediately cholera appears on board, to weigh anchor and proceed to sea: is not this a satisfactory proof that, by naval officers, it is not believed to be a contagious affection? For surely a ship, howsoever good the accommodation, cannot be the best place to separate the clean from the unclean.

When the disease attacked the sixth regiment, at Colabah, in July 1828, its commencement was a remarkable proof against its contagious nature: it was in the midst of the most violent rainy season; not a case had for months been seen either in or near Bombay, when assistant-surgeon Campbell, in paying his evening visit to the hospital, found an old soldier, who had been some time under treatment for an hepatic affection, suddenly seized with cholera: he went to the opposite extremity of the build-

ing for the purpose of consulting with the surgeon, and found him busily employed with another man who had been almost simultaneously affected. Now it is physically impossible that these two individuals could have received the disease by contagion, because that which does not exist cannot have issue. From this beginning the disease proceeded, with unabated violence, until it proved fatal to sixty men and several women. It attacked a number of men who were labouring under, or convalescent from, diseases of the liver and bowels; none of whom, so far as my memory serves, recovered. Besides those in hospitals, others who were in excellent health were infected in the barracks. It is curious that, whilst the disease was committing such dreadful ravages amongst the soldiers, no instance of an officer's servant, or officer being attacked, occurred. This may be accounted for by their being less exposed to the vicissitudes of the weather, and, from possessing greater comforts, were less predisposed; not from their being separated from the diseased, because the officers were in the habit of paying daily visits to the hospital, and conversing with the men of their different companies who were dying, and the servants were in the constant society of their comrades in the barracks: indeed, it was often necessary for them to take their duty at the funeral parties, the distance which the corpse had to be carried being at least half a mile. They were thus exposed most powerfully to the contagion, had any existed. During this melancholy period, we had as many as two hundred and fifty men in the hospital out of seven hundred; it may, therefore, easily be imagined that the medical officers and attendants must have been constantly employed; and although we were with the sick the greater part both of night and day, and never deemed it necessary to use the slightest precaution against infection, yet we all escaped. The apprentices and dressers slept invariably in the wards, and always in the next bed to the worst cases. None of them were infected. We were allowed a number of orderlies from the barracks during the whole time the epidemic lasted; these were constantly employed performing various duties round the beds and about the persons of those labouring under cholera, but there was no in-

* See the letter on this subject in the present No.

stance of their being struck with the blight, if I may so term it. The nurses whose duty it was to attend the women admitted into the hospital, being scarcely ever out of the wards, had a like immunity. In fact, there was but one circumstance which, even by the most prejudiced contagionist, could be laid hold of to support the doctrine. A serjeant, who attended his wife, became slightly attacked, but was one of the few who recovered.

It is worthy of observation, that the number of fresh cases in the day bore a very near connexion to the state of the weather; for so sure as the day was clear, allowing the blinds to be thrown open and a free ventilation, together with the different effects upon the spirits of the men, induced by the change of atmosphere, there would be no one attacked; but perhaps the next day being wet the virandahs were closed, every thing inside was dark, damp, and miserable—then, indeed, the poor fellows breathing impure air, being weak and wretched, seeing their comrades dying all around them, were certainly predisposed, soon infected, and died. The pestilence, however, after remaining with us two months, gradually subsided as the monsoon terminated.

It will be seen that, although I have stated what I believe cholera *not* to arise from, I have not said what *is* the cause; nor, in truth, am I able to advance more than merely a suspicion of its being a poison circulating in the atmosphere—whence arising, or how existing, I know not. But I am fully convinced that certain circumstances, operating together, render individuals exceedingly liable to its attacks; amongst which are poverty, unwholesome or insufficient nutriment, despondency, fatigue, debility from previous disease, or whatsoever depresses mentally or bodily: thus the followers of a long-protracted and unsuccessful campaign, or the poor inhabitants of an over-populated and disaffected country, are particularly predisposed, and, when once attacked, have little power to resist its fatal tendency.

In proceeding, it now becomes necessary to describe the general appearances of the disease. As I have already hinted, it is liable to assume two distinct forms, which, being very different in character, but passing under one name, have, if I mistake not, caused considerable discre-

pancy amongst even Indian practitioners. The one approaches very nearly to the affection so named in England, which occurs generally in the autumn; it is attended with vomiting and purging simultaneously, the matters ejected being highly mixed with bile; extreme pain in the abdomen; sometimes cramps in the gastrocnemii, and great exhaustion; the pulse may sometimes be very small and weak, and the surface of diminished temperature; but on the application of the hot bath, and administration of a little diffusible stimulus, reaction becomes developed, and the patient may perhaps require the abstraction of blood, which, with a little calomel and opium, generally restores to health. Thus it is that persons seeing this form of disease in time readily cure the patient, and spread forth the doctrine that cholera is to be treated by bleeding, attributing blame to others who, seeing the disease under a different form, have not had recourse to the same measure. But I am convinced that if even this description of case be not seen in time, before the powers of life are too far sunk, bleeding will be inefficacious, at least until reaction shall have been restored by other means. Young healthy individuals are most liable to this species of attack, especially shortly after arriving in a tropical climate, and I have bled them myself with decided advantage. In fact, I repeat, if seen early, and occurring in healthy subjects, this is an exceedingly manageable complaint. Not so with that form where the patient, perhaps, having the moment before been attending to his ordinary duties, for instance, on sentry, has been suddenly seized with an inclination to go to the rear, attended with an uneasy sensation of the abdomen, and sickness at stomach. He passes, as he believes, a very copious evacuation, and perhaps may return to his post, to which he shall scarcely have got, ere he is irresistibly called again to the necessary; now, or perhaps before, he evacuates the contents of the stomach, mixed with a frothy mucus: sometimes he is by these operations so exhausted as to fall down on the ground. In this state he is found; and perhaps before the medical officer can see him, the pulse at the wrist will not be perceptible, but the skin covered with a clammy perspiration, with severe cramps in the abdomen and calves of the legs; the countenance

ghastly, and expressive of the utmost anxiety; the nose sharpened, the corners of the mouth drawn back, and the eyes sunk, with a depression under the lower lid. It is astonishing how soon this expression supervenes; and of so marked a character is it, that it were impossible for a person once having seen the disease not to recognize it even by this sign alone. Indeed, in a less degree it is perceptible before the other symptoms become violent; and I have more than once, whilst passing in the ward, or conversing with a soldier, detected the disease before the patient was himself aware of being infected, or at least before he made any complaint. The thirst is most insatiable, and the desire is generally for cold drinks. There is great restlessness, the patient never remaining in one position a minute, throwing his body from one side of the bed to the other; the purging continues incessant, the matters ejected being thin and white, and have been aptly compared to rice-water, but generally being very foetid. Nothing now remains on the stomach, and the individual, however strong, is sunk to the most extreme prostration; the finger-ends are shrivelled, as though they had been long immersed in hot water; the nails are of a dark black, or blue colour; the respiration difficult and hurried. The patient retains perfect possession of his mental faculties till the last, and is generally much influenced by surrounding circumstances; he will catch with great avidity at the most trifling expression of the medical officer or attendants. The disease sometimes runs its course in two or three hours; again, I have known it protracted to three days before the fatal termination. There is considerable difference in cases relative to the severity of the cramps; in some they are most excruciating, in others but little complained of; and I think generally the latter are the least manageable, as in them the powers of life are more sunk.

Dissection has thrown but little light on this disease; the large veins in the thorax and abdomen are generally gorged with blood, of a thin oily appearance, uncoagulated, and of a very dark colour, which issues in great abundance from the lungs and liver on being cut. The gall-bladder is always exceedingly distended with a thick dark

tar-like substance; the veins of the peritoneal coat of the intestines are most minutely injected. The mucous surface of the stomach and bowels I have rarely found inflamed, but there is a thick greyish secretion found in great abundance in the intestinal tube; there never is a particle of bile, or any thing bearing the appearance of fæces, to be discovered. I have often seen in the intestines long round worms, which, as well as the tape worm, have also repeatedly been passed by patients, both by vomiting and by stool; whether or not they had any connexion with the disease I am not prepared to say. The urinary bladder was always found empty, and collapsed. The pathology of the disease, then, does not appear to be much elucidated by dissection, but I entertain a very strong belief that chemistry will one day or other assist us much in this particular, for besides the stagnation, which is evident both before and after death, there is, if I mistake not, a material change in the quality of the blood; what that may be, or how induced, I will not attempt even to suggest, but I think a knowledge thereof might be of wonderful importance, and to this circumstance I would particularly direct the attention of the profession. So far, however, as we already know, the indications to be fulfilled are to support the powers of life, until by some means the balance of circulation can be restored, and then, by exciting the various organs to renew their suspended secretory functions, the disease may be overcome; but I am bound to confess that my own efforts to this end have been unavailing in an immense majority of cases;—nevertheless I have not yet learnt any plan better calculated to save the patient than the one we adhered to at the close of the epidemic. We were not so inhumanly blinded by prejudice as to persevere in what we saw ineffectual, but adopted means which had been serviceable in the hands of others; however, they were not alike so in ours, and after a fair trial we reverted to our original principles, and which I now recommend, not as the best that may be, but as the best which I know of.

On being called to a patient labouring under cholera, such as I have described, I would administer immediately a large dose of calomel, viz. twenty grains, followed by a draught, contain-

ing about two ounces of brandy, a little hot water, and two or more drachms of laudanum; a small quantity of sugar and spices may be added, if palatable to the patient: the calomel to be repeated every half hour, or hour, until the stools become bilious, or the evil symptoms subside; the laudanum and brandy, with the addition of the carbonate of ammonia at intervals, and in quantities to be regulated according to the violence of the vomiting, purging, exhaustion. &c. If the patient be much exhausted, he had better not be put into a hot bath; but if the attack be at its commencement, or not severe, it will be very beneficial; the temperature should be exceedingly high, 150 degrees, or more, for the skin is so deadened as to lose, in a great degree, its sensibility. As a substitute, the patient may be enveloped in blankets, immersed in hot water, in which the nitro-muriatic acid has been mixed; this measure, I think, I have seen of service. If neither of these means should be adopted, frictions should be used to the whole surface of the body, with strongly stimulating liniments. Six persons, at least, ought to be constantly employed. Sinapisms of mustard should be applied to the calves of the legs and feet. A large blister may be applied to the abdomen, but one of the ordinary kind will be of little avail, as it seldom vesicates. I have, therefore, known nitric acid applied, but have not adopted it myself. The cramps being so severe, the patient constantly calls loudly for some one to rub the belly, and therefore I usually order a liniment of spirits of turpentine, opium, and oil, to be used, which at all events produces slight temporary relief. The less the patient drinks the better; but the demand being so urgent, liquids cannot be entirely withheld; they should be warm and slightly stimulating. I knew one patient who rejected every thing but cold water; another whose demand was for gin; others drink in preference congee; and perhaps it is well in some measure to yield to this caprice. If under the measures adopted the extremities become warm, and the skin regains its elasticity—if the pulse increases in volume, the vomiting ceases, and the purging be diminished, or the matters passed be tinged with bile, a favourable result may be prognosticated, though the hope is often fallacious, as the patient is liable to re-

lapse. This leads me to remark, that the most incessant watching is requisite for the well-being of a case of cholera; and I am convinced, that when the disease is epidemic a number of patients die from the impossibility of paying them that devoted attention which they would receive were it a single case. In the event of the purging returning, of the cramps being renewed, or any other symptom occurring indicative of a relapse, perhaps a dose of the cholera mixture, which consists of opium, brandy, and spices, or the timely application of a hot bath, may ward it off, and therefore a patient in cholera ought never to be left for a moment, as a quarter of an hour lost is irretrievable. Should the symptoms I have mentioned pass off, and be succeeded by a re-action of proportionate violence to the previous depression, then, but not till then, would I recommend the use of the lancet; and the extent to which the depletion must be carried is to be relative to the fever and local determination. The brain and liver are generally the organs affected in what may be termed the sequela of cholera. In one case which I attended, the brain became inflamed to an exceedingly violent degree, after a smart attack of cholera; the patient was bled largely both from the arms and temporal artery, with other depletory measures, before the febrile excitement could be subdued, and though ultimately he recovered his bodily health, he was an inmate for two years of the lunatic asylum. Another instance occurred where cholera was succeeded by fever of the pure typhoid character, which continued three weeks before the man became convalescent. One man was attacked with jaundice, attended with much fever, and after lingering many weeks, died a maniac. At Halifax, Nova Scotia, a man of the 34th regiment was admitted into hospital on the 13th September, having been drinking for some days before; he had incessant vomiting and purging, severe cramps, and great exhaustion; the countenance sunk, the pulse very small and weak, and the skin cold; it was considered by the medical officers a genuine case of cholera morbus. By the administration of appropriate measures re-action took place; there was considerable pain in the head, and heat of skin, attended with copious bleeding from the nose; the surface of the body became of a deep yellow hue,

petechial spots appeared on the limbs and breast, the tongue became dry and brown, large quantities of blood were passed by stool, and the patient sunk under these accumulated evils in seven days.

On dissection, the viscera generally were seen to partake of the yellow hue; the liver, except in colour, seemed healthy; the gall bladder was empty and collapsed, the internal coat of which, as well as of the cystic, hepatic, and common ducts, were of a red colour. The peritoneal coat of the intestines was throughout more or less injected; but on opening the stomach and intestines, the internal coat was of a dark or chocolate colour, which seemed to be caused by an extravasation of blood into the cellular tissue connecting the mucous to the muscular coat; the vitality of the mucous coat appeared to have been destroyed, as, with the point of the finger, it could easily be separated from the others; this appearance was less extensive in the jejunum and ileum than in the other parts of the alimentary canal. For this case I am indebted to my friend, Mr. Rankin, Surgeon of the 31th; and as it illustrates so well the position of cholera being merely a stage of fever, I deem it an invaluable addition to the present essay. By the description at the commencement, taken from the register, I am convinced that the case approached very near to the Indian cholera; and in the latter stage, in which I saw the man, there was the most exact resemblance to some of those alluded to above. The last case to which I shall allude, after suffering much from inflammation of the liver succeeding cholera, died dropsical. Other cases still fresh in my memory, after surviving the attack of cholera, remained long in a very precarious state. I am therefore inclined to believe cholera to be merely a stage of fever, such as Dr. Armstrong would have termed the congestive. It has never been my lot to witness the yellow fever, but I entertain a strong suspicion that there is a close connexion between it and cholera morbus; however, as I only profess to treat on what I have seen, I must here let the matter rest, at least for the present. Although little can be done in this dreadful disease when once established, for even under the most favourable circumstances a very large proportion of cases

terminate fatally, still there are many measures which, in a prophylactic point of view, should not be neglected, for, being adopted, they are attended with the most beneficial effects. I therefore would suggest, that when cholera appears as an epidemic, especial care should be taken to preserve the animal vigour, which is to be effected by a wholesome nutritious diet, rather stimulating than not, being careful to avoid whatever is indigestible, or likely to disagree with the stomach; therefore all rich viands should be abstained from, together with pastry and vegetables, avoiding excesses of quantity as well as quality, either in eating or drinking; that every person who has the means, should take a moderate quantity of wine or spirits daily; that the clothing should be warm, particularly about the abdomen and feet; that the bowels should be kept gently open, avoiding scrupulously purges of a drastic nature, at the same time being careful that the liver secretes properly; the mental vivacity is likewise to be sustained, avoiding all causes likely to produce despondency or lowness of spirits—in short, by every means to endeavour as much as possible to steer clear of debility, howsoever induced, it being the grand predisposing cause of cholera; and I venture to assert, that these measures will have infinitely more influence in saving us from its invasion than the quarantine laws, be they ever so strict.

I have hitherto said little upon the efficacy of general bleeding in cholera morbus, for though the adoption of it has been highly lauded by some persons in whom I have great confidence, it is, nevertheless, contra-indicated according to the principles which I entertain of the treatment. This difference of opinion may arise, as I have already stated, from those gentlemen having witnessed a different form of disease; and this is exceedingly likely to be the case according to the different stations occupied; for I think it were scarcely possible to observe a greater difference in the appearance of any body of men, than existed at one time between those of two of his Majesty's regiments, the one at Poonah, and the other at Colabah: those at the former quarter were stout, fresh coloured, and possessing almost an European vigour and health; the others were emaciated, pale, and debilitated. The climate of the two

places is sufficient to account for the whole; at Poonah the atmosphere is rare, dry, and elastic, the nights cool and delightfully refreshing; whereas at Colabah—in fact any where in what is called the Conean, or low country—the atmosphere is moist, heavy, and hot, relaxing to the greatest degree; and we had ample opportunity of judging of its effects upon the constitutions of the soldiers. We had the utmost difficulty in proportioning the doses of medicines to the constitutions of men who, under other circumstances, would have borne any treatment. In such an irritable state was the mucous membrane of the bowels, that one drachm of salts would produce twelve or fifteen evacuations; and I have known more than one case of cholera certainly induced by the most minute doses of salts or castor oil; on the other hand, when we had been even a short time in the Deccan, the men improved wonderfully, and required more than the ordinary doses of medicine to affect them.

To return, then, to the subject of bleeding; I can only add, we tried it in several instances, but only once with success. In this case the man was seen early, but slightly affected, and recovered. In fact it seems unnecessary to say more upon the subject, for if there be no blood in the arteries of the extremities, I cannot conceive how it can be expected that any should follow the puncture of a vein; and cases where there is free circulation in the extremities, are not those of severity, or at least are seen before the severe symptoms have come on. To shew how suddenly the disease appears, and how soon the circulation becomes suspended, I will adduce one more case.

I had seen a patient one morning at ten o'clock, who had been long suffering from chronic liver complaint and stricture; he was anxious that morning for me to discharge him, as he felt himself better than he had been for some time, and thought air and exercise would be of service. At half-past twelve I was called to the hospital to see this man, who had been attacked with cholera. Perhaps half an hour had elapsed from the commencement till I saw him in company with Dr. Strachan, Inspector-General of Hospitals; we found him completely prostrate, with severe spasms, incessant purging, but no vomiting; the skin of the extremi-

ties was cold and moist, whilst over the abdomen it was hotter than natural; not the slightest pulse could be felt at the wrist. Dr. Strachan desired some stimulus to be given him, and the man wished much for gin; he drank about four ounces of it, which produced for a few minutes a small thread-like pulse; other means before alluded to were adopted, but the man died in a few hours. Indeed, to think of bleeding a person under such circumstances would be contrary to every principle of common sense. It is not the quantity of blood in cholera which requires to be diminished, but the circulation and quality of it which require to be changed. Some sudden impulse should be given to the heart, in order to restore it to action, whereby alone the internal parts can be relieved of the congestion, and the balance restored; probably electricity might have the effect, but I have never had an opportunity of trying it.

ENGLISH CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

THE two following cases of severe cholera having fallen to my charge during the last three weeks, and as I am daily consulted for the relief of diarrhœa, I am tempted to ask the question whether “an epidemic constitution of the atmosphere may not be at present prevalent in London?” I never remember these disorders so frequent at this season of the year as they are now.

On Monday morning, October 31st, A. B., about thirty-two years of age, kitchen-maid in a family residing in Bryanstone-Square, got up to her usual labours about seven o'clock. She had scarcely arrived at the bottom of the stairs when she was seized with violent abdominal pain, vomiting of clear fluid, purging, and insensibility, or collapse. She was carried to the upper part of the house and laid upon the bed, the family believing her to be dying. I arrived to her assistance a little before eight. She appeared, certainly, more like a dead than a living body, and had very much the semblance of a shrivelled up old woman (her sister said that she

should not have known her). She answered every necessary question, but in a whisper; made no complaint of pain. I could not feel the pulse at the wrist, her hands were bloodless, and, with the feet, were quite cold. She complained of feeling intensely cold, and her head was like "cold marble;" there was also present a kind of convulsive sobbing. I covered her up with blankets, had five long stone bottles of hot water placed round her, and commenced the administration of stimulants. I considered it necessary, during one hour and a half (which was the time I was with her before I dared leave her) to give about two wine-glassfuls of brandy, five or six teaspoonfuls of spirit of sal volatile, and the same quantity of spirit of lavender, combined with a teaspoonful of laudanum, with two or three cups of warm tea. The stomach retained all this; there was neither vomiting nor purging. As soon as she was sufficiently recovered, she informed me that she had not been very well for some days, but during the night she had been harassed with pains about the insteps and back of the legs.

10 o'clock.—I found her much more comfortable, and, under the influence of the brandy, &c. she had taken, her pulse was full and bounding, skin hot, and her tongue, which was before quite clean, and I may say swollen, was now brown in the centre and dry. I expected a violent attack of fever, and that it might be necessary to remove her from the house; but having an appointment with Dr. Nevins on at one, I decided on leaving the case for his opinion, and I directed her to be kept covered up, to take warm tea, and prescribed as follows:

R Hydrargyri Submuriatis, gr. v.
Extr. Colocynth. Compos. gr. x.
Ol. Menih. Pep. gtt. i. M. it. pil. iij.
statim. sumendæ, c. dose, misturæ
sequentis.

R Magnes. Sulphatis, ʒj.
— Subcarbonatis, ʒj
Aque Menthæ Piper. ʒvj. M. ft.
Mistura Sumat. quartam partem
omni horâ donec alvus responderit.

1 o'clock.—I met Dr. Nevins on. The woman was in a most profuse perspiration, complaining only of slight pain in the head; in fact, all the symptoms of alarm had subsided.

She was perfectly recovered, and able to resume her usual occupation in two days.

Friday morning, November 10th, about nine o'clock, I was requested immediately to visit —, a cook, in Gloucester-Place. I found her complaining of burning pain at the pit of the stomach, with constant vomiting and purging (but I could not obtain any satisfactory account of the colour of these evacuations), and crying out with cramps at the back of the legs, beginning at the toes and extending up the thighs; the instant she put her hands out of bed, her fingers were clenched towards the palm, and the pain extended to her shoulder, &c. She had not been very well for a day or two, but was seized suddenly during the night, and had been writhing in tortures for some hours. Her countenance was very dark and ghastly; the housekeeper said she could not have believed so great an alteration could have taken place in so short a time. The fœtor was very great (nearly to excite nausea). I threw open the window, and directed her to be covered over with blankets, hot bottles to be applied, with friction, &c. and she took without delay,

R Hydrargyri Submuriat. gr. v.
Pulv. Rhei, gr. x.
Olei Carui, gtt. ij. M. ft. pilulæ, iij.
statim c. haustu sequente sumendæ.
R Spiritus Lavendulæ Compos. f. ʒij.
Spir. Camphoræ, ℥xx.
Tinctura Opii, ℥xx.
Aque Carui, ʒx. M. ft. haustus c. pilulis sumendus et post horam repetendus.

11 o'clock.—I again visited her, and met Dr. J. Clarke, of George-Street, he having been sent for by the family. The patient was now very much better; she had neither vomited nor been purged since taking the medicine; the cramp in the legs still continued; the pain at the stomach was less, but she was continually moving about in bed. Clarke was of opinion that she was much relieved by the means adopted, or he would have sent a message to Dr. Russell, who resides in the neighbourhood. Dr. C. advised that the draught should not be repeated for an hour, and directed a fire, to assist in ventilating the room.

1 o'clock.—I visited her again. She had relapsed; her thirst for cold drink had been intense, and she had indulged in some toast and water, which had been rejected by the stomach, and the cramps were increasing. I sent her directly,

Ol. Rorismarini, gtt. xx.

Magnesiae Subcarbonatis, ℥j.

Aq. Puræ, ℥iss. M. ft. haustus.

2 o'clock.—The draught had done her good, and I now repeated it.

4 o'clock.—Better; crying out occasionally with cramp, &c. The draught to be repeated.

6 o'clock.—She now complained only of great soreness all over the body; the cramps less frequent, but the moment she exposed her hands she had tingling and numbness of the arms. She had not voided water for upwards of twelve hours. I inquired what effect the medicine produced? She said, "every dose did her good."

R. Ol. Rorismarini, f. 3j.

Magnesiae Subcarbon. 3j.

Aq. Puræ, f. 3vj. M. ft. mistura, sumat 4tam partem 3tiis vel quartis horis.

Saturday morning, Nov. 12th.—Very much better; the cramps had ceased; her knees were so stiff and painful that she could not bend them. Urine had been abundantly passed during the night. To remain in bed.

R. Ol. Rorismarini, mxl.

Magnesiae Subcarbonatis, 3j.

Aq. Puræ, 3vj. M. ft. mistura, sumat 4tam partem 5tis vel 6tis horis.

Sunday morning, Nov. 14th.—Quite recovered. Her bowels had acted once freely, but comfortably. To get up, but not to leave her room.

I do not adduce these two cases (far from it) under the name of Cholera Indica, Asiatica, or Contagiosa; but I must maintain that the first might be denominated a mild case of cholera asphyxia, the latter of cholera spasmodica.

THOMAS BUSHELL.

117, Crawford-Street, Portman-Square,
November 19, 1831.

OBSERVATIONS

ON THE

CHLORIDES OF LIME AND OF SODIUM,

And Suggestion of some forms of Application, &c.

*To the Editor of the London Medical
Gazette.*

SIR,

It is not without astonishment we contemplate the little favour enjoyed in England by public hygiene. On the continent it is the subject of particular

study, and of general attention, and has given rise to many voluminous works of surpassing interest. Periodical publications are exclusively devoted to it; councils and commissions of men of science are established every where purposely for its superintendence, and statesmen are deemed incapable, or neglectful of their duty, who do not devote a due proportion of time to its encouragement. The remarkable increase of longevity in England of late years is principally attributable to those improvements in hygiene which, although unsought for, have followed the advance of the inhabitants in comfort, because in a certain degree inseparable from it. But if any one should deem these improvements sufficient, let him read, were it only the index, of the "*Annales de l'Hygiene*," and he will see the number of practical applications of science to the welfare of mankind which the public never hear of in England, although it is the fatherland of philanthropy*. However, the scourge now approaching will be an imperative monitor in favour of public hygiene, to whose lore it directs us as emphatically as a statue in a cemetery points to the legend of a tomb. Public hygiene requires not only men of science to invent and suggest, but the great mass of the nation to read and co-operate; and we must hope for the day when it will beneficially supplant that study of "Buchan" and the medicine chest, by which so many people drug themselves into disease, and not a few to death.

I hope these observations may be excused, though they may appear too grandiloquent, if considered only as prefatory to the humble suggestions I have to present; and even the latter may be deemed supererogatory if the cholera be considered as neither contagious nor infectious. However, the great majority of men of science are of a different opinion, and of the justice of this opinion we shall but too soon have personal experience. In many places where this fearful traveller has arrived, he has appeared at first in disguise, and has deceived the inhabitants by his equivocal character. Perhaps the peculiar locality, or the unfavourable season, has

* There are some peculiar forms of phraseology in this paper, which is from the pen of an intelligent foreigner; but as they are generally very expressive we have left them unchanged.

prevented his epidemical and contagious nature from appearing distinctly. But, after a certain time of incubation, when warmer and more relaxing winds have begun to prevail, he has rushed forth in all his epidemical fury, and falling upon some class of men of defective vitality, caused either by depravity or poverty, has revealed all his contagious banefulness.

Of all the chemical agents that have been employed to destroy effluvia, none can compete with the chlorides of sodium, lime, and potash. It is not, therefore, surprising, that on the appearance of cholera in Europe, that truly praiseworthy chemist, Labarraque, should have been applied to on the subject of his antiseptic process. Labarraque advises two glasses of water to be daily given, containing from thirty to forty drops of chloride of sodium; the hands and face to be washed with a solution of it; baths, with six or eight ounces of the liquid chloride in each; plates, containing the antiseptic liquor or powder, to be placed at the doors and on the mantel-pieces; and curtains of coarse linen to be hung before the windows, which are to be kept moistened with the chloride. Desirous of promoting this mode of purification, I wish to recommend some precautions in its use, and next some more convenient forms of application.

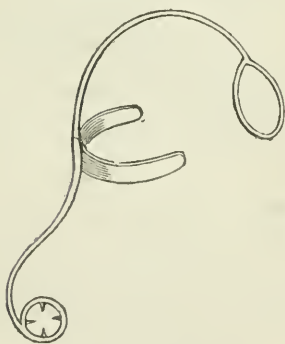
The chlorine which is evolved in the manner above recommended—a destroyer of colours, odours, and effluvia, in its unmixed state is also a destroyer of life. We know how rapidly an animal dies when immersed in a vessel of chlorine gas. We have also the examples of an English physician who died of a disease of the lungs occasioned by his experiments on this gas, and of several chemists who have suffered from the same cause. Although chlorine has been employed in the treatment of some diseases of the lungs, and although when diffused in a small proportion in the air it has not appeared deleterious, not only it cannot but deteriorate in some degree the natural atmosphere of respiration, but when employed in the abundant manner recommended above, we may fairly suppose that it may become highly deleterious, particularly to persons very young, very old, or very weak. We may suppose this to be the case even when no palpable effects are immediately produced, for may it not oppress the powers of life, just as we see in houses

either situated in a low or marshy locality, or near the common sewer, the miasmata slowly destroy the inhabitants, perhaps without producing even one single well characterized attack of fever?

To obviate, as far as possible, these objections, we would recommend the following measures:—Whenever it is desired to cleanse a house, or a room, of effluvia or impurities, a large quantity of one of the chlorides should be placed in the most favourable state for the evolution of the gas. Every body should then withdraw, taking care to remove all the coloured articles of furniture, gilt frames, &c. liable to be injured, and the doors and windows be shut as hermetically as possible. Thus the place will be more speedily purified; and doors and windows being thrown wide open after a due lapse of time, no person's health will be injured. When, on account of the presence of the sick in the same room, or in the same house, a constant evolution of gas is required, flat dishes full of the chlorides should be put only where there are the greatest currents of air, placing the dishes in as high a situation as possible; as it has been found that otherwise the gas, which is heavier than the atmosphere, will not mix sufficiently with the air, and only lie in a more concentrated stratum on the floor. If, from time to time, there is a suspicion of too large a proportion of chlorine existing in the room, let a little liquor ammonia be thrown on the floor, and the rising of a white vapour will reveal the existence of an excess.

When you walk through an infected place, or approach a person labouring under a highly-contagious disease, it has been recommended to keep a handkerchief to the mouth, wetted with the disinfecting liquid. This obstructs the speech, whilst the gas inconveniences the lungs, and the protection is liable to be removed in incautious gesticulation. I have had a simple contrivance made to send abroad, of which the design is subjoined; it has the advantage of leaving the speech free, and of being a protection from deleterious emanations, without depriving the lungs of communication with the usual atmosphere. This mask consists of a wire coming from the back of the head to project before the mouth, where it terminates in a ring, furnished with points, to which a sponge dipped in the solution of chloride of sodium may be attached. A thin and short band of brass crosses the wire, and fixes

this simple apparatus to the head immovably. Being elastic, this mask fits the head of every adult, and may be put on and taken off in a second by each



person going into the sick room, and causes no inconvenience in any movement of head or body. Besides its use in contagion, its adoption would save the lives of nightmen, who often sink in their disgusting duty.

Salts containing an excess of acid, when mixed in the dry state with chloride of lime, cause the slow evolution of gas by the new combination that arises. Mr. Garden, whom I have consulted on the subject of this letter, and whose chemical acumen often rests on ingenious minutia from more important pursuits, has had the kindness to construct a smelling bottle containing a powder of the above description. Hung by a ribbon round the neck, the heat of the body will favour the evolution of the gas when the stopper is removed. A more

effective protector than camphor and other agents carried on the person, this precaution will not alarm the sick friend or the patient. Should the cholera, or any other contagious disease, become very virulent, the medical attendant might wear cotton gloves, with the end of index and next finger of the right hand cut off. The gloved hands being dipped in the disinfecting solution, the pulse, or any other part of the body of the patient, might be examined without fear, the gravity of the fluid making it constantly flow to the apices of the exposed fingers. Nurses may wear oil-skin gloves, and an oil-skin smock-frock, closing hermetically round the throat, wrists, and over the chest.

Besides the chlorides many other "disinfectants" exist, and not a few of these whose source of power must remain concealed. It would appear that most bodies that emit strong odours possess a virtue against contagion. The cholera has had little influence in places where the forests in the neighbourhood have spontaneously ignited, and the wood has remained smouldering over the fire. It will be said, perhaps, that here heat and the pyroligneous acid have played important parts; but manufactories of snuff have been untouched by the cholera raging around, so that smoking has been enjoined in despotic countries where it was previously forbidden. Nor have examples been fewer during epidemics of the plague, where the centre of a town, the vicinity of the most nauseous trades, have remained free from the pestilence raging around. Therefore, the general confidence in the fumes of acetic acid, when not carried too far, is not without some reason for its support. To meet the wish of some friends to possess an antiseptic of this description, I have applied the acetic ether to the beautiful philosophical fumigator first imagined in Germany, and now manufactured in this country by Mr. Garden. It is constructed on the knowledge of the singular and well-known fact, that a platinum wire once ignited, remains in this state as long as it is held over spirit or ether. Thus a platinum fillagree, appended to a glass rod that dips into a scent-bottle full of acetic ether, may be made to diffuse it in the form of a very agreeable and exhilarating vapour throughout the room of an invalid.

HENRY BELINAYE.

Berners-Street, Dec. 5, 1831.

CHARACTER AND TREATMENT OF CHOLERA*.

To the Editor of the London Medical Gazette.

SIR,

At a period of excitement like the present, when not only the public generally but even the profession are in no small degree alarmed at the progress of malignant cholera, it becomes every one, who wishes well to the best interests of his species, to do every thing which may lie within his means to render the subject of these fears less the occasion of alarm. It has occurred to me that, in some respects, it was in my power to co-operate somewhat to the attainment of this most desirable object; and should you find space in the next number of your Gazette for the enclosed letter to Dr. Barry, and the subjoined remarks by which it is accompanied, you will oblige

Your obedient servant,
C. NEGRI, M.D.

26, Poland-Street, Oxford-Street,
December 1, 1831.

London, November 29, 1831,
26, Poland-Street, Oxford-Street.

Dear Sir,—After all I have read on the subject of malignant cholera, and after having paid particular attention to the character of the disease, it struck me very much how remarkable was its resemblance to that class of diseases so well described by Torti, in his valuable work entitled "*Francisci Torti Mutinensis Therapeuticæ Speciales ad Febres quasdam Perniciosas inopinato ac repente lethales, una vero China China, peculiarimethodo ministrata, sanabiles*;" which was published, for the first time, in 1709.

Speaking of the character of those fevers, Torti says, "The pernicious intermittent, more especially that wearing the tertian form, kills about the beginning of the paroxysm, when it is accompanied with violent bilious vomiting and purging of bilious humours, equally vicious both in quality and quantity, being sometimes clear, at others coloured, and occasionally of inspissated greenish bile; to which vomiting and purging are added, hiccup, a

hoarse sonorous voice, hollowness of the eyes, pain of stomach, small sweat upon the forehead, weak pulse, and cold or livid extremities—in one word, all the symptoms which usually mark *cholera morbus*; from which, however, this, as it were, *choleric affection*, is to be distinguished, since it is a mere symptom of the fever, the period of which it follows, as a shadow does a body."

These pernicious fevers he distinguishes into different species, according to the peculiarity of their symptoms, and I was much surprised in reading the two following descriptions of what he calls "*febris perniciosa cholericæ*." At the fourth, or, unless I am deceived, the fifth accession, such violent, frequent, and copious vomiting supervene, together with purging of bilious, serous, and corrupted matters, at very short intervals, that the patient becomes almost exhausted, universally chilled, lies supine, as though unable either to sit erect or turn on either side, with a pulse almost abolished, sunken eyes, hiccup, and difficult breathing.—"The wife of B. G., after having suffered from two paroxysms of a simple tertian, in which pain in the bowels, vomiting, and diarrhoea were such prominent symptoms that their intermittent character was almost overlooked, sustained a third attack still more severe, accompanied with acute pain in the stomach and intestines, constant vomiting, purging of immense quantities of watery fluid; in consequence of which she became cold and pulseless, with a mortal pallidity of countenance, sunken eyes, pinched nose, and collapsed temples. Her disease was regarded by every one as choleric, and her state was considered hopeless. I was, however, sent for, and, upon examination, found the patient almost destitute of pulse, scarcely able to speak, complaining much of intestinal pain, and so cold that I was wholly unable to decide whether the disease were fever or ordinary choleric."

Then, as to the cause of the irregular appearance of these pestilential fevers, he says, "These fevers, however, are, comparatively speaking, less frequent than the mild intermittent; so much so, that, in some years, physicians of respectable practice, have no opportunity of witnessing them. At the same time, however, I dare say, that, in some close states of the atmosphere,

* The original extracts were sent for insertion with the translation given by Dr. Negri; but from the late period at which they were received it has been impossible to comply with his wish by inserting them.

more especially near the dog-days, and after the autumnal equinox, they are so prevalent as to appear almost *epidemic*."

No direct mention of pernicious agues is made by preceding writers, with the exception of Ludovicus Mercatus, first physician to the Kings of Spain, who has given an accurate description of the nature and treatment of pernicious tertians. To give you a proof that the class of diseases described by Mercatus is quite the same with that which Torti depicts, let me transcribe the following passages from that author:—

"There is a certain pernicious tertian which simulates the character of an ordinary tertian ague, but is most fatal, and complicated by many most perilous symptoms." Then, dividing these fevers into different species, according to the peculiarity of their symptoms, he says; "the third form arises from the depravity of the humours, not from their putrescency, in consequence of vicious food, which is also the cause of *cholera morbus*, a disease that *very frequently lapses into pernicious fever*, as Valcrius very properly asserted; or it depends upon that fervent and depraved habit of body which glutinous and vitiated bile produce."

Many other points of resemblance with the present malignant cholera may be seen in Torti's Book, *lib. ii.* which is a complete commentary on Mercatus; till, at last, in the eighth chapter, he takes up from the celebrated English author, Richard Marton, the following passage. "When the febrile poison possesses an emetic as well as a cathartic nature, the patient is affected with frequent vomiting and purging; and unless its virulence is counteracted during the first stage of the fit, *cholera morbus* is formed, and the fever progresses without manifesting itself either by the *pulse*, the *temperature*, or the *urine*. In the meantime the stomach or bowels, or both, mortifying from the intense acrimony of their contents, the patient either rapidly yields to his fate, or, after a few deceitful respites, the same cruel and fatal symptoms re-appear in a more aggravated form at the next paroxysm, unless, by a seasonable exhibition of the specific antifebrifuge, they are dissipated."

And, lest periodicity, which all these quotations refer to, might be supposed to indicate the dissimilarity of these affections to malignant cholera, I beg to

refer you again to Morton's authority. "Among" (says he) "the innumerable symptoms attending these fevers, there is none which may not rise to a great height, endangering the life of the patient, so that *typhus fever* (masked in its stages of cold, heat, and sweating) supervenes, rendering it impossible to be distinguished by the *urine*, *temperature*, *pulse*, or indeed any other means; but, concealed under the appearance of cold, vomiting, diarrhoea, *cholera morbus*, colic, or other disease, not unfrequently mislead the physician. These symptoms, indeed, may be plied with the ordinary remedies, but in vain; for, after the first accession, the following exacerbation approaches, with symptoms either equally or more severe; so that, like the stone of Syphilis, the physician labours to no purpose, and the patient pays the penalty of his attendant's ignorance or inattention by the sacrifice of his life."

In addition to Morton's authority on this point, if Torti's work be consulted, many sentiments of a similar nature will be found.

But now comes, after all, the most important point for your consideration. What is the method of treatment in this class of diseases? As you may have already seen from Morton's last extract, the *anchora sacra*, on which we must rely for curing them, is bark, as may be seen by the following passage from Torti's work, *lib. ii. cap. i.*

"Indeed, without any feeling of partiality, or without laying any stress upon my peculiar views, which I consider of small moment, I look to bark to rescue my patients from a destruction which, under any other treatment, is inevitable, but which I can promptly and effectually prevent by means of this agent. And it is useless to ask, whether, in cases of danger threatening death, the cause of the ague be wholly or partially removed, and whether the cure be only palliative or permanent, since he that is in *articulo mortis* is rescued from destruction in whatever way his cure may be effected. If this object can be attained, I leave the *modus operandi* of my remedy for others to explain. When I can obtain a safe and certain cure in all cases, even the most hopeless, by a timely employment of *Peruvian bark*, I pass over all theories without respect, believing them to be very generally deceitful.

From all this it appears, that the point of importance is, to give *bark* as soon as we can, and in *large quantities*. Torti himself being ill with one of these fevers, says, "me metipsum epotis uno haustu six drachmis illius (Corticis) a morte imminente illico evasi, et etiam nunc vivo, et etiam nunc scribo,"—lib. i. cap. i. We employ in general the bark in those diseases in Italy, in doses of two or three drachms every second hour, or more, and oftener, according to the severity of the symptoms. Sometimes it is given in tincture, equivalent in strength to that dose of the *powder*, and it has been thrown up into the *rectum* in the form of *injection*, when the dose requires to be very large and frequently repeated. Morton mixed sometimes bark with a certain quantity of laudanum when there were vomiting and profuse purging. We use the same method, and I found it very beneficial in two cases of that kind which occurred to me in Italy. The best vehicle for bark in this instance is wine and water, or what is better, in my opinion, cinnamon water. I should object to the use of the sulphate of quinine because I tried it once, and it partially failed. Morton, in certain instances, bled his patient to relieve the circulation, and then he gave bark.

Having deemed these facts of considerable importance, and having now taken the liberty of laying them before you, I beg to ask first, whether you think that the *malignant cholera* of our days belongs to the same class of diseases which was seen by Mercatus in Spain, by Torti in Italy, and by Morton in England; and, secondly, if you think that bark ought to be fairly tried in cholera, *according to Torti's method*?

As bark has been since Torti's time considered as an *infallible* remedy in these *febres perniciosæ*, I dare say, that the same drug might be found of some efficacy in cholera, if *judiciously* administered.

You must not, dear sir, however, think me either presumptuous, or as pretending to the possession of a *specific* for this dreadful scourge; and may I request, that after you have directed your attention to what I have written on this matter, you will have the kindness to send me your opinion on the points which I have now submitted to your notice.

Believe me always truly yours, &c.

C. NEGRI, M.D.

OBSERVATIONS ON THE CHOLERA MORBUS, AND THE PREVENTIVE MEANS.

To the Editor of the London Medical Gazette.

SIR,

AT a period when the public attention is so much excited by the distressing accounts of the cholera morbus, and of some individuals in Northumberland having been afflicted by this alarming disease, it is the imperative duty of every medical man to impart whatever may be the results of his own reflections, with the hope that some useful practical results may arise. It is the subject of inquiry, whether the disease is to be considered contagious or infectious; by the former I understand such diseases as can only be imparted by actual contact; by the latter, all diseases where miasma are produced, and diffused into the surrounding atmosphere, and by this medium communicated to such constitutions where there exists a predisposition: it is to the latter I presume that the cholera morbus is to be referred.

I understand that in those countries where the cholera morbus has prevailed, it has been preceded by an influenza similar to that which we have lately experienced in this country; and I have been informed by some highly-respectable practitioners, that those who have previously suffered an attack of influenza, are more predisposed to the action of the cholera morbus. It has been generally noticed, that when the symptoms of this complaint have made their appearance, if a copious perspiration be produced, the greatest advantages are subsequently experienced from the employment of such powerful stimulants as opium, brandy, &c. All the symptoms evince great constitutional debility, the energies of life depressed by the poisonous miasma, and generally accompanied by the most excruciating spasms. The post-mortem examination evinces the stomach and intestines to be loaded with depraved matter, and an acid suspected to be acetic; and Dr. Davy remarks, that there is a great deficiency of carbonic acid in the process of respiration.

These observations lead me to submit to the public consideration a plan of

treatment, which I think ought to be generally adopted as a preventive, by destroying any predisposition in the constitution to take on the complaint. In the city where I reside, we have the incalculable advantages of public warm springs, by which the healthy condition of the surface of the body may be preserved. In a work I have lately submitted to the public, relative to the influence of the *Barbadoes Green Naphtha*, I have introduced some observations on the process of digestion, in which I have endeavoured to demonstrate, that the solvent principle in the stomach is the sub-carbonate of soda secreted by the middle portion of that viscus; whilst the hydrochloric acid, which is intended by nature to effect a subsequently separatory operation, results from the pyloric portion of the same organ: the contents of the stomach being found depraved and acetic acid evolved, demonstrate that the active solvent principle is not properly secreted; and as carbon is the most powerful corrective principle we can with safety employ in such cases of animal or vegetable composition, it becomes of importance to introduce it in that form in which it will most readily mix with the animal fluids. It is on this account that I earnestly recommend the *Barbadoes green naphtha*, in a weak solution of the carbonate of soda in water, to be taken every morning. By a continuance of this plan, the deficiency of carbon noticed by Dr. Davy would be supplied, whilst the important process of digestion would be carried on with results conducive to health, and thus constitute the best preventive as to any infectious complaint.

Yours respectfully,

C. H. WILKINSON, M. D.

Sydney-Place, Bath,
Nov. 6, 1831.

IMPORTATION OF CHOLERA INTO THE MAURITIUS.

*To the Editor of the London Medical
Gazette.*

SIR,

I BEG to enclose an extract of a letter I have received from a gentleman who held a high official situation at the Mau-

ritius when cholera made its appearance there: it bears directly upon the disputed point of importation.—Your's,

A. C. HUTCHISON.

Duchess-Street, Nov. 20, 1831.

November 16, 1831.

I PRESUME your query originates out of the assertion made in a letter signed Omega, in the *Courier* of the 10th inst. that the cholera prevailed at the Mauritius previous to the arrival of the *Topaze* on that station; upon which it is attempted to overthrow one of Sir Gilbert's proofs in support of his belief of the communicable nature of the disease by persons and things. I was particularly struck with it when I read it, and can assure you that no case of cholera was heard of at the Mauritius until nearly three weeks after the arrival of the *Topaze*. This vessel anchored in Port Louis harbour, on the 30th or 31st of October, 1819, and the first case of cholera that occurred to my knowledge was on the 19th of the ensuing month of November, in the person of a negro belonging, I believe, to the establishment of a Mr. Lucombe, who was engaged in removing goods to and from the quay, weighing them, superintending their landing, shipment, &c. I cannot take upon myself to say whether any cases of cholera occurred on board the *Topaze* after she left Trincomalee, but I think the number of convalescent cholera cases that were landed at the Military Hospital in Port Louis, was sixteen.

Too much time has elapsed to enable me to say whether the extract from Dr. Burke's letter be a faithful one or not, but of this I am certain, that the larger proportion of the French practitioners believed the disease to be infectious, and to have been introduced into the Island by the *Topaze*; and that, among the inhabitants, the belief was universal. If the documents of the Colonial Office be examined, it will be found that a violent controversy arose out of the difference of opinion between a Dr. Guillemeau, a member of the medical committee, and Dr. Burke, as to the character of the disease; and that the unanimity of opinion referred to by Dr. B. in the extract made from his letter, was not so perfect as Omega would have it believed.

A CASE OF
ULCERATED NÆVUS,

Successfully treated by the new Operation.

By MARSHALL HALL, M.D. F.R.S.E. &c. &c.

MR. HEMING, to whom I am already so deeply indebted, has again afforded me an opportunity of observing the effect of puncture in the cure of nævus. The case was doubly interesting from combining ulceration with the ordinary circumstances of that affection.

The tumor was oval, about one inch and a half in length, and three-fourths of an inch in breadth. The central part was undergoing the ulcerative process; around the ulcer the nævus existed in the form of a ring, about one-sixth of an inch in breadth. The edges of the ulcer were ragged, and slightly phagedenic; the surface of the ulcer was *malimoris*, and had bled from time to time. The nævus rose about an eighth or a tenth of an inch above the surface of the skin.

A common broad needle, with cutting edges, was passed through the substance of the nævus, at its base, and under the ulcer, from side to side, in every direction. Several punctures were made, but one would have been sufficient.

In the course of one week the ulcerative process had undergone the most interesting change into the adhesive. No better, or more beautiful illustration could be presented of the *Hunterian* doctrines. The ragged edges became smooth and white; the ulcerated surface covered with a film of coagulated lymph. In a few days more the whole ulcer was completely healed.

As a much slower process, the deposit of coagulable lymph encroached on the edges of the remaining ring of nævus, which in the space of one month had become reduced to one-third its original breadth.

At this period it was thought right, in order to accelerate the process of obliteration, to repeat the operation. But on examination, the process of obliteration of the ring of nævus was going on so beautifully that we determined to leave it for daily observation.

The deposit of coagulable lymph, with obliteration of the vessels, gradually

encroached upon the breadth of the ring of nævus, and destroyed its redness and tumor. Each successive week induced an obvious change. The whole process was so distinct, and so peculiar, that, however a shade of doubt might be cast upon the former case, none could possibly subsist in regard to the nature of the cure in this.

That every kind of vascular nævus, and even some tumors, morbid growths, and ulcers, may be cured by this simple operation, I have no doubt. A mode of obliterating vascular texture, and of changing vascular action, must have numerous applications in surgery. I have thought it, therefore, incumbent upon me once more to draw the attention of the profession to this subject.

14, Manchester-Square,
Dec. 6th, 1831.

DR. CAMPBELL ON PUERPERAL
FEVER.

*To the Editor of the London Medical
Gazette.*

SIR,

I AM desired by Dr. Campbell, of Edinburgh, the author of one of the most valuable works we possess on Puerperal Fever, to communicate to you the following letter for publication, which was addressed to me in the month of May last.—I am, sir,

Your obedient servant,
ROBERT LEE.

14, Golden-Square,
Nov. 15, 1831.

Edinburgh, 4, Picardy-Place,
May 26, 1831.

My Dear Sir,—Your obliging communication of the 17th April reached me only two days since, and from this unaccountable delay I fear you may not receive my reply in time to enable you to make any use of it.

My experience in the disease, which has of late engaged so much of your attention, has not been very extensive since the publication of my work on the subject; and of many such cases as have fallen under my notice, I am ashamed to acknowledge, that I have not preserved detailed records. Since

the close of 1822, the patients I have seen have not exceeded thirty in number: of these many died, and in most of them the examination of the body was obtained. In far the greater number, the peritonæum, generally including the uterine reflections, presented decided marks of inflammation. Both the ovaries, in most instances, were either simply enlarged or much disorganized. Though I observed that the uterus generally participated in the morbid changes, and in a few instances to a great extent, yet it was rarely so much affected, either in its external tunic or substance, as other parts of its investing membrane. In one instance only I have seen uterine phlebitis, though I have for several years past particularly attended to this point. This last was a case in which, as was obvious on dissection, injury had been done to the uterus by the medical attendant, in rectifying some malposition of the fœtus. The woman died early on the sixth day after delivery, and in four days and six hours from the commencement of rigors. Four days after her decease, the infant (a female) died of erysipelas and abdominal inflammation.

On the question of infection, I am as much as ever impressed with the belief, that unless the practitioner has been engaged in the dissection of the bodies of those who have fallen victims, the disease cannot be conveyed by him, from females labouring under it, to others recently delivered; but, if he has been so engaged, I have strong reasons for believing, that he may be the means of propagating it. In October 1821, I assisted at the dissection of a woman who died of the disease, after an abortion of the early months; the pelvic viscera, with the external coats, were removed, and I carried them in my pocket to the class-room. The same evening, without changing my clothes, I attended the delivery of a poor woman in the Canongate; she died: next morning, I went, in the same clothes, to assist some of my pupils, who were engaged with a woman in Bridewell, whom I delivered with forceps; she died: and of many others, who were seized with the disease, within a few weeks, three others shared the same fate in succession. Dr. James Orr, a discerning and judicious practitioner, who is now established at Belfast, as-

sisted me at the time. In June 1823, I assisted some of my pupils at the dissection of an unmarried female, who died of the disease at Canon-mills, after delivery with forceps. For want of accommodation, I was unable to wash my hands with that care which I ought to have done: on my arrival home, finding that two patients required assistance, I went to attend them without further ablution of my hands, or changing my clothes, and both of them were seized with the disease, and died. Since this last occasion, I have almost always allowed such dissections to be conducted by gentlemen who were not afterwards likely to be engaged with women in child-bed. For the last two years, I have scrupulously avoided, by *actual contact*, assisting at such post-mortem examinations; and although I have witnessed three, if not four dissections of the disease, and that it has been rather prevalent here during that period, yet I have not in any instance been able to trace its migration from the quarters where those examinations took place, to any part where either my pupils or myself were afterwards in attendance. My notions regarding the treatment are before the public, and I have little more to add on that head. Such patients as I have seen recover, were certainly indebted for the happy result to venesection; but my belief is, that there are some cases in which no sort of treatment can be of any avail, even when the disease is early attacked. In three cases, an enema, composed of lb. j. of warm gruel, and ℥ij. ol. tereb. vol., almost instantaneously relieved the *termina* and *tenesmus* under which the patients laboured; and were followed by copious, bloody, mucous dejections, and recovery. You are at liberty to make what use you please of the foregoing statement, or any part of it.

With many thanks, believe me, my dear sir,

Very sincerely yours,

WM. CAMPBELL.

COMPOSITION OF CINCHONA.

To the Editor of the London Medical Gazette.

SIR,

I REQUEST the favour of the insertion in your journal of the following notice on *Cinchona Cordifolia*.

The constituents of the *cinchona cordifolia* are ascertained by analysis to be—

1st, A free acid, easily disengaged by distilled water; 2d, quinine, obtained by neutralizing the acid No. 1 with magnesia; 3d, quinine independent of that which is in combination with the acid No. 1; 4th, bitter extractive; 5th, resin; 6th, gummy matter; 7th, gluten; 8th, tannin, combined with gallic acid; 9th, colouring matter; 10th, muriate of soda; 11th, sulphate of soda; 12th, iron; 13th, woody fibre.

Quinine is a bitter substance of the bark, forming characteristic salts with acids, and not containing any of the principles from No. 4 to 13; it appears to be an elementary substance.

In the preparation of the *liquor cinchonæ cordifoliæ*, it has been attempted to combine all the efficient properties of the bark (excluding Nos 6, 7, and 13, in which no active principle can be discovered), and it is presumed that the object has been effected in that preparation.

The *sulphate of quinine* can only be partially efficient as a medicine, in consequence of the absence of all the properties above mentioned (from No. 4 to 13); and the decided superiority of the *liquor cinchonæ*, as a medicine, being fully admitted by many competent judges, it appears to me to be of importance to place that preparation under the attention of the profession at large.

I have the honour to be, sir,

Your obedient servant,

R. BATTLEY.

Ophthalmic Laboratory, Moorfields,
Nov. 15, 1831.

VARIETIES OF MAGNESIA.

To the Editor of the London Medical Gazette.

SIR,

IN the Medical Gazette of August 13th, 1831, is a case of poisoning by sulphu-

ric acid, by Martin Sinclair, M.D. Fellow of the Royal College of Surgeons of Edinburgh.

I request your insertion of a few remarks suggested by the following passage in the account, (vide Med. Gazette, Aug. 13th, 1831, page 625, first column.) The passage is as follows:—“By medical writers the carbonate of magnesia is recommended in preference to the carbonate of lime, and is certainly superior, by producing a saline aperient by combining with the sulphuric acid. In the present case, however, the carbonate of lime was preferred as having a greater affinity for sulphuric acid, and because being more easily missible with liquids, it could be administered in greater quantity in a short space of time.” This latter clause would indicate that the writer was acquainted only with the ordinary carbonate of magnesia of commerce, prepared from the residuary liquor of sea-water, after the extraction of common salt. This is light, mixes imperfectly with water, and occupies a large bulk even when mixed; it has a taste which, though not very readily discoverable on placing a small quantity on the tongue, is to many very disagreeable in a copious draught. This taste appears to depend in some degree on the presence of chlorides and sulphates of magnesia, lime, and soda, left behind in the process of washing the carbonate of magnesia. But since the nauseous flavour of sea-water is chiefly owing to dissolved animal and vegetable matters, and is scarcely perceived in water taken up at a great distance from any shore, I know from experiment so much of the readiness with which soluble, animal, and vegetable substances unite with and adhere to all the common earths, (I waive the question of a real combination) that I should expect this flavouring matter, too minute and subtle, perhaps, to be at present detected by our chemical tests, to adhere to, and be precipitated with the carbonate of magnesia, rather than to remain dissolved, and be washed away. Certain it is that I do not find on analysis of light carbonate of magnesia so large a portion of muriates or sulphates as fully to account for its disagreeable flavour. But there is another carbonate of magnesia to be met with in trade which I have been in the habit of recommending and supplying, and which is approved, I believe, by all who have tried it: this is known

as pure, or heavy carbonate of magnesia.

An ounce measure, lightly filled, weighs about 160 grains; of the common sort, the same bulk weighs 48 grains. This heavy carbonate is free from taste; it is not prepared, in any stage, from sea-water, but from solution of sulphate of magnesia, free from lime, from muriates, and from vegetable bitter. There is another somewhat curious reason of trade for its closer approach to purity. Carbonate of magnesia, when first precipitated, is very light, and of course much mixed with the solution of the alkaline precipitant. Repeated washing, while it carries off the alkaline salt, breaks down that crystalline structure on which the lightness depends, and leaves it heavy. A few, and but a few generations back, carbonate of magnesia was many times its present price, so as to be worth adulterating with carbonate of lime; and, moreover, that ready and infallible test for chalk in such a mixture, its not dissolving entirely in moderately diluted sulphuric acid, was unknown, or in few hands.

Medical practitioners had then no easy way of judging of its freedom from adulteration with chalk but from its extreme lightness, and to procure it light rather than tasteless, was the great desideratum of the laboratory.

Neither the spread of chemical knowledge, nor the example of Dr. Henry's superior preparation, has entirely abolished this prejudice, and hence the manufacturers of common carbonate of magnesia have a strong motive to wash slightly, in order to preserve that lightness which is now a real defect without a countervailing advantage. The manufacturers of pure, heavy carbonate of magnesia, who do not aim at lightness, have not this motive to spare washing, and their preparation is as much distinguished by mixing readily with liquids, and by freedom from taste, as it is by chemical purity. I think no one accustomed to its use could write, "carbonate of lime was preferred because more easily missible with liquids;" it is actually less so.

Two things I wish to guard against; first, the being supposed to possess any nostrum in magnesia, or to claim the exclusive source of supply for the heavy carbonate, or the calcined magnesia from it, although, in my own con-

nexion amongst apothecaries, I am in the habit of furnishing them largely with this magnesia.

Next, the appearance of imputing any blame to the medical practitioner who treated the case. He seems to have acted with zeal and promptitude, and the same termination might have taken place under any other management; but I beg leave to point out a circumstance forming a reason for preferring magnesia, or its carbonate, to chalk, which he has not noticed, whether aware of it or not, and which in other cases is quite likely to make all the difference between *life* and *death*. It is, that from the comparative insolubility of sulphate of lime, when chalk is added to dilute sulphuric acid, however large the quantity of either, the action of the sulphuric acid on the carbonate, and its saturation with lime, soon stop, from the first portions of sulphate forming a crust round the carbonate, protecting it from the further action of the acid. Even agitation, without actual crushing, is insufficient to bring the whole of the carbonate of lime within the sphere of action of the sulphuric acid. There may exist together, therefore, in the stomach a great mass of carbonate of lime, enveloped and protected by sulphate of lime, and at the same time a large quantity of sulphuric acid, either kept wholly separate from, or very slowly acting on, the chalk, but exerting the full force of its deadly energies, chemical and physiological, on the structure or functions of the stomach, without check or controul. With magnesia this cannot possibly be the case. I should recommend as the best preparation in every such case, if at hand, Henry's calcined magnesia, or the pure calcined magnesia obtained by calcining the carbonate above described, on account of avoiding distressing efforts at eructation in the corroded state of the œsophagus and surrounding parts. Next to these, pure heavy carbonate of magnesia, or even supercarbonate of soda, and chalk only as a temporary substitute, in cases occurring in the country, where chalk might be in the house, and a mixture could be prepared and administered whilst a messenger was on the way to the nearest place where some variety of magnesia could be obtained.

WILLIAM WEST.

Leeds, 31st of 10th month, 1831.

DR. STEVENS'S ACCURACY QUESTIONED.

To the Editor of the London Medical Gazette.

Suffolk-Place, Pall-Mall,
5th Dec. 1831.

SIR,

ANXIOUS that no misconception or misinterpretation of my statement in the Westminster Society, relative to Dr. Stevens, may go forth to the public, I solicit a corner in the next number of your journal for the following remarks.

A few weeks ago, I received a rather voluminous document, with enclosures, from Dr. Hackett, chief medical officer at Trinidad, charging Dr. Stevens with misrepresentation of facts, as they occurred at Trinidad, and as they formed certain data for his doctrine and treatment of fevers; also of garbling, to suit his purpose, a letter from Mr. Greatrix, relative to the alkaline treatment of yellow fever. These and other grave charges were, in my mind, calculated to do injury, or even disgrace, to the medical profession, and, without perusing any more of the document, I laid it aside, determined not to make it public. But my attention being roused by the announcement of Dr. O'Shaughnessy's paper, and more especially by the very strong terms in which Dr. Stevens's discoveries were spoken of in the Medical Gazette, in which it is stated that Dr. Prout considered these discoveries as of the last importance to mankind, I felt that it would be culpable, if not criminal in me, to withhold from the profession the counterstatements of the army medical officers of Trinidad, from which it appears that soda had nothing whatever to do with the success of the treatment in the above island; that the blood of *sodafied* patients, after death, was found to be precisely similar to that of the *unsodafied*; and lastly, that the changes, real or apparent, produced by the admixture of soda with blood, out of the body, could be produced, without any soda at all, simply by putting the patient into a hot bath, and bleeding him in the bath.

On perusing the whole of the document, I am happy to say that it contains the most important mass of information, both pathological and therapeutical, which has been laid before the public for many years, and that it brings forward views which may tend to elucidate

the nature and treatment of the reigning epidemic. On this account I shall publish the document forthwith, leaving Dr. Stevens to justify his conduct and support his hypothesis in the best way he can.—I am, sir,

Your most obedient servant,
JAMES JOHNSON.

EXPERIMENTS ON THE BLOOD.

To the Editor of the London Medical Gazette.

SIR,

THE notices repeatedly inserted in the Medical Gazette, of certain opinions advanced by Dr. Stevens, regarding the effects of some of the neutral salts on the blood, have naturally directed attention to this point, at a time when the failure of all ordinary methods of treatment in cholera has almost unavoidably led practitioners to inquire, whether there be any yet untried expedient which might by possibility be useful. About a month ago, having procured successive supplies of newly-drawn blood, I mixed it with different substances, obtaining the well-known general result, of rendering the fluid dark and thick by means of strong acids, and of a bright scarlet by means of the neutral salts. The transition from the modena hue of venous, to a vermilion *resembling* that of arterial blood, is certainly a remarkable phenomenon, and those present agreed, that if any thing was to be hoped for from effecting a similar change in urgent cases of cholera, it was most rationally to be attempted by direct injection into the veins—a process which Mr. Arnott undertook to perform, should circumstances occur to render the proceeding feasible. Meantime, however, reflecting that the change of colour had been most conspicuously produced by a salt (muriate of soda) containing oxygen, in a state of combination (with sodium) in which it was next to impossible it should be separated, I very much doubted whether any thing like oxygenation took place, and this idea was farther strengthened by recollecting, that though nitrate of potass contained abundance of the principle in question, in a state readily separable, yet the salt passed through the circulation unchanged, was eliminated

by the kidneys, and might be obtained from the urine, still as nitrate of potass. Under these circumstances, the only rational conjecture which presented itself to my mind was, that the change of colour was effected by some process not involving the decomposition of the salts, but analogous to the phenomenon displayed by acids, in reddening the blue vegetable infusions. In this dilemma, I applied to Dr. Prout, in the hope of obtaining from him some additional light upon the subject; but he informed me explicitly, that though long engaged in investigating the effects of different agents on the blood, he had not been able to satisfy himself as to the manner in which the change alluded to was effected. Thinking there was little chance of my finding out what the first of our organic chemists had failed to discover, but still desirous of making some advance towards ascertaining whether the salts would act in any degree as a substitute for the arterialization of the blood, I tried the following experiment upon rabbits:—The windpipe was compressed till the animal was rendered insensible by the circulation of black blood, after which, and while the heart still continued to act, solutions of muriate of soda and of nitrate of potass, of various strengths, were injected into the jugular vein. In no case did any degree of resuscitation, however short or trivial, indicate that any influence was produced by the injection; nor did the heart act longer where the injection was fairly thrown in, and where by examination afterwards the blood was found to have been reddened by it, than in another case where the pipe had slipped from the vein, and the solution passed merely into the adjacent cellular texture.

In another instance, a solution of nitrate of potass was injected into the carotid artery, so as to make it pass at once to the brain, and the animal recovered, but it was found that respiration had been too imperfectly interrupted to render the experiment at all satisfactory, and I have had no opportunity of repeating it.

I admit readily that these observations and experiments are not sufficient to warrant any positive inference, and that the subject is deserving of investigation, but so far as they go, they are not calculated to strengthen our hopes

of benefit from the suggestion contained in your last number.

My reason for giving publicity to any remarks so imperfect is, that I wish it to be understood exactly to what extent I have offered any opinion. The *Morning Post* has erroneously identified me with all that has been said in the *Medical Gazette*, but this is at once an honour and a responsibility which, not being due to me, I must decline. When, at a recent public meeting, the *Medical Gazette* was accused of endeavouring to take the credit which belonged to another, I was enabled to contradict the statement, seeing that my own attention had been particularly directed to the subject in consequence of what had previously appeared in the pages of that journal.—I am, sir,

Your obedient servant,

R. MACLEOD.

Henrietta-Street, Cavendish-Square,
December, 5, 1831.

MORINDA CITRIFOLIA.

THERE are two species of *Morinda*, the *umbellata* and *citrifolia* (small and large leaved), which are found indigenous and abundant among the islands forming the eastern Archipelago, where they are used as props for the pepper-vines, or planted as a shade for the coffee-plants, and is named by the natives *Mangkudn* *. The roots of the first are only mentioned as being used as a dyeing material in the eastern Archipelago. The *Morinda* is indigenous also to the Philippine Islands, where it is named in the Tagalo *Tambungaso*. The natives of these islands, when a limb is fractured, use the leaves of this shrub, anointed with oil, to lay over the surface of the fractured limb; and it is considered by them of benefit in allaying the inflammatory action.

This shrub attains the height of ten or twelve feet. At Tahiti, and most of the Polynesian islands, where it is also found indigenous, the inner bark of the root is used for dyeing the native cloth of a fine yellow colour: this is done by infusing the bark in water, into which

* In the languages of the western countries of the Archipelago, the tree is named according to the idiom of the pronunciation of the people, *Mangkudu*, *Bangkudu*, or *Wangkudu*; the three initial consonants in these cases being commutable and very arbitrarily used.—*Crawford's Indian Archipelago*.

the cloth intended to be dyed is afterwards placed, and being suffered to remain for some hours, is taken out and dried in the sun. At Tahiti the shrub is called Nono, or Aari; at the Sandwich Islands it is called Noni. The fruit is eaten at some of the Polynesian Islands, in seasons of scarcity.—*Mr. G. Bennett's MS. Journal on Polynesia, &c.* 1829-30.

THE MAMI TREE (ANTIARIS) OF
THE ISLAND OF TUCOPIA, SOUTH-
THERN PACIFIC OCEAN.

Natural Order, *Urticæ inter Brosimum et
Olnediam.*

Class, *Monocia*.—Order, *Tetrandria*.

DURING a brief visit to the Island of Tucopia, on the 7th of May (1830), I observed this tree planted in rows, near the native dwellings, but whether it was indigenous to the island I could not correctly ascertain. It is allied to the far-famed Upas-tree of Java, and seems to accord with the *Antiaris macrophylla* described and figured by Mr. Brown, in the Appendix to Flindel's Voyage, vol. ii. p. 603*. It is called Mami by the natives.

The tree is of slender growth, with pendulous branches. I have seen it growing to the height of eight and twelve feet; the leaves are oblong, large, pointed, distinctly veined, and of a light green colour; the fruit is oval, rather longer than a pigeon's egg, rough externally, and of a beautiful crimson colour. On making a section of the fruit, between the husk and kernel is a quantity of white viscid juice; the kernel, of a white colour and intensely bitter taste, is enclosed in a thin shell, of a greyish colour. It is used by the natives either in dyeing or manufacturing their native cloth, but which I could not, from my short stay, exactly ascertain.—*Mr. G. Bennett's MS. Journal on Polynesia, &c.*

* He describes it as "a shrub, or very small tree, observed in stony places, on the shores of the Company's Islands, adjacent to Arnhem's Land, on the north coast of New Holland, in about 12 degrees S. latitude, bearing both flowers and ripe fruit in February 1803."

SPONTANEOUS COMBUSTION.

On the 23d of March, 1830, when at the island of Rótuma, Southern Pacific Ocean, I had requested a native to bring me some of the sweet-scented male flowers of the Pauhuf (*Pandanus odorotissimus*); they were accordingly brought on board, enveloped in native cloth, and packed in baskets formed of the cocoa-nut frond; and having been brought off to the ship during a heavy sea, were wetted by the spray. I laid them aside unopened in my cabin. Some hours after, looking at them I observed a steam arising from the basket; and, on taking them out, found that a spontaneous combustion had taken place among those situated underneath, most of which had become completely blackened, and the heat which proceeded from them was very great. Had they been incautiously stowed in a ship's hold, the consequences might have been very serious.—*Mr. G. Bennett's MS. Journal, 1830.*

LUMINOSITY OF CORAL INSECTS.

On the 20th of March, 1830, when coming off to the ship at Thor Bay, island of Rótuma (Southern Pacific Ocean), some hours after dark, a long reef of coral extended some distance from the beach, some part of which was dry at low water, and at other places the water was very shallow. The canoe in which I was going off grazed with some degree of violence on the coral nearly at the termination of the reef, when the surface of the water immediately became brilliantly phosphorescent, and remained so for a brief period. The water at other places I did not observe, this night, displaying any phosphorescent light: can we infer from this that the coral zoophytes have luminous properties? — *Mr. G. Bennett's MS. Journal.*

THE SUGAR CANE—(SACCHARUM
OFFICINARUM.)

THIS valuable cane is indigenous to South America, Persia, East Indies,

China, and the Polynesian Islands, and of it there are several varieties. At Polynesia, as in the Eastern Archipelago, it is only cultivated by the natives for eating in a raw state. Sir Stamford Raffles states, that in Java they have eight varieties, and the sugar cane is designated by the name Tebu, both at that island and throughout the Polynesian Archipelago. Among the varieties at Java, the dark purple cane, "which displays the 'greatest luxuriance, and shoots to the length of ten feet, is the most highly prized;" the Javans, as well as the Polynesians, are unacquainted with any artificial mode of expressing the saccharine juice and granulating it. Among the Polynesian Islands I have seen it attain a larger circumference, and a greater length of eatable cane (from ten to twelve feet), at the Sandwich Islands and Tahiti, than at any of the others. At Tahiti Mr. Bicknell, and Mr. S. P. Henry, manufacture sugar from the canes, and the quality is most excellent. At the Society and Sandwich Islands, Rótuma, Tongatabu, New Hebrides Group, &c. I observed several varieties, differing in the colour and quality of the stalk, and in a very slight degree also in the leaves.—*Mr. G. Bennett's MS. Journal on Polynesia, &c.*, 1829-30.

ANALYSES & NOTICES OF BOOKS.

L'Auteur se tue à allonger ce que le lecteur se tue à abréger.—D'ALEMBERT.

An Essay on the Epidemic Cholera of India. By REGINALD ORTON, Surgeon H. P., late of his Majesty's 34th Regiment of Foot. Second Edition, with a Supplement. 8vo. pp. 488.

It would be occupying the pages of our journal with matter already familiar to our readers, were we to enter into minute details regarding the history and symptoms of cholera. Had the work before us presented itself sooner to our notice, we should have borrowed largely from its pages, as containing upon the whole the most complete account of the disease which has yet been offered to the public. But though we must thus pass by much of its contents without notice,

there remains behind a large portion of highly-interesting discussion, contained in an extensive supplement and appendix. In these some of the opinions contained in the first part, which has scarcely been altered from the former edition, are considerably modified—particularly that regarding contagion; the accumulation of evidence having produced on the mind of Mr. Orton the same conviction which must, sooner or later, be admitted by every unprejudiced person—namely, that though the assumption that the disease is transmitted from man to man is not without some difficulties, yet these are "as dust in the balance," compared to those which present themselves if we adopt the opposite hypothesis.

In the supplement we find the opinions of the writer thrown into the form of propositions; and the first of these relates to the contagiousness of the disease, concerning which the inferences are—1st, that it is conveyed either mediately, or immediately, from man to man; 2d, that the virus is very subtle, or volatile, and is readily conveyed by the atmosphere, whence it happens that there is little, if any, increase of danger from the most intimate communication with the sick, above what attends the common intercourse of society; 3d, that the period during which the poison lies dormant is generally very short, and sometimes imperceptible, though at other times it is more protracted. A full account of the chief points tending to prove the contagious nature of cholera having recently appeared in this journal, we do not think it necessary to go over the ground again, but can confidently refer those desirous of farther information to the lucid observations of Mr. Orton.

This brings us to the second section, treating of the influence of the seasons and states of atmosphere on the disease, on which subject the propositions laid down are as follow:—

"Great irregularity and intemperature of the seasons has preceded and attended the rise and first spread of the epidemic over India.

"The disease has prevailed in India chiefly during that half of the year when the sun was on the same side of the equator.

"A high atmospheric temperature is a great cause of the disease.

"A condition of the atmosphere

indicated by the formation of thick clouds, heavy rain, and storms (particularly thunder-storms), or the immediate approach of these phenomena, is a great cause of the disease."

Mr. Orton, in commenting on the preceding positions, alludes to the opinions of various writers who have taken a different view of the subject; and certainly not a few, both at home and abroad, have been opposed to him on this point. He observes, however, that a high atmospheric temperature, especially in connexion with heavy rains, is acknowledged to be a principal cause of that form of cholera which prevails in Europe, to which the sporadic cholera of India is closely allied—so closely that, according to Mr. Orton, in the symptoms no distinction can be drawn between them; and he goes on to shew, by reference to numerous Indian reports, that similar states of atmosphere generally attended the most severe visitations of cholera; and we must say, that he adduces some very remarkable facts, particularly as regards the immense quantity of rain which fell in some places.

"Such and so striking (says our author) being the circumstances attending the rise of the malady, and its first and principal ravages all over Lower Bengal, we may fairly infer that it was owing to this exaltation of the common causes of the endemic or sporadic disease that it took on the epidemic and contagious form, and thus became capable of diffusing itself far and wide over the earth."

The influence of season is farther shewn by a reference to the calculations of Mr. Jameson, as to the appearance of the disease in separate camps during the different months of the year; and these being thrown into a tabular form, stand thus:—

January.....	1
February	5
March	10
April	18
May	23
June	14
July	10
August	22
September.....	16
October.....	7
Novembsr.	5
December	0

From March to September, the number of irruptions of cholera, 103; whilst
210.—IX.

in the other half-year it is only 26; and during the months of December, January, and February, or half the latter period, the number is only 4. The author establishes a clear line of distinction between the first and subsequent visitations of the disease. In the first it prevails among masses, many of whom are susceptible, and carries off great numbers; but afterwards, the "original stock of susceptibility" having been exhausted, the contagion only lurks among the people, to become developed when the atmospheric condition is peculiarly favourable.

The third section is devoted to the discussion of the proposition, that "troops on their march from station to station, or in the field, and persons travelling in India, are particularly subject to the disease;" and the evidence adduced in illustration of it is such as must convince any one who takes the trouble to peruse it.

Section fourth brings us to the influence of localities on the prevalence of the epidemic; and the propositions regarding this point are as follow:—

"PROPOSITION.—The disease shews an evident preference for the low and level parts of a country, and avoidance of more elevated tracts.

"It is found to prevail most severely in all the moist, close, and filthy parts of any city or neighbourhood.

"It commonly follows the course of rivers, both navigable and unnavigable.

"It prevails most severely in all those countries and places where, from the prevalence of the endemic fever, malaria is known to exist; and in such situations its epidemic duration has commonly been protracted."

It appears to have been on the first spreading of the epidemic that the exemption of particular situations was most remarkable; for, in its subsequent visitations, it generally reached the places it had previously left untouched; and in some instances, on its second journey, if we may so speak, it visited those places only which had previously escaped. This, however, was not the fact generally. On most of these occasions it would appear that the disease displayed a greater degree of malignity than at first, attacking those who had before seemed unsusceptible of its influence. High situations are undoubtedly, as a general rule, less subject

to cholera than those which are low; and dry spots are more exempt than damp; while in any locality a dull, moist, hot state of atmosphere is more favourable to its development and ravages, than one which is cool and clear. It seems never to have reached the interesting plateau constituted by the Neelgherry mountains, though it prevailed extensively in the country bordering upon them. Where fevers were endemic, cholera was sure to prove severe, especially on the banks of swampy rivers, a fact attributed by our author to two causes—"human intercourse and malaria."

The fifth section relates to the prevalence of other diseases in connexion with that of cholera; and the proposition is,

"The extensive prevalence of various other endemic or epidemic diseases, particularly the malaria fevers, has often accompanied or immediately preceded or followed that of the epidemic cholera."

It appears that in 1816, being the year before cholera broke out in Bengal, there was an extraordinary prevalence of remittent fever, and likewise of an "infectious malignancy nanche;" while, on the other side of the peninsula, on the coast of Kutch, the plague made its appearance, and spread to a considerable extent. Besides these unusual visitations, it seems clearly proved by the evidence before us, that the common diseases of the country were increased in prevalence during the existence of cholera. In some instances the epidemic soon passed into very severe remittent fever, of which the patients sometimes died "almost instantaneously, with every symptom of apoplexy." All this goes far to prove, that there exists a peculiar state of atmosphere, which facilitates the development of cholera, and aggravates its violence; and farther, that this epidemic atmospheric peculiarity likewise occasions or favours the irruption of various other diseases.

Section sixth treats of susceptibility to the disease, and of its exciting causes, giving rise to these inferences:—

"In every body of people there is a large proportion incapable of receiving the disease, though exposed to all its usual causes.

"Having undergone an attack of the disease confers a great de-

gree of immunity, at least for a time, from its future attacks.

"The indigent part of society, the old, the weakly, or unhealthy, and the intemperate, are particularly liable to the disease.

"The usual exciting causes of the disease are exposure to great heat, cold, or moisture, errors of diet, over exertion, the depressing passions, and in general whatever tends to debilitate or disorder the frame."

In India, not less remarkably than in Europe, has the disease been especially prevalent among the lower classes of society—the ill-fed, ill-lodged, and dissipated; and, taken altogether, the proportion of persons susceptible of the malady is small in comparison to those who fall under the influence of other epidemics, as plague, or yellow fever. It is, however, very difficult to say on what peculiar condition of the system the susceptibility depends; for while some, such as those above alluded to, are obvious, there seem to be others entirely hid.

"How often (says Mr. Orton) have we seen persons of the most robust and healthy habit, and the last whom we should expect to be obnoxious to cholera, seized by it, even when it was not prevailing to any considerable degree, in its very worst forms! Doubtless the disease found in them some congenial diathesis, some hidden flaw in the constitution, by which it was enabled to make its fatal invasion."

Section the seventh and last, consists of a comparison between the Indian and European cholera. Their identity is admitted, but considerable difference is held to exist with respect to the febrile state which follows the first stage of the disease in Europe. To this circumstance Mr. Orton is inclined, and we think with much justice, from the greater development which has occurred of the contagious principle, and the power evinced by the disease of resisting cold—a power which it has apparently acquired since it entered Russia. The extraordinary prevalence of sporadic cholera all over England is particularly alluded to, as shewing a peculiar state of atmosphere; and the valuable Reports of Dr. Burne, published in this journal, July 2d, are especially referred to, in corroboration of our author's views as to the relation-

ship between English and Indian cholera. It is curious, too, that Mr. Orton fixes upon Sunderland as a place likely to display the more virulent form of the disease, although the opinion must have been written some weeks before its actual irruption.

"I have also (says our author) the authority, *vivâ voce*, of Dr. Clanny, for the most unusual prevalence and malignancy of cholera at Sunderland during the present autumn. It is greatly to be feared that these are but the skirts of the approaching shower. This unusual prevalence of common cholera remarkably accords with the same fact, as already stated, occurring on the peninsula of India the year before the epidemic reached it, and when Bengal was actually suffering its ravages."

We must here close our short account, or rather our notice, of Mr. Orton's valuable work: to give a faithful digest of its contents would require several entire numbers of our journal. Every medical man desirous of making himself well acquainted with the all-engrossing subject of which it treats, ought to have the volume in his possession; and we trust we have said enough to induce them to study its contents.

MEDICAL GAZETTE.

Saturday, December 10, 1831.

"*Licet omnibus, licet etiam mihi, dignitatem Artis Medicæ tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso.*"—CICERO.

WHAT OUGHT THE ANATOMISTS TO DO ?

NEVER, we believe, since anatomy was first cultivated in this country were the teachers of it reduced to such awkward extremities as at present. Shackled with a weight of responsibility from the obligations entered into with their pupils, they cannot possibly throw up their business, much less can they adopt the rash expedient (which we are sorry to hear has been in actual contemplation)

of calling on government to close their schools—while a mass of difficulties arising from the spirit of surveillance, which has been awakened by the disclosure of the recent atrocities, and from the unusual degree of odium attaching to their profession in consequence, must not only interfere to prevent the fulfilment of their engagements, but serve to drive them almost to desperation. In this painful predicament they, of course, must look round them to see what is best to be done: and we can very well imagine the anxiety with which they turn from public feeling to the press, and from the press to the prospect of parliamentary relief. From public feeling, we fear, they have nothing to expect: if the schoolmaster has been abroad, here at least he has not done his duty; and little or none has been the improvement which has taken place among his precious dogged-headed slow-to-be-moved scholars. It may be that much of this is owing to the faithless stipendiaries to whom, in his multifarious employments, he commits the task of the public enlightenment on the usefulness of anatomy; but we can tell him that it is a task, of which it were worthy of all his own best exertions, to attempt the accomplishment. Surely the people of this country, if they were to be judged by the progress which right notions regarding the nature of anatomical pursuits, have made amongst them, must be accounted the most backward and obstinate people in Europe. We know that they have been encouraged and complimented ever and anon for this perverse adherence to old prejudice by certain self-constituted masters of morals among them; but the *opiniâtreté* of the vast majority is too deeply-rooted, and too wilful, to be influenced one way or other by the fulsome few who would pretend to direct it, and at the same time presume to inculcate a spirit of opposition to one of the most

useful of sciences. The British vulgar, in short—including in that denomination some of the very highest as well as the mass of the very lowest persons in the community—are doomed, we fear, to be more slowly convinced of the advantages flowing from the advancement of knowledge in this branch, than in any other. Facts for the last few years have most remarkably evinced the truth of our assertion; and therefore is it that we feel ourselves deliberately bound to repeat that the friends of anatomy have little, if any thing, to hope from improvement effected in that quarter.

The prospect, however, is better when we look to the press. This great engine for working out the destiny of nations, has, we rejoice to think, undergone a thorough amelioration of sentiment with regard to the question of anatomy; and unsanguine as we are respecting the impracticable materials on which it has in the present instance to operate, all our hope is entirely rested upon it. We have seen the day, and we are not so very much older now than we were then, when the leading journals of Great Britain, with but few exceptions, were up in arms on the least mention of a bill for the regulation of the schools. The case is now materially altered. Circumstances, some of them of an appalling character, have occurred during the interval, and have had their due weight, along with time, “the greatest of innovators,” in effecting this most desirable change. The result is certainly promising. There is, to be sure, much warning, much advice, tendered by the press to the advocates of dissection; but there is also much consolation held out—and above all, there is a certain degree of good feeling and earnestness abundantly displayed.

But the tribunal which must determine the question in the last resort is centred in the collective wisdom. What change, if any, has been effected in the removing

of prejudices in this quarter, it is difficult for us to say: but we should hope, that in the event of a simple, straightforward legislative measure being proposed for the consideration of parliament, no serious obstacles, as on a former occasion, may supervene. Our fears, however, must not be dissembled;—it is in the upper house, we apprehend, that all the difficulty of legislating on the subject will lie; and it ought never to be forgotten, that Mr. Warburton’s bill, such as it was, (and any thing, heaven knows, would be better than the present anomalies,) received its finishing *coup* in that house: it became necessary, as our readers will recollect, to withdraw it, in consequence of the determined hostility of a large number of peers, and nearly all the bishops—a curious instance, by the way, of the occasional approximation of the feeling and intelligence of the extremes of society.

As matters stand, most assuredly no respectable body in the community have more to complain of than the teachers of anatomy; and, perhaps, with the exception of their actual grievances, nothing can be more galling to them than the pretended remedies proposed by their ill-judging friends. “We think we know a way,” says one, “to reconcile the safety of the people from murder, with the regulated prosecution of the science of anatomy. Let the schools be *licensed*, and let one condition of the license be, to keep an *exact register* of every subject, describing exactly its age, colour, complexion, &c. by which it may be best identified, as well as the stage of its progress to decay—inserting dates, *even to the hour*. Then the schools must be open to the familiar visitation of two intelligent *inspectors*, who may examine the registers, and the *subjects* too if necessary, and who may be in constant communication with the Home Secretary’s office. This would throw no dif-

feulty in the way of the anatomist (!), while it would completely obviate the burking system. Let us give an illustration of the working of the plan :—A person is missing ; his friends apply at Bow-Street ; the application is forwarded to the visiting commissioners ; they examine the registers, going back to the period from which the missing person was last seen. If he has been *murdered* (what ! though this notable plan was to obviate all murder ?), though it were years before, there stands the clue to evidence against the murderers : the servants of the school can be produced against the murderers, and will furnish more or less satisfactory proof. The existence of this system will be quickly made known to the resurrection-men ; and if, after they know it, they repeat the practices of Burke and Bishop, they must know that they are rushing into certain destruction." Will it be believed that this is seriously put forth by one of the "best possible public instructors," as a certain remedy for the present untoward state of dissecting establishments, and actually wound up to a conclusion without telling how or whence the supply of bodies is to be procured. Yes, true, there are *resurrection-men* mentioned in the scheme. The dealing with these worthies, then, is to be connived at, while an exact account of their proceedings is kept in the register ; the school is to be turned into a sort of *morgue*, and a complete trap for the Burkers ! Excellent legislator ! while not one hint is thrown out of allowing the body-snatchers even a little more latitude in their operations. Surely he thinks that these old hands at every desperate mischief will simply suffer themselves to be caught with chaff, and that they will deal freely with those whom they know to keep a regular "hue and cry" of their proceedings ; and surely he supposes, in his wisdom, that the gentlemen who keep

schools may be thus turned into a useful body of police, and will think themselves liberally dealt with by having the use of the bodies (for which they have paid enormous sums) for their pains. Rank stupidity ! Rather let the doors of the theatres of anatomy be closed for ever, and the *living* dissected "without remorse or dread," than the students of a liberal profession be found mean enough to submit to such a system of organized thief-catching. Away with such *friends* of anatomy !

Yet this man, we have no doubt, means well, or at least not unkindly, to the profession, and is a fair specimen of the well-disposed, who think that, what with the prejudices, and what with the host of difficulties which have to be dealt with, nothing better than this simple "way" can be suggested. We could give an instance of another, who, in his regular morning dose of swaggering nonsense about "human shambles," and the atrocities of anatomists (whom he really seems anxious to identify with the "Burkers"), mixes up such a quantity of ignorance along with what would otherwise seem deliberate insult to the profession, that the beverage is perfectly neutralized and harmless. But he is the only exception, that we know of, to the liberal feeling so generally manifested around him ; and as his only object seems to be to attract notoriety by his obstinate wrongheadedness, we shall leave him in his honourable singularity, and not gratify him by a word more exposure.

All this, however, only serves to set the oppression of the anatomists more conspicuously in view, while the remedy may, perhaps, be as much as ever darkly concealed from sight. Such is not unfrequently the case ; the means that are most obviously within our power are often the most likely to be overlooked ; and this must be our excuse for appa-

rently recommending such simple expedients as every one might seem to have thought of before.

Much will depend on the line of conduct now adopted by anatomists themselves; much on the proceedings of the Council of the College of Surgeons assembled for the special purpose of providing a remedy for the existing evils; much on the tone in which a measure of relief shall be brought before parliament; and every thing else must rest with the temper of the House of Lords. From the people, nothing is to be expected: they will abide, we presume, by the decision of their representatives.

The uncereemonious remedy to which we have already alluded, of appealing to the government, and demanding that all the schools of anatomy be suppressed throughout the kingdom, until they can be revived again upon a proper footing, would be a remedy with a vengeance. Let the friends of anatomy beware how they adopt this method of proceeding. It has too much the air of bravado and intimidation to be productive of the least benefit. But we cannot seriously think that any liberal-minded body of men would be capable of such absurdity. A simpler course lies open to them. All *they* can do is, to make a feeling statement of their grievances, accompanied by a calm expostulation, addressed to the higher powers, and if actually determined to add a sanction to their representations, let them threaten *obedience to the laws*.

As for the College, their line of proceeding is more clearly marked out. Let them pointedly express the difficulties under which the profession labours; let them show what measures are most likely to remove those difficulties; and let them, in fine, gently, yet firmly declare, that *they will examine no candidate for their diploma, until due provision has been made for the schools*.

With regard to the legislative measure most proper to be recommended,

we would be understood to speak with the greatest diffidence. We dread to think that the prejudices against deriving the supply of the schools from the class of the "unclaimed poor" are as deeply-rooted as ever: they would seem to be almost invincible: not open to the artillery of reason, but deeply entrenched in a national peculiarity of thinking. Should this prove to be unalterably the case, the only resource is evidently to be found in the supplies which the jails, penitentiaries, and houses of correction will afford. The sentence of murderers must remain unaltered, and the bodies of suicides must be turned to advantage:—though we fear in this instance also we may have prejudices to deal with, there seems to be a *national* principle engaged in this matter; and *suicide*, without benefitting the interests of science, may probably be claimed as a *privilege* by the better classes in our community.

Above all, let promptitude and energy be displayed in whatever steps are taken. Nearly three years have now elapsed since Mr. Warburton's bill was lost among the Lords. Nothing has since been done. Political engagements, and great national schemes in progress, have been pleaded in excuse for neglecting the interests of anatomy. Can they be longer pleaded, or may the provision of the schools be any longer put off with impunity?

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MEETING OF ANATOMISTS.

SOME evenings ago a meeting of lecturers on anatomy was called together, in order to consider what steps they ought to take, in consequence of the recent events connected with the supply of subjects for dissection. A resolution, having for its object to address a petition to the Privy Council, praying that all the anatomical schools in the kingdom might instantly be closed, was agreed to, with, it is said, only one dis-

sentient voice; and a committee appointed to carry this design into effect. On the occasion alluded to, some of the principal teachers were absent, while others were taken by surprise; and another meeting having been convened two days after, the above rather precipitate resolution was negatived, some of those, on reflection, opposing it who had previously voted in its favour. It was decided that a committee should be appointed to wait upon the Secretary of State for the Home Department, to confer with him as to the measures which ought to be adopted in the present crisis.

ALLEGED DISCOVERY OF AN ANTIDOTE TO CHOLERA.

A WELL-KNOWN physician at Sunderland has written to the government, announcing the discovery on his part of a cure for cholera, and intimating his readiness to make it known on receiving an adequate compensation. Desirous as we are to express ourselves with moderation, we can scarcely restrain our indignation at the bare idea of any man stopping to make his real or supposed discovery a matter of trade and barter at the moment his fellow-creatures are dying around him; and such we have heard is also the sentiment contained in the cutting answer which has been sent to him. If the Doctor has made any discovery, let him follow the example of Jenner, by giving it to the world, and, if it prove successful, the country will not think any sum too large a recompense. But then it must be shewn that it really is a remedy, and, to do this, a very different rate of mortality from that which has hitherto prevailed, must be exhibited. Unless there be some explanation to be offered, such as we cannot at present anticipate, this event will be a crowning to the discreditable proceedings which have already taken place at Sunderland.

DR. JOHNSON AND HIS "CREED."

DR. JAMES JOHNSON, in the last number of the *Lancet*, says, "The editor of a contemporary journal has instituted a series of attacks on me, apparently for changing my creed; though he himself sets out with a declaration that he has entirely changed his own, from anti-contagion to contagion. I have not changed my creed; but *why* he changed his creed, is best known to himself." It would be affectation not to admit that we are the culprits here alluded to, and the significant "*why*" (in italics) would seem to imply that we had better or worse reasons than other people for having supposed, from the history of cholera in India, that it *was not* contagious, and believing, from its history in Europe, that it *is* contagious. If the querist can discover why Dr. Russell, Mr. Orton, and many others, have become contagionists, we shall not require to answer his question; the why and wherefore, as applied to them and to us, being, we presume, precisely the same. The writer goes on—"At present I shall not pursue this subject. I am not in the habit of making attacks on any man; but I will not hold myself bound to suffer the arrows of malevolence to fly about my head without repelling them." The "arrows of malevolence" flying about one's head, is a very pretty figure of speech, but the worthy Doctor is quite mistaken in supposing that such weapons were levelled at him by us; neither were our observations made, as he says, because he "changed his creed," but because he chose to address the public, through the medium of the daily press, in a series of papers, which we thought calculated to do harm. The opinions of any man who does this, become public property, and we have a right to deal with them as such. But why cannot our "ancient ally" have patience?—that same pursuing of the subject to which he alludes, and which is to appear in his next number, ought surely to content him. Though, it seems, our contemporary does not recommend the use of *common salt* in cholera, yet every one knows how largely he deals in *Attic salt* when he himself is choleric. Doubtless, the forthcoming "article" is an exquisite one of its kind, and will produce a most enlivening effect—if he will only let us have it fresh and sparkling, to begin the new year withal,

instead of suffering the spirit to ooze out by degrees through the pages of the *Lancet*.

NATIONAL VACCINE BOARD.

WE understand that this establishment is to be discontinued: if so, we trust that some efficient mode of keeping up the supply of vaccine lymph will be adopted.

WOUNDS FROM ROCKETS.

IT is rather a remarkable fact, that not fewer than four persons were brought to St. George's Hospital, the evening of the coronation, with wounds of the head from the shafts of rockets. In each instance, the wound was on or near the *left* temple, as if the head most naturally turned towards the right shoulder, in looking at an object nearly straight above the spectator. One of these lads lost his life, the others were severely wounded; and we allude to the circumstance at present for this reason, that we have been informed the rockets were procured from Woolwich, with loaded shafts, on purpose to make them go straight up, whereas care ought to be taken (as these instances show) to give them an oblique direction, so as, at least, to clear the ground occupied by the spectators.

INJECTION OF SALTS INTO THE VEINS IN CHOLERA.

WE perceive that a charge of a very absurd description was made against us at the Westminster Society on Saturday evening last by the gentleman who there and then read a paper suggesting a *new* method in the treatment of cholera. Certainly, considering the multitude of remedies proposed and tried in this fatal disease since its first introduction into Europe, to be able to hit on any thing new in that way, is a matter of some merit; but this is not what we have immediately to do with; we are not going to discuss the value, or probable value rather, of a remedy which has not as yet yielded any practical result; our present business is to settle the claim of the self-supposed originator of the method.

In our last number, under the head of "*remedies tried at Sunderland in cholera—others suggested,*" we took occasion to say,

after noticing some other things, "We earnestly recommend a trial of injecting medicated solutions into the veins, particularly some of the neutral salts, as muriate of soda;" and "we think that the views of Dr. Stevens, as to the effect of salts on the blood, to which we have repeatedly called attention, ought to be put to the test as speedily as possible." And this was the foundation of the charge of plagiarism—of course by anticipation—which the gentleman alluded to thought fit to bring against us. But, forsooth, the subject of the gentleman's paper was intimated (however obscurely) on that night week: *ergo*, it was "upon that hint we spake:" we "embezzled," in short, (for this was the polite expression used towards us) our suggestion from Dr. O'Shaughnessy's notice—whence else could we have had it? How superlatively ridiculous! We question if the desire of being thought a discoverer ever betrayed an individual into such a depth of absurdity. We would fain hope that Dr. O'Shaughnessy has thought better of it since, and that he is convinced, ere this, of the precipitate folly of his charge; but lest he should not be so, we shall take leave to set him right on the validity of his claims, and that with an overwhelming mass of evidence, the most incontrovertible.

Let us first, though, settle exactly what this new method or singular discovery is, that is laid claim to. The idea of injecting fluid into the veins in almost any unmanageable disorder now-a-days, is most assuredly no novelty: nobody would be so silly as to take merit to himself for simply proposing injection into the veins in cholera: the merit, then, in the present case, must be supposed to be centered in the proposal: 1, to oxygenate, or at least redden, the blood in cholera by the exhibition of salts; and, 2, to do it by injection. From the nature of the charge made on us, we conjecture that these are the meritorious points in the *new* method which are sought to be appropriated by Dr. O'Shaughnessy—but we shall at once shew how evanescently slender are the claims of this gentleman on both points to be considered as original.

It would be impertinent in us to presume our readers unacquainted with the views of Dr. Stevens relative to the condition of the

blood in malignant diseases—or his method of treatment founded on those views: we shall only beg to state expressly that they were given to the public through a paper read before the College of Physicians in May 1830, and of which an ample analysis was given in this journal at the time*. Since then we can refer our readers to the more ample and masterly communication from the same gentleman recorded in our pages about three months ago†, and the perusal of which drew from Dr. Prout the flattering opinion, that “it contained the germs of discoveries of the last importance to mankind‡.”

But even anterior to this last communication of Dr. Stevens, we had ourselves more than once had occasion to refer to his opinions—particularly in a leader on the Symptoms and Treatment of Cholera§; and again in a leading article, in which we detailed at some length our own impressions relative to the state of the blood, and the effect of saline medicines in malignant diseases||. We will not occupy our space with extracts from those papers, thinking it sufficient to give exact references to them; though we should be extremely glad, did our limits permit us, to insert at length the remarks of Dr. Sewall, of Washington, which will be found appended to the leader last referred to. This gentleman, after stating with admirable precision the results of Dr. Stevens's experiments, adds, “Whatever practical inferences or change in the treatment of diseases these experiments may lead us to, the idea that the red colour of the blood is owing to the saline matter which it contains is entirely new: and no one can deny to Dr. Stevens the merit of having been the first discoverer of this interesting fact¶.”

So much for the principle of the pretended new method. But, perhaps, the gentleman who stands candidate for all the honours it will bring him, rests his claim on the particular manner of applying the saline substance to the blood—by injection. Hear

what a valued correspondent of ours, Mr. Smart, of Cranborne, says, in an excellent paper on transfusion of blood in cholera:—“When I spoke of cholera as a ‘corrupter,’ I mean to allude to the changes which take place in the blood in that disease. This is a fact known to foreign pathologists; it is also known by Clanny and Stevens, that changes, perhaps similar changes, take place in the blood of typhous patients. Hence, there is some reason for assimilating the two diseases, as well as on the additional ground that, after the violence of the attack of cholera is past, fever of a typhoid character is apt to ensue. If the restoration of the healthy properties of the blood be so important a point in the treatment of typhus, as the researches of those gentlemen would affirm, and if this can be effected by the internal administration of carbonic acid gas or the neutral salts, I have long thought, *why should not those substances be directly conveyed into the current of the circulation by injection?*” These remarks are dated the 14th November, and were published in this journal in the very number which came out just before the meeting of the Westminster Society, at which Dr. O'Shaughnessy gave notice of his paper, that is to say, more than a week before the said paper was read.

Here we might stop, and conclude our observations with the question, who now is the *embezzler*?—but we have no wish to part on bad terms with Dr. O'Shaughnessy: we can allow much for the lengths into which the fond ambition of shining forth as a discoverer seldom fails to carry the youthful aspirant. We look upon his present exertication as highly creditable to his talent for chemical research; but most assuredly we cannot flatter him by professing to see *novelty* in it, except there be any in the proposal to use a particular salt (a variety of which will suggest themselves to every chemical investigator engaged in such experimental inquiries)—or if he lay any stress on the particular vein into which he would have the injection made (the external jugular), we can assure him that we think the *merit* of this suggestion completely overbalanced, by the singularly imminent hazard to which it would expose the life of the patient.

* Medical Gazette, vol. vi. p. 217.

† Ibid, vol. viii. p. 740.

‡ Ibid, p. 770.

§ Ibid, vol. viii. p. 404.

|| Ibid, vol. viii. p. 722.

¶ See also the able paper of Mr. Carlyon, of Truro, on the Effects of Nitre on the Blood.—Med. Gaz. vol. viii. p. 626.

WESTMINSTER MEDICAL SOCIETY.

Saturday, Dec. 3, 1831.

Laws relative to Dissection.

On the motion of Dr. Somerville, seconded by Dr. Sigmond, the following resolution was unanimously carried:—

“That the Committee of the Westminster Medical Society be requested to prepare petitions to both houses of parliament to represent the difficulty which attends the study of anatomy from the scarcity of dead bodies, and pray for an alteration of the laws upon that subject.”

Nature and Treatment of Cholera.

DR. O SHAUGHNESSY opened the discussion by reading an elaborate paper on the propriety of injecting oxygenated salts into the veins in cholera, but we regret to say in so low a tone that a considerable part of it did not reach us. It commenced by stating that it was not without much hesitation that he (Dr. O'S.) had on previous occasions ventured to offer contributions to the society; the subjects, however, were of an experimental kind, and scarcely admitted of an argument being adduced against them. In the present instance his observations were strictly experimental, and therefore admitted of demonstration. From all the statements and history of cholera yet presented, it was evident, that notwithstanding the numerous therapeutical agents employed the disease sometimes began with such dreadful violence that medical aid failed in arresting its progress. For the truth of that assertion, sufficient evidence was to be found in the Warsaw epidemic, where the disease proved extremely fatal, notwithstanding that it was treated by a combination of the most devoted men that medicine had perhaps ever furnished to mankind. At Sunderland, the mortality of cases termed “malignant,” was so great that the experience of the past seemed almost valueless as to the future protection afforded against the malady by medicine. Hence it arose, that up to the present moment the most practical men in the profession were assiduously occupied in search of some more specific remedy than any which had been yet discovered. It devolved on practical chemists to inquire whether in the remote causes, in the physiology or pathology of this disease, any data could be discovered which might lead to the application of a remedy. It would be admitted that the chemical nature of the poison was unknown, some terming it animal, others vegetable, some aerial, and others terrestrial. Looking to the consecutive and remote causes, what were the common circumstances observed? What were the phy-

siological changes, if any? What were the chemical changes, if such there were? To answer those questions correctly, they must consider the disease both in its abstract and essential forms, divesting it of all accidental phenomena. What were the first effects of the remote causes? From the evidence of those who had witnessed the disease, it appeared that the effects were particularly observable in the sanguineous system. Hence the skin became blue, the secretions were suspended, the arteries contained black blood; no carbonic acid was evolved in the lungs, the returned air of respiration was as cold as when it entered those organs. Such were the primitive effects of the remote causes, and these gave rise to a third set of phenomena, to which it was unnecessary for him to revert on this occasion. The following questions, then, naturally suggested themselves. First, what was the alteration in the black and thickened condition of the blood, the state of arterialization, the impeded secretions, &c. by which the affection was attended? Secondly, what was the best mode by which that artificial state could be remedied? In considering the first question, they must admit that in all diseases it would of course be a proper procedure to counteract the remote causes, if they were visible, according to a rational mode. Having shewn the improbability of attaining a knowledge of those causes, were they justified in attacking its effects? Such a proceeding was not without a precedent in other cases, and it was generally acknowledged that this was the principle to be adopted in the treatment of the disease now under consideration. They found a great majority of practitioners earnestly recommending venesection, not as a sedative, not as an antiphlogistic, not as a blind empirical specific, but as the means of diminishing the venous congestion, of restoring the suspended circulation, of recovering the arterial quality of the blood, of which a bright scarlet hue was the outward and visible sign. Such was the recommendation of Dr. Johnson, Dr. Kennedy, and others who had reasoned on the phenomena of this complaint. Another class of physicians, embracing the same views, proposed by different means to attain the same desirable object; and in order to remedy the absence of arterialization, they recommended the inhalation of oxygenated gases, or the protoxyde of azote. Now it might rationally be expected that the success or failure of these methods would afford a touch-stone of some authority in deciding on the rationality of the principle on which they were practised. Accordingly, it was established by the most numerous writers that detraction of blood had removed the effect, and cured the disease. But before proceeding further, it was necessary to examine briefly into causes which interfered with the universality of ve-

venesection, and those might be found to reside in the debilitating influence, whether transient or partial, which the detraction of blood sometimes occasioned, and which, when added to the debilitating effects and remote causes of the affection, become sufficient to destroy the vital powers remaining in the system. Again, blood might not be procurable; and when they considered that the venous circulation was almost completely suspended in violent cholera, it could scarcely be a matter of surprise that the detraction of blood by venesection was frequently found to be unattainable. The inhalation of oxygen gas had failed, but that did not shew that the re-arterialization of the blood would not cure the disease. Many causes conspired to prevent the entrance of the gas into the circulation of the lungs. Again, the suspended action of the heart permitted but an insignificant portion of blood to be circulated, and the withdrawal of a certain portion allowed a new stratum to be acted upon by the inspired air. Let them next inquire, whether there did not exist some certain method of inducing arterialization, and that method free from the objections to which venesection and gaseous medication were exposed. It appeared to him (Dr. O'S.), that if they could bring a certain saline substance, highly oxygenated, freely into contact with the blood, they could restore the arterialized colour, and most probably alleviate the bad symptoms of the case. It was, however, obvious, that in a disease of such rapidity, one in which the circulation was almost suspended, and the intestines covered with a tenacious matter, it was in vain to suppose that time, as in the case of the yellow fever, would permit the absorption of materials taken into the alimentary canal. He, therefore, conceived the idea of injecting such substances into the veins as would restore the natural quality of the vital fluid. Before, however, so novel a practice could be put into execution, it was necessary to have a distinct notion of the effects of individual salts upon blood without the body, and when circulating in the living frame. Those which contained the greatest quantity of oxygen were the nitrate and chlorate of potassa. A few grains of those salts would change the colour of a larger quantity of blood than the pulmonic circulation generally contained. They also knew that the nitrate and chlorate of potassa entered the system from the intestines, and passed off in the urine—wherein they might be detected—without injuring the organization of the blood through which they passed. From those facts it was evident, that the injection of the substances he had just mentioned into veins could not possibly do mischief, but might effect much good. In corroboration of the probable efficacy of this mode of treatment, he briefly alluded to some experiments he

had made in prosecuting his toxological inquiries. In one series of experiments, from 10 to 60 grains of chlorate of potassa were injected into the cervical veins of a large dog, which seemed to experience little ill effects. The pulse rose in fulness and frequency; he passed urine copiously in a quarter of an hour, which evidently contained the chlorate. Blood was drawn from the brachial vein, and presented a fine red colour. In another series of experiments, the dog was brought into a state of asphyxia, and when he was apparently dead, half a drachm of chlorate of potassa was injected into the jugular vein. The pulsation of the heart returned, and in twenty minutes he passed urine, containing the injected salt. He (Dr. O'S.) trusted he should not be mistaken as supposing that the practice of injecting into the veins would prove equally successful in cholera; but these experiments seemed to him to prove, that by injecting the nitrate or chlorate of potassa, the blood was restored to its arterial tint, and the process of circulation and arterialization thereby accelerated. In cases of cholera, he would recommend injecting into the jugular vein where it crossed the sterno-mastoideus muscle. The syringe used for the injection should not contain more than two or three ounces; the solution should be injected warm; the tube should not be more than an inch long, and gently curved, in order to assist manipulation. In performing the operation, he would merely make a puncture in the vein, and not detach it from the surrounding parts. The injection should be made deliberately; from 10 to 30 grains of the salt might be employed. It would be desirable, before the experiment was tried minutely, to analyze cholera blood; for, to his mind, no satisfactory analysis had yet been completed. He then briefly recapitulated the advantages which he thought likely to result from the plan he had proposed, and proceeded to remark, that he was unwilling to trespass farther on the time of the society, or he might derive an additional argument from the recent experiments on the blood by Drs. Prout, Christison, and others. He had intended to draw collateral support from Dr. Stevens's experiments, but he understood the doctor was shortly about to publish his work on that subject, and therefore he (Dr. O'S.) had foregone his original design. Before he concluded, he had to call the attention of the society to a recommendation, similar to his own, proposed in the Medical Gazette of that day, and which was nearly similar to the notice he had given at the preceding meeting. If the present were a time for private feeling, he might appeal to the gentleman who presided on the last occasion—to the secretary—to the meeting itself, whether he did not distinctly make

the proposition;—he might appeal to Dr. JONXSON, or he might appeal to the reporter of the Gazette himself; but he would forego all feeling of rivalry, whether the person who availed himself of his (Dr. O'S.) remarks were the Editor of the Medical Gazette or any other—for he thought it unlikely that any member of the profession could be wilfully guilty of *the baseness of thus embezzling* the ideas and suggestions of another—and would merely remark, that he was pleased his opinions had received support from so respectable a quarter. There was, however, this difference between them;—the Medical Gazette recommended the injection of common salt, and he (Dr. O'S.) advised the nitrate or chlorate of potassa. He would venture to remind the editor of the journal in question, that the chloride of sodium, though it reddened venous blood, did not oxygenate it, because it contained no oxygen. There were many substances capable of reddening the blood that would not oxygenate it. He thought so far as theory went, that the salts he had proposed were likely to afford greater therapeutical advantages than those proposed in the Medical Gazette.

Dr. MACLEOD rose and said, that being well acquainted with the contents of the Medical Gazette, he could positively state that the charge of pirating Dr. O'Shaughnessy's opinions was wholly and entirely groundless, inasmuch as every circumstance mentioned, and every suggestion thrown out, had been made in the pages of the Gazette before Dr. O'Shaughnessy's paper had been read, or its announcement taken place. The effect of the neutral salts on the blood had over and over again been pointed out by various writers in the Gazette, as all who read it must be fully aware; and however ingenious Dr. O'Shaughnessy's paper might be, not the least proof of that ingenuity was the manner in which he had contrived to bring forward his ideas with so little allusion to Dr. Stevens, to whom, and to whom alone, was due all the merit that belonged to novelty. He (Dr. M.) stated that Dr. O'Shaughnessy was in error in asserting, that common salt injected into the veins contained *no* oxygen. No one dreamt of injecting dry chloride of sodium; and any one acquainted with chemistry knew, that when thrown into water, the sodium became united with oxygen to form soda. He did not mean that the oxygen was likely to be separated by the blood, because he suspected that the change of colour was dependent on quite a different cause: he merely wished to answer Dr. O'Shaughnessy's remark on that particular point. The very identical proposal of injecting nitrate of potass into the veins, now adduced by Dr. O'Shaughnessy, had been made a fortnight ago by a writer in the Medical Gazette, and he had

himself been engaged in some experiments upon the subject. [Dr. Macleod then detailed these, but as he has described them in a letter, inserted at page 357, we omit this part of his speech.]

Dr. JOHNSON here made some remarks, which we deem it unnecessary to insert, as they are embodied in his letter, at page 357.

Dr. MACLEOD said, he was extremely mortified to hear such a charge against Dr. Stevens; but that he had nothing to do with his accuracy or inaccuracy; he merely maintained, that whatever merit there was, be it great or small, in the proposal of using the neutral salts to change the state of the blood, was exclusively his.

Dr. BLICK then rose and said, that it appeared to him that if they were to come to any thing like a legitimate conclusion on the propositions of Dr. Johnson, they ought to be taken *seriatim*. [This Dr. Blick accordingly proceeded to do, amid some confusion, and speaking in a low tone, was imperfectly heard. He argued in favour of the identity of the Indian and English cholera.]

Dr. SIGMOND now claimed the attention of the Society, while he adverted to the identity of the disease. It was supposed that the cholera, mentioned by Sydenham, more or less resembled the Asiatic disease; but he agreed with Dr. Copland that the diseases differed in every point of view. Sydenham was in the habit of sitting down by the bedside of the patient, and correctly noting every symptom. Small-pox and gout had been described by him in the most accurate manner, and of cholera he had given six particular symptoms. Sydenham, however, in his six symptoms, spoke of cholera, dysentery, and griping, as being very common in a certain year. Dr. Sigmond then produced several ancient bills of mortality, published during the years in which Sydenham wrote his account, in which an immense number of persons were described as having died of "griping of the guts," and proceeded to remark that Sydenham made use of the word cholera when speaking of bile. The list of the six symptoms of which Sydenham spoke, was that of collapse, which he described as frightening the by-standers, and carrying the patient off in 24 hours. He (Sydenham), however, preceded that statement by remarking, that the disease seldom or never occurred except during the months of September and October, and that it was not to be mistaken for a surfeit. He did not speak of the cold breath, nor of the blue appearance of the body, two circumstances which it was impossible to believe could have escaped the notice of so accurate an observer, had they occurred in any patient under his care. The disease described by Sydenham was totally different from the Asiatic cholera. There was not one of the old writers who described the blue symp-

toms and the cold breath. They described the gripings, twistings, and convulsions, arising from a flux of the bowels, but not that extraordinary intestinal state which occurred in this disease. With respect to the observations of Dr. Johnson, as to the affections arising from some peculiar changes in the earth, it was a remarkable coincidence that plague, when first described, was considered to proceed from the same circumstance. Earthquakes, comets, &c. took place prior to the occurrence of both these malignant affections; but he (Dr. S.) could not trace any connexion between the supposed cause and effect; and it appeared to him that the cholera arose from a poison immediately affecting the brain. The Indian medical men, though of high talent and great research, found a disease with which they were unacquainted before: they did not know what name to give it, and therefore gave it one with which they were best acquainted, as producing most dreadful symptoms, namely, cholera. They were the first persons to name it, and did so from mere comparison.

Mr. BOYLE would not have trespassed upon the patience of the meeting, but for the conflicting opinions which prevailed as to the actual symptoms of the disorder under discussion. He had seen cholera as it existed in various parts of the world, and the distinctions between Indian and common cholera, he believed, were not to be found in the state of collapse, in the dark areolæ around the eyes, or the severity of the spasms; but in epidemic cholera there were spasms producing a wrinkled state of the fingers, there was coldness of the tongue, cold breath, and the absence of bile. The wrinkling of the fingers deserved particular attention, for he had never seen a case terminate fatally, unless that symptom were present. With regard to Dr. O'Shaughnessy's paper, he, as a practical man, must say that he did not think it was applicable to this subject. Dr. O'Shaughnessy seemed to think that if he could change the appearance of the blood, he would cure his patient; but it must be recollected that there were other symptoms present besides the want of oxygenation of the blood, and remedying that defect would afford no relief to the other symptoms. Mr. Boyle then referred to the opinions he expressed on the last evening of discussion, as to the pathology of the affection consisting essentially in obstruction of the gall-ducts, from which he again deduced an argument in favour of emetics, which, he said, was materially strengthened by the plan adopted by Dr. Barry, of administering salt and water, which must act on the same principle as emetics.

Mr. KING rose in consequence of the opinion expressed by Mr. Boyle, as to the obstruction of the biliary ducts. He had opened nearly a *thousand* bodies! and in

about one-half he had been unable to make the bile pass from the gall-bladder into the duodenum. After some farther rather desultory observations, the speaker went on to give an account of a case of cholera, which he had attended in conjunction with Mr. Lobb, of Aldersgate-street. "The old woman" was so severely attacked by the disease that he (Mr. King) thought recovery impossible; but to his great astonishment, in consequence of the application of a mustard poultice by himself (adopting Dr. Blick's suggestion, or rather anticipating it), about eighteen inches square—[Some one in the gallery at this moment exclaimed, Hear, hear, hear! in the loud and solemn manner in which Mr. King is wont to do so—the imitation was perfect, and produced long and loud bursts of laughter, by which the speaker was for some time interrupted]—I say, eighteen inches square over the whole abdomen: the patient speedily rallied. The laughter which accompanied the recital of this case induced the speaker, at the conclusion of his narrative, to vociferate, that if the audience did not believe him, they might make inquiries at No. 92, in Fleet-street, where the patient herself would corroborate all his statements. He then concluded by deprecating the practice of injecting the jugular vein, as proposed by Dr. O'Shaughnessy, on account of the danger of exciting phlebitis.

Dr. WEBSTER hoped that at the next meeting he should have in his power to lay before the Society a communication from Nottingham, relative to the prevalence of cholera in that town about ten or twelve years ago, at which period the mortality was from twelve to fifteen persons daily. He had also heard that the disease once prevailed to a fearful extent at Rochester. He concluded by repeating some of his former observations relative to the identity of cholera.

Mr. ——— begged to correct two slight errors; the one committed by Dr. Macleod, and the other by Dr. Sigmond. Dr. Macleod had spoken of Dr. Stevens as having been the first person who introduced a knowledge of the deranged state of the fluids, in opposition to the French doctrines of solidism. That honour was due to Dr. Clanny, of Sunderland. Dr. Sigmond had spoken of Servetus, as having first discovered the circulation of blood; that great physiologist merely discovered the pulmonary circulation.

Dr. O'SHAUGHNESSY replied very briefly, that he had little to say which would not involve a repetition of the arguments read in his paper. He objected to Dr. Macleod's experiments, as by that gentleman's own showing he had *burked* the rabbits, and it was not to be expected that an animal actually dead could be resuscitated.

The Society was then adjourned. The debate is to be introduced this evening by Mr. Searle.

MEDICO-BOTANICAL SOCIETY.

Twelfth Session.

This society commenced its sittings last month, and reports of two meetings have been lying on our table, but cholera has so fearfully overspread our pages, and travels still with such alarming rapidity up one column and down another, that we can even now afford but little space for noticing two curiously interesting papers which were read to the Fellows at the two November meetings.

The first, communicated by Dr. Walsh, (accompanied with specimens, not only of the plant itself, but also of the alluvial soil on which it grows, the subjacent rock, &c. &c.), related to the "*Fungus Militensis*" of the isle of Gogo, which was once so highly valued by the Knights of Malta that a constant guard was kept for its especial preservation.

The second essay, by M. Bonastre, of Paris, and translated by the Secretary, contained a particularly interesting detail of certain vegetables found growing in the tombs of Egypt, of a totally different kind to those which form the present circumjacent flora. We only regret our inability to do more than announce the subject of this paper, which excited, and most deservedly, very general attention.

The Professor of Botany likewise offered some important observations on the laws of "Isomorphism," or "Homomorphism," in vegetables, of which we shall probably give a further account on some future occasion. The Society, at its rise, adjourned to Dec. 13th, when it was announced that a lecture would be delivered by the Professor, "On the Connexion of Medical with General Botany."

MEDICAL SOCIETY OF LONDON.

On Monday, November 28th, a paper on cholera, comprising an account of an epidemic English cholera, which appeared amongst his Majesty's ships in ordinary, in the river Medway, in the months of July, August, and September of the present year, was read to the Society, written by Mr. James Hall, the surgeon of the ships in ordinary at Sheerness, a corresponding member of the society. The subject of cholera was resumed on Monday last, and will be again discussed on Monday next.

REPORTS OF CASES OCCURRING AT PUBLIC INSTITUTIONS.

ST. BARTHOLOMEW'S HOSPITAL.

Ligature of the Common Carotid—Excision of Superior Maxilla.

WE subjoin an account of a very interesting operation performed by Mr. Earle last Saturday. Our report may appear rather tame after the highly-coloured one which appeared

in the newspapers, but we have reason to believe that it is very accurately drawn up. The operation, though not common, we need scarcely say is not new; and we ourselves had occasion to record its performance at the London Hospital, by Mr. John Scott, less than a year ago*. Mr. Scott tied the external carotid; Mr. Earle preferred taking up the common trunk. The forceps used in both was Liston's, made very strong, and slightly modified; indeed, we believe the identical instrument was used in both. It is but justice towards Mr. Earle to remark, that the operation was performed with the greatest dexterity and coolness; but we strongly disapprove of the manifestation of feeling to which it subsequently gave rise in the operating room. They who make a successful operation the subject of testifying approbation, will also hold themselves entitled to visit an unsuccessful, though perhaps not less skilful performance, with marks of their displeasure; and it would be difficult to imagine any thing more calculated to injure the cause of operative surgery than such displays. We are informed that Mr. Earle, with the candour which those who know him would expect, spoke in the handsomest manner of the assistance afforded him by Mr. Scott. Our reporter states that, at the commencement of his clinical lecture on Wednesday last, Mr. Earle, after reporting the progress of the case, took the opportunity of publicly expressing his thanks to Mr. Scott, for the readiness with which he had come forward, and for the many valuable hints and able assistance which he had rendered him. "It is thus," he concluded, "that men attached to public institutions ought always to act. We should render every assistance to one another, and be rivals only in doing good."

Mary Cave, æt. 45, was admitted into the hospital under the care of Mr. Earle, November 20th. She had always enjoyed good health until about two years ago, when, after a blow, which she received on the right cheek, she perceived a slight swelling beneath the infra-orbital ridge, attended with pain, which she compared to a face ache from cold. This swelling continued to increase, though in no very marked degree, until last August, when the tumor began to extend itself through the front of the superior maxillary bone and alveolar processes into the mouth, she having lost the teeth of that side some considerable time previously. This induced the patient to apply to a surgeon at St. Albans, who lanced it, and afforded her some relief; but from that time the disease increased rapidly, causing a greater projection of the cheek, and nearly filling the mouth. Nothing was done to it from the month of August up to the present time. By reference to the sketch, it will be seen that

* See Medical Gazette, vol. vii. p. 236.



the diseased mass projects some distance from the mouth, which it nearly fills: that portion which is seen in the left side of the mouth is moveable, and having no connexion with the left maxillary bone. The tongue was pressed upon and much confined by the tumor, causing considerable difficulty of deglutition. The offensive fœtor was very distressing. No enlargement of the cervical glands, or any apparent disease in other parts of the body.

The operation was performed on Saturday, the 3d inst. in the following manner.

As a preliminary step, Mr. Earle secured the common carotid artery, which was done with great facility. A strong flat silk ligature was passed round the vessel, and tied with a slip knot, so gently as not to divide the inner coats of the artery; a small portion of cork being interposed, to act as a tourniquet and facilitate the removal of the ligature at the conclusion of the operation, should it be found that any ill consequences to the brain resulted from its application.

The patient being supported with pillows, the first incision was made, extending from about a quarter of an inch from the inner angle of the eye, downwards by the side of ala nasi, to about the same distance from the commissure of the lips. A second incision extended from the mouth through the cheek, beyond the anterior edge of the masseter muscle, below the course of the parotid ducts. This triangular flap was then dissected from the front of the tumor upwards, as high as the orbit. The parts contained in the latter were next separated, by means of a curved

silver scalpel, from the floor of the cavity; the interior oblique muscle of the eye being detached at its origin, and the infra orbital nerve divided before it entered the canal. The front of the tumor and adjacent bones being thus exposed, Mr. Earle, with a large pair of Liston's forceps, divided the malar bone close to its junction with the superior maxillary bone. The forceps entered the sphenomaxillary fissure, and cracked the orbital plate of the superior maxillary bone transversely across, an inch posterior to the infra orbital ridge. The forceps were then applied to the nasal process of the superior maxillary bone, which was divided at about the middle of the groove of the nasal duct. The first incisor tooth on the right side was next drawn, and the superior maxillary bone cut through close to the symphysis. The tumor, and bones to which it was connected, were now almost isolated, and only retained their posterior attachments. During the removal of that part of the tumor connected with the bones, it was separated from the other part contained in and projecting from the mouth; this latter portion required to be dissected from the back part of the cheek and tonsil. The removal of this diseased mass being completed, a large clasm was formed, at the bottom of which were seen the fauces and cavity of the nose.

No hæmorrhage took place during the operation. The ligature was then loosened, and the cork removed, when some slight bleeding was apparent from the internal maxillary artery. As the patient was rather faint, it was apprehended that the hæ-

morrhage might increase; and as no bad effects had resulted from the temporary compression, a common silk ligature was connected with the flat one, which was withdrawn, and the artery finally secured in the usual manner.

The edges of the wound were then accurately brought together with hare-lip pins and sutures; and the patient was put to bed, and forty minims of laudanum administered in a little wine.

Towards the evening the pulse rose considerably, and fourteen ounces of blood were taken from the arm. The patient has not had one bad symptom since. The whole extent of the wound is nearly united by the first intention, with the exception of part of the upper lip. The pins have been removed, and some of the sutures.

On examination of the tumor, all the parietes of the antrum, excepting the orbital plate, were found to be absorbed. A thin plate of newly-formed bone inclosed the upper part of the tumor, which was of a firm sarcomatous structure, with spiculae of bone intermixed. Similar plates of bone were irregularly dispersed through the tumor. The lower portion, projecting into the mouth, was of a softer texture, and approached in character to the medullary sarcoma.

December 9th.—The patient is going on as well as possible.

OUR EXTRA SHEETS.

WE think it right towards our readers to inform them, that it is not intended to increase the size or raise the price of this journal; and at the same time to explain the circumstances which have led us, on several recent occasions, to give an additional sheet. The number of papers sent to the Gazette during some weeks past, has been so great, that it would not have been possible to have inserted them all, even if the whole journal had been devoted to that purpose alone. We have, therefore, thought it better to give occasionally an extra sheet, rather than either disappoint—perhaps offend—many valuable contributors, or sacrifice the other departments of our paper. Many of the communications relate to Cholera—a subject which, after a time, must become exhausted, so that we do not contemplate being frequently obliged to trespass upon our subscribers. We have still to apologize to several gentlemen for the postponement of their papers, for which we have been unable to make room this week, notwithstanding the omission of Dr. Thomson's Clinical observations; it will be seen, however, that besides the Lectures on Medicine and on Surgery, we have inserted, in the present number, not fewer than thirteen Original Communications, all bearing the names of the contributors—a circumstance by which we have been partly guided in the selection made.

HOT AIR BATHS.

It is but justice towards Mr. Green to state, that his baths are not made by him for sale, as the notice in our last number might seem to imply. Mr. Green has, at much expense, constructed very efficient baths at his own house, where they are open to the inspection of the profession.

METEOROLOGICAL JOURNAL

Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.

December.	THERMOMETER.	BAROMETER.
1	from 33 to 48	from 30.09 to 30.02
2	40 48	29.96 29.99
3	39 48	30.05 30.02
4	39 51	29.96 29.86
5	40 50	29.80 29.56
6	39 51	29.38 29.42
7	43 53	29.90 29.99
8	43 55	29.15 29.22

Wind N.W. and S.W. the latter prevailing.
Except the 6th, cloudy; rain at times.
Rain fallen, .325 of an inch.

NOTICES.

The communications of Mr. Huntley, Dr. Tweedale, Dr. Negri, Mr. Bennett, Dr. Badeley, a General Practitioner, Mr. Crowdy, Mr. Alsop, Dr. Burke, Dr. Holbrook, J. F., and F. A. B., have been received.

Dr. Blackmore and Dr. Tweedale in our next.

Dr. Gilchrist.—We are sorry that our reporter should have mistaken the doctor; but having inserted, in our last number, his denial of being influenced by personal feeling, we must decline doing anything farther in the matter.

Medicus, M. D., and X. Y., are respectfully informed, that GALVANISM having already been repeatedly suggested, we do not think it necessary to insert their letters.

"A Chemist and Druggist," who writes in answer to Mr. Greening, is too personal. We have no objection to insert the fullest vindication of his "order;" but, as he does not give his name, he must speak of events, not individuals.

"X."—We shall inquire about it, and perhaps be able to give the information desired next week.

"Physicus" is under consideration.

ERRATUM.

Some accident occurred in the arrangement of the types in passing from the second to the third sheet of our last number; we shall set it to rights on resuming our analysis of Dr. Bright's work, in which the mistake took place.

On the 1st of January, 1832, will be published, Part I. price 5s.

OF THE

CYCLOPÆDIA

OF

PRACTICAL MEDICINE;

COMPRISING

TREATISES ON THE NATURE AND TREATMENT OF DISEASES,
MATERIA MEDICA AND THERAPEUTICS,
MEDICAL JURISPRUDENCE, &c.

EDITED BY

J. FORBES, M.D. F.R.S.	ALEX. TWEEDIE, M.D.	J. CONOLLY, M.D.
Physician to the Chichester Infirmary, &c.	Physician to the London Fever Hospital, &c.	late Professor of Medicine in the London University, &c.

PART I. CONTAINS:

Abdomen, Exploration of <i>By Dr. Forbes.</i>	Alteratives <i>By Dr. Conolly.</i>
Abortion <i>Dr. Lee.</i>	Amaurosis <i>Dr. Jacob.</i>
Abscess <i>Dr. Tweedie.</i>	Amenorrhœa <i>Dr. Locock.</i>
Abstinence <i>Dr. M. Hall.</i>	Anæmia <i>Dr. M. Hall.</i>
Achor <i>Dr. Todd.</i>	Anasarca <i>Dr. Darwall.</i>
Acne <i>Dr. Todd.</i>	Angina Pectoris <i>Dr. Forbes.</i>
Acupuncture <i>Dr. Elliotson.</i>	Anodynes <i>Dr. Whiting.</i>
Age <i>Dr. Roget.</i>	Anthelmintics <i>Dr. Thomson.</i>
Air, Change of <i>Dr. Clark.</i>	Antiphlogistic Regimen <i>Dr. Barlow.</i>
Alopecia <i>Dr. Todd.</i>	Antispasmodics <i>Dr. Thomson.</i>
	Aorta, Aneurism of .. <i>Dr. Hope.</i>

LONDON:

SHERWOOD, GILBERT, & PIPER, AND BALDWIN & CRADOCK,
PATERNOSTER-ROW;

WHITTAKER, TREACHER, & CO. AVE-MARIA-LANE.

The want of a comprehensive work on subjects connected with PRACTICAL MEDICINE, including PATHOLOGY and PATHOLOGICAL ANATOMY, is one which has long existed in this country. The Medical Dictionaries heretofore published, and the Systems of Medicine in the hands of the student, may be said, without invidiousness, to fall very far short of presenting the English reader with such a compendious survey of the actual state of BRITISH and FOREIGN MEDICINE as is absolutely required by him. Some of them are too limited and too superficial in their character; others are too voluminous, too intricate in their arrangement, and too indiscriminate in their contents; and all are open to the serious objection of failing to represent the improvements and discoveries by which the scientific labours of the members of the medical profession, in various parts of the world, have been rewarded since the commencement of the present century.

It is the object of the CYCLOPÆDIA OF PRACTICAL MEDICINE to supply these deficiencies, and to meet the acknowledged wants of the medical reader. Such ample arrangements have been made for effecting these important objects, as enable the Editors to lay before the public the nature and plan of a publication in which they have endeavoured, by dividing the labour of a work including subjects of great diversity, and all of practical importance; by combining the valuable exertions of several contributors already known to the medical public; by excluding mere technical and verbal explanations, and all superfluous matter; and by avoiding multiplied and injudicious divisions; to furnish a book which will be comprehensive without diffuseness, and contain an account of whatever appertains to practical medicine, unembarrassed by disquisitions on subjects extraneous to it.

In pursuance of this design, every thing connected with what is commonly called the PRACTICE OF PHYSIC will be fully and clearly explained. The subject of PATHOLOGY will occupy particular attention, and ample information will be given with relation to PATHOLOGICAL ANATOMY.

Although the excellent works already published on the subjects of MATERIA MEDICA and MEDICAL JURISPRUDENCE can be so readily and advantageously consulted, as to make the details of those branches of science uncalled for in the Cyclopædia, it belongs to the proposed plan to comprise such notices of the application and use of medicinal substances as may be conveyed in a general account of each class into which they have been divided, as of TONICS, NARCOTICS, &c.; and to impart, under a few heads, as TOXICOLOGY, SUSPENDED ANIMATION, &c. such information connected with Medical Jurisprudence as is more strictly practical in its character.

It is almost unnecessary to say that a work of this description will form a LIBRARY OF PRACTICAL MEDICINE, and constitute a most desirable book of reference for the GENERAL PRACTITIONER, whose numerous avocations, and whose want of access to books, afford him little time and opportunity for the perusal of many original works, and who is often unable to obtain the precise information which he requires at the exact time when he is in greatest need of it.

The STUDENT OF MEDICINE, who is attending lectures, will also, by means of this work, be enabled, whatever order the lecturer may follow, to refer, without difficulty, to each subject treated of in the lectures of his teacher; and it is presumed that Lecturers on Medicine will see the advantage

of recommending to their pupils a work of highly respectable character, the composition of original writers, and which, it is hoped, will neither disappoint the advanced student by its brevity and incompleteness, nor perplex those commencing their studies by an artificial arrangement.

But, whilst the Editors have felt it to be their duty to prepare a safe and useful book of reference and text-book, it would be doing injustice to those by whose co-operation they have been honoured, not to avow that they have also been ambitious to render the work acceptable and interesting to readers who have leisure and inclination to study what may be termed the *PHILOSOPHY OF MEDICINE*: whatever is truly philosophical in medicine being also useful, although the application of the science to the art requires much reflection and sound judgment.—For the assistance of those who desire to pursue a regular course of medical reading, ample directions will be given when the work is completed; and for those who may be anxious to prosecute any particular subject to a greater extent than the limits of the *Cyclopædia* permit, a list will be given, in an Appendix, of the best works relating to each.

The means of accomplishing an undertaking of the importance of which the Editors are fully sensible, will, doubtless, be appreciated after an inspection of the list of contributors who have already promised their co-operation. It is, of course, desirable that a work of this kind should be characterised by unity of design, but, at the same time, as each author will, generally speaking, contribute his knowledge and his opinions on the subjects which have occupied his chief attention, the superiority of the whole performance to any thing which the mere labour of compilation could accomplish will be unquestionable. To each important article the name of the author will be appended.

The acknowledged want of such a publication, already alluded to, and the extensive encouragement which Dictionaries of much greater extent have met with in *FRANCE* and *GERMANY*, although some of them are very unequal as regards the value of different parts, and encumbered with much that is absolutely useless, afford sufficient reason to hope for the success of a work in which what is valuable will, as much as possible, be separated from what is merely calculated to distract the attention, and to frustrate the inquiry, of those who study the science of medicine with a view of regulating and improving its practice.

In order to insure this success, it is the desire, and will be the endeavour of the Editors to make the *CYCLOPÆDIA OF PRACTICAL MEDICINE* not only obviously useful to those for whom it is more immediately intended, but so creditable to *BRITISH MEDICAL SCIENCE* as to deserve and to obtain the patronage of all classes of the Medical Profession.

CONDITIONS.

I. The Work will be published in Parts, and printed on superfine Paper, in a small Type, with double Columns, and in the largest Royal Octavo Size.

II. The first Part will be published on the 1st of January, 1832; and the Work will be continued on the first Day of every Month, until the whole is completed. Each Part (price 5s.) will contain as much Matter as is found in an equal Number of Pages of the large Quarto Encyclopædias.

III. The Arrangements made by the Publishers are of such a Kind as to enable them to engage for the regular Appearance of the Work, in London, Edinburgh, and Dublin, on the First Day of every Month.

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THE LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, DECEMBER 17, 1831.

LECTURES

ON

THE THEORY AND PRACTICE OF
MEDICINE ;

Delivered at the London University,

By DR. ELLIOTSON.

PART I.—LECTURE XI.

*Varieties of Inflammation, according to its Seat
and other circumstances.*

I HAVE mentioned, gentlemen, the general symptoms, the general course, and the general consequences of inflammation, and I have now to speak of the varieties which take place in all these three particulars from certain circumstances.

Varieties according to the Structure affected.

In the first place I shall direct your attention to those varieties which are occasioned by the particular structure of the part in which the inflammation chancas to occur. The consideration of the varieties induced in inflammation by the structure affected, was first thought of, I believe, by Dr. Carmichael Smith, who published in the second volume of the Medical Communications, printed in 1788, a very admirable and classical paper on this subject. He it is, I think, who really has the merit of first considering diseases according to the structure which they affect; he, however, considered only inflammation. Nearly ten years after this, Pinel, a celebrated French physician, adopted this same arrangement, and he, I believe, upon the continent generally has the credit, while our countryman is overlooked. Bichat, in his General Anatomy, afterwards pointed out that all diseases, indeed, might be considered according to the structure which they affect.

There are five chief structures which Dr. Carmichael Smith considered as giving rise to the most decided varieties in the symptoms of inflammation, and these are the skin,

the mucous membranes, the serous membranes, the cellular membrane and substance of the organs; and, according to him, the muscles, but we ought rather, perhaps, to say the fibrous membranes, such as ligaments and aponeuroses, tendons, and the periosteum.

Skin.

When the part inflamed is the skin, it exhibits an intense shining redness, which is instantly removed by pressure of the finger, and as instantly returns, or nearly so, when the finger is removed; so that you make the skin white, but almost as soon as you remove the finger, you find a blush pervading the part, and then it becomes as red as before. If this inflammation of the skin be of a simple character, and in a patch, if there be nothing more than the inflammation of the part, the term given to it is *erythema*. It is not every inflammation which is erythema, but the simplest patch of inflammation on the skin is so designated.

The character of the pain when the skin is inflamed is burning and smarting, and when the inflammation subsides, it is frequently followed by the separation of the cuticle in branny particles, or scales, or even large shreds. This process is called *desquamation*.

Very frequently when the skin is inflamed we have vesicles appear, of various sizes; and not unfrequently there is a very great effusion of serum into the subjacent cellular membrane; so that where the skin is inflamed, the whole part becomes swollen, puffy, œdematous. As the skin is a continuous surface, every inflammation of it which is entirely superficial has a great tendency to spread, and sometimes when that takes place the part first affected recovers itself, so that the disease appears to wander along the body like a cloud, forsaking one part and going to another. But on the other hand, you sometimes have the part originally inflamed still affected, the disease having merely extend itself still farther. Sometimes the inflammation does not spread in this

way, but dips down, so that the cellular membrane below not merely becomes filled with serum, but becomes the subject of actual inflammation. If this occur in mere points matter may be formed, and you have what are called *pustules*, a number of little minute collections of pus. Sometimes they may be formed, indeed, without the cellular membrane being affected, the surface of the skin alone being attacked; but frequently the pustules exist also through the skin, deep in the cellular membrane. Sometimes, however, frightful extensive inflammation of the cellular membrane takes place, suppuration being very likely to occur, and then the disease is designated *erysipelas phlegmonoides*, or as some choose to call it, *diffused inflammation of the cellular membrane*. You therefore see that the skin may be inflamed superficially, and remain so, or the inflammation may dip down into the cellular membrane below, and then you have all the characters of inflammation of the cellular membrane in addition to those of inflammation of the skin. This may be considered a complicated disease, at least an inflammation of two parts.

Mucous Membranes.

The second structure which gives variety to inflammation is the mucous membranes. Mucous membranes, you know, are very nearly allied in structure, and in all other qualities, to the skin. You will find in Bichat's beautiful Treatise on Membranes the analogy between mucous membranes and the skin pointed out. It is a work well worth your study. When a mucous membrane is inflamed, it becomes thicker than usual, its temperature increases, and it is tender to the touch. The pain which is experienced is dull and diffuse, and sometimes it is of a pricking character, but if not pricking, it is obtuse and diffused. When the inflammation first begins, the secretion of the part is increased; it is, however, watery, or thin, and perhaps acrid. This any one may observe in a common catarrh. When a cold begins in the Schneiderian membrane of a nostril, the nose runs, the secretion is increased, but the secretion at first is not very excessive, nor is it thick but thin; frequently, however, it is more or less acrid. As the disease advances, the discharge becomes, perhaps, entirely suppressed, no secretion, perhaps, takes place, and then, as the disease declines, the secretion increases again, becomes excessively copious, exceedingly viscid, perhaps offensive, and then again it gradually lessens. If the inflammation be not intense, the secretion remains mucous; although it may be altered in its quality, may be diseased, still it is mucous. If, however, the inflammation be more intense than that, the secretion becomes puriform, and a still higher degree of intensity will give rise to absolute pus—purulent discharge. All this you see in inflam-

mation of the urethra, where inflammation is far more violent than that which occurs in the nose. The inflammation of catarrh generally produces nothing more than mucus; but the inflammation of severe gonorrhœa is so violent as to cause the formation of pus. If, however, the inflammation be still more violent than that which occurs in purulent gonorrhœa, you may not have simply pus produced, but fibrin will be poured out. One of the most violent inflammations of a mucous membrane is the crop of children; and in that, the mucous membrane pours forth fibrin, so that the parts become obstructed, and a tube of new secretion is produced. I have seen the same occur in the urethra, where an individual, in consequence of a violent gonorrhœa, has used strong injections, the result of which was the excitement of a dreadful inflammation. Pus had been secreted before, but now fibrin; and a portion of fibrin was discharged from the urethra, exactly resembling an earth-worm in appearance, except in colour.

Occasionally, when mucous membranes are inflamed, hæmorrhage takes place from them; they become so overloaded with blood that the vessels allow its escape, and a hæmorrhage ensues. This is more particularly the case with inflammation of the mucous membrane of the intestines and of the urethra. These membranes more frequently discharge blood when they are inflamed, I think, than any other mucous membranes. Occasionally (but this is a more rare circumstance than the former) inflammation of the mucous membranes induces mortification, or sloughing; this is particularly observed in the throat. Now and then you have ulceration, and this generally occurs in the throat and intestines; it often takes place from the very beginning, and is not at all in proportion to the degree of inflammation. There often appears to be a strong disposition to ulceration. Inflammation of a mucous membrane frequently degenerates into a state of chronic discharge, so that when the inflammation is over, the part continues to pour forth a quantity of fluid, without pain and without heat. This particularly occurs when a mucous membrane has been inflamed several times; and we especially notice this gleet, as it is termed, from the bronchiæ and the urethra. The latter is every day seen in young men, and the former in those who are old. Hence we have *catarrhus senilis*—a gleet, a constant discharge from the mucous membrane of the air-passages, without any inflammation.

If we examine these membranes after death, when they have been inflamed, we find them thicker and softer than natural. If the inflammation have continued very long, they will frequently become very soft and very thin; but, on the other hand, just the reverse is frequently observed. If in-

flammation have induced softening, you may rub them off from the cellular membrane beneath, with the finger. Sometimes, however, these mucous membranes become indurated. It is no proof, if you find a mucous membrane soft, as you frequently do in the case of the stomach—it is no proof that it has been inflamed. I believe that a part will occasionally become softened without any inflammation; at least I know that the softening is frequently out of proportion to any inflammation that existed. Generally, when a mucous membrane has suffered acute inflammation, it is found to be thicker and softer than natural; it is much more rare to find it hardened. The usual effect of acute inflammation is to soften parts; but if, by chance, it should occasion a deposition of fibrin, though it has itself a tendency to induce softening, yet the lymph so poured forth being hard, will cause an appearance of induration. Baron Dupuytren, the eminent French surgeon, and Dr. Thomson, of Edinburgh, found inflamed arteries softened; and the stomach, the bronchiæ, and the heart, when inflamed, are continually found soft. This is the more general effect, therefore, of acute inflammation of mucous membranes.

Sometimes, when a mucous membrane has been severely inflamed, you will find it almost perfectly black. Through the mere congestion of blood you will find it look dark, as if it were mortified; but, on handling it, you find it not lacerable, like a mortified part—you find it still pretty firm and solid generally, and devoid of fætor. Great mistakes have been continually made in the inspection of bodies. After inflammation of the intestines, you often find portions of them almost as black as your hat, without any particle of mortification; and frequently such cases have been pronounced at once mortification of the intestines. Mere congestion of blood will make a mucous membrane almost black, as is seen in the case of the genital organs of rabbits during heat. You will find the error to which I have alluded pointed out by Dr. Baillie in his *Morbid Anatomy*.

It is a general remark that the feverishness, the pyrexia attending the inflammation of a mucous membrane, is comparatively mild. The pulse is generally soft, although it is quickened; and the more frequent the attacks of inflammation in any individual mucous membrane, the more mild they are, and the more early and profuse is the discharge.

Serous Membranes.

If the part inflamed be a serous membrane, such as the arachnoid, the pleura, the pericardium, the peritoneum, or the tunica vaginalis, the pain is for the most part acute, darting, lancinating. The pulse is not soft, as in the case of inflammation of a mucous membrane, but is generally hard, though by

no means always. It is a general but not universal remark, that the pulse is hard. The inflammation causes here, not a secretion of mucus, but a copious effusion of serum, which is sometimes limpid, clear, of a lemon colour; but generally it is turbid, except, perhaps, in inflammation of the head. The serous collection is generally turbid and yellowish, like whey; and sometimes it is puriform. The fluid, in many portions, resembles pus, although other portions of it may be more like serum. Sometimes, however, absolute pus is produced; so that the pericardium has been seen filled with pus. Not only is serum effused, but fibrin is likewise poured forth, the appearance of which is very various. Sometimes it forms a jelly-like yellowish mass; sometimes minute shreds or flakes are seen swimming about the fluid; and sometimes it forms regular layers, looking like so much skin. These deposits of fibrin will at length grow vascular, and when vascular they will adhere to the surrounding serous membrane, and at last become thin again, and in all respects resemble cellular membrane; and hence they have been frequently called *false membranes*. A false membrane is nothing more than an effusion of lymph originally, which has become vascular and adherent to the surrounding serous membrane from which it has been poured forth, and then become, in all respects, like cellular membrane.

This effusion is undoubtedly fibrin. Some have thought it to be one thing, and some another; but, in an article contained in the twelfth volume of the *Medico-Chirurgical Transactions*, Mr. Dowler shews it to be fibrin. I never had any doubt upon this subject, and many persons coincided with me in opinion; however, Mr. Dowler has brought forward evidence in proof of it. You will find it stated, in Andral's *Morbid Anatomy*, that Lassaigne, a French writer, has also ascertained the same circumstance experimentally.

In this deposition of fibrin you will find two portions—one solid and the other fluid. The solid is fibrin itself; but within the various portions of the solid mass, between the layers, or, more properly speaking, in cells, is found a fluid which is ascertained to be albumen. When these depositions have become adherent, they may be injected from the surrounding parts; but, as I mentioned with respect to a coagulum of blood, that it has been seen to have vessels of its own, independently of its connexion with the surrounding vessels—although such vessels become at last connected with those that surround them—just in the same way these depositions of fibrin will, before they are adherent to the surrounding serous membrane, become vascular; vessels will form in them, and within them there has been found even pus, while they were still unconnected with the sur-

rounding parts. Andral says that he has distinctly found a fibrinous concretion swimming in liquid unconnected with the surrounding parts, in a serous cavity; and frequently, even in such as these, he has found pus. It is said that the circulation within these parts is of three kinds. When they are first formed, currents take place in the interstices, through irregular areolæ; in the next place, insulated vessels are found; and, thirdly, these vessels at last become continuous with those of the neighbouring parts; and Andral says that each of these stages may exist in different parts of the same fibrine.

I may mention, that when you open a serous membrane which has been inflamed, you will find upon it points, or stars, or patches of vascular redness. In the first degree of inflammation there are merely points; if there be still more, there are stars; but if there be a still higher degree, and very numerous vessels contain blood, then you have a diffused redness.

Cellular Membrane and Substance of Viscera.

When the part inflamed is either the cellular membrane or the substance of organs—as, for instance, the liver—the pain is fixed; not diffused, as in the case of a mucous membrane, but fixed and rather acute, but not so acute as in an inflamed serous membrane. It is rather dull, but rather acute notwithstanding, and even pulsating, or throbbing. There is an effusion of serum into the surrounding parts, and at length suppuration takes place in that spot where the inflammation is most intense; and in the midst of lymph an abscess is formed, such as I have previously mentioned. If any tumor be produced, you find it hard, and, of course, when it suppurates it becomes soft, and the pain remits, though, generally, the tension from the collection of matter keeps up the pain and the other symptoms. When the matter escapes, the part heals by granulation and cicatrization, as I mentioned when speaking of abscesses.

Now inflammation of the substance of the viscera, or of any part of the cellular membrane forming an abscess, rarely ends in gangrene, except that particular form which is called *carbuncle*. The inflammation of the cellular membrane, or substance of the viscera, is called *phlegmon*; when, however, it is near the surface, and has a tendency to produce gangrene, it loses that name, and we call it carbuncle. In carbuncle, the cellular membrane will die to such an extent, that surgical aid is necessary, in order to make a very large opening for its escape.

Inflammation of the cellular membrane and of the viscera is sometimes not insulated, but diffused, and, as I mentioned, when speaking of the skin, this is frequently called

erysipelas phlegmonoides—provided the skin is also inflamed, or *diffused inflammation of the cellular membrane*, if the skin is not inflamed.

This inflammation is occasionally chronic. Nothing is more common than for inflammation of a mucous membrane to be chronic: but this inflammation is not so frequently chronic, though sometimes it is, and we observe this especially in the lungs and liver, so that people labour under chronic peripneumonia, and more frequently under chronic hepatitis.

Fibrous Membranes.

With respect to the fibrous membranes, this name is given to fascia, aponeurosis, tendons and their sheaths, ligaments, and periosteum. If these be inflamed, the common disease which exists is rheumatism. You may, of course, have inflammation of any of these parts without rheumatism; but if there be a general tendency to inflammation of these throughout the body, it is called rheumatism; frequently, however, the inflammation of a single one of these is the same disease. In rheumatism, although the pain is obtuse, yet there is a feeling of tension, and gelatinous exudations take place, something like synovia. This form of inflammation rarely runs on to suppuration; still more rarely, if ever, to mortification. The pyrexia may be out of all proportion to the violence of the local inflammation, and indeed one character of the disease is the occurrence of profuse sweating. In acute rheumatism, you seldom find profuse sweating to be absent, either at one period of the disease or at another. It generally occurs at the commencement of the affection; at any rate, if the patient be kept at all warm, the tendency to it is sure to show itself; and the sweats are frequently of so marked a character, that you can be at no loss instantly to discover their nature. They are usually very sour. This variety of inflammation generally terminates in resolution, or degenerates into the chronic form; perhaps with a chronic collection of gelatinous matter about the joints and in the thecæ of tendons.

The peculiarity of this inflammation is, its great tendency to wander—to *migrate*, as it is said, so that you shall have it now in one joint, and in a few hours in another. Occasionally it is metastatic; it will cease in these particular parts, and inflammation of an internal part will begin, generally an inflammation of some membrane, either the arachnoid, I believe, or the dura mater, we cannot say exactly which; but, certainly, more frequently than not, it is the pleura or the pericardium, the latter, however, more frequently than any other. But it is not by metastasis only that this internal inflammation takes place; for you will continually see inflammation of the pericardium or of the pleura occur in rheumatism, while the affec-

tion is going on in the joints, just as violently as before; and frequently this internal inflammation does not take place till after the rheumatism has been over some time. When the pericardium, for example, has been inflamed, if the person do not die, the pericarditis has a great tendency to become chronic; disease of the heart is then set up; the substance of the heart itself frequently becomes thickened, or hardened, or softened; the internal membrane of the heart, chiefly about the valves, becomes affected, and you have all the symptoms of a diseased heart: you have organic disease of the heart established. This is by far the most common origin of diseased heart in young persons. Pericarditis is first set up, and this pericarditis is generally connected with rheumatism.

Such are the chief varieties of inflammation occasioned by structure.

Inflammation.—Varieties in the Symptoms of Inflammation from Sympathy.

The sympathies of the particular part affected likewise occasions varieties in the general symptoms. You have some symptoms from the sympathies of the *general* structure affected: for example, if the structure affected be a serous membrane, from its sympathies you have a peculiar pulse; in general, a hard pulse. From another sympathy, when fibrous membranes are affected, you have a particular sweating. But, besides these general sympathetic effects—these effects from a particular structure, you have peculiar sympathetic effects from the *organ* itself, to say nothing of its structure. When the stomach or intestines are inflamed, you have generally a sensation of great weakness and a wiry pulse; but this does not occur as the consequence of an inflammation of the peritoneal coat of the intestines, or any other of their coats, for then it would take place when a similar structure was inflamed in other parts, but it occurs because the organ affected is the *intestines* or *stomach*. When the kidney is inflamed, you have vomiting, a retraction of the testis of the same side, and various other symptoms, simply because the *kidney* is inflamed. Hence you have a certain set of symptoms in inflammation from the peculiar sympathy of other organs with the part affected.

There are other varieties also observed. Erysipelas of the head has a great tendency to run to the brain. One can hardly say this is metastasis, because the inflammation frequently continues violent in the face itself, when the symptoms of phrenitis come on. Inflammation of the parotid glands of that peculiar kind said to be contagious, called *mumps*, has a great disposition to occasion sympathetic inflammation of the brain or of the testes. Indeed, when any organ is inflamed, and the inflammation continues any length of time, it is not uncommon for ano-

ther and a distant organ to become inflamed likewise; and these organs are generally either the brain, the lungs, or the alimentary canal; sometimes however the skin, and sometimes the throat, but more frequently the brain, the lungs, or the alimentary canal. When a part has been itself once inflamed, it has a great tendency to fall into inflammation again from slight causes. You certainly have inflammation of an organ from sympathy with the particular organ affected; and very frequently when this second inflammation occurs, although it may be very slight, it is attended with very considerable ill effects; the constitution is so much impaired by the first inflammation, that the addition of disease in another organ produces, though slight, very great danger, and frequently death.

Intermittent Inflammation.

Inflammation is sometimes intermittent, and sometimes absolutely periodical. Inflammation of the eye has been observed to recur at particular periods; it has frequently been observed to be intermittent—to come on violently every evening, or every other evening, at a certain hour: I have seen periodical catarrh.

You will have variations of inflammation according as the constitution is healthy or unhealthy. If the constitution be healthy, the inflammation is more active—is more rapid, and the terminations, when they occur, are more perfect. When the constitution is in an unhealthy state, the parts inflamed are feeble; there is less activity of the inflammation, less brightness of colour; the terminations which take place are less perfect, and the secretions of the part are more unhealthy.

Specific Inflammation.

You have still farther varieties in the symptoms of inflammation, according to the cause which has produced it, and the inflammation on this account is frequently termed *specific*; for example, inflammation of the skin, as in small-pox, is induced by a specific cause, and therefore it is called a specific inflammation. Independently of its cause, some inflammations run a peculiar course, and are therefore specific: such is erysipelas.

PASSIVE INFLAMMATION.

I have been considering active inflammation, but inflammation is said also to be passive. If in an inflammation there be but little pain—if the redness be dull—if the heat be but slightly increased, perhaps not at all—if there be more than the usual swelling, and a very abundant secretion, the inflammation is said to be passive. I have already mentioned that some persons object to this term, and say that such a set of symptoms as these are not inflammation; and

Andral, to avoid the difficulty, calls all inflammations and congestions hyperemy—the excessive presence of blood in the part. Active hyperemy is synonymous with active inflammation; whereas, if the symptoms be those I have just described—a dingy colour, flaccidity of the parts, little or no pain—then, instead of calling it passive inflammation, which he says is absurd, he calls it passive hyperemy.

Acute and Chronic.

The terms acute and chronic apply merely to the duration of inflammation, and not to the difference as to whether it is active or passive. An acute inflammation may be at first passive. When a person is already weak before the attack of inflammation, or the part has been frequently inflamed before, then at the very outset the inflammation may be of a passive character, although acute. After inflammation has continued for a length of time, it may be still active, the part may still be hot and painful, and the state may yield only to the remedies of active inflammation. Hence active and passive with regard to inflammation, as with respect to all other diseases, are not synonymous terms with acute and chronic.

Tonic and Atonic.

You will find inflammation sometimes distinguished into tonic and atonic; and these terms refer not to the active or passive condition of the part, but to the powers of the system. If the constitution be strong, the inflammation is called tonic; whereas, if the system be in a weak state, the inflammation is called atonic, because inflammation is sometimes very violent in part, and yet there is no power, the activity is all of short duration, the powers are exhausted, and perhaps mortification takes place. The activity—the violence of inflammation in a part—is more than commensurate with the powers of the system. If the part be in a state of activity, the inflammation is called active, but if not, it is called passive; whereas, the words atonic and tonic rather refer to the powers of the system at large.

Effects of Inflammation on Structure.

Now inflammation, when it continues, will cause an enlargement of parts; it will cause them to increase—to be over-nourished, and then the occurrence is termed hypertrophy—excessive nourishment. On the other hand, it will cause them to waste—to be under-nourished, if I may so speak, and then it is called atrophy. It will occasion them to be indurated, to be softened, and it will give rise to various transformations, so that a part shall become pretty firm cartilage, or actual bone. These results of inflammation are sometimes combined in the same part, so that a part shall be both hypertrophied and atrophied. If a part be compounded of dif-

ferent structures, one structure may increase, become harder and thicker than before, while another constituent of the same organ shall waste away; and one part shall become indurated while another shall become softened, so that you may have hypertrophy and atrophy—induration and softening—in different structures of the same compound organ. For example, you frequently see a thickening of the investing membrane of an organ, and a wasting of the substance of the organ itself; you will sometimes see a thickening of the cellular membrane, while one of the other structures shall waste away. The retina is sometimes found in a state of atrophy, while other portions of the eye are excessively thick, or indurated to cartilage. Acute inflammation more frequently, as I have already said, causes softening than hardening; and wasting rather follows acute inflammation than chronic, excepting when the chronic form induces such over-nourishment of one constituent part of an organ that another structure wastes. Generally, however, acute inflammation is disposed to produce softening; and if any other change occur, it is rather that of atrophy. Chronic inflammation, on the other hand, chiefly causes parts to become hard, and hypertrophied, over-nourished, thickened; and it is this which causes transformations—causes parts to become cartilage or bone. I do not mean to say that these will not arise without inflammation; but when they are produced by inflammation, it is usually the chronic and not the acute form which gives rise to them.

Analogous and Non-analogous Transformations.

Besides, however, these alterations of structure with regard to size and consistency, and these changes to a new structure, but still a structure which exists in the body, you will find parts which have been inflamed acquire a perfectly new character, and form a structure unknown to the healthy body. If the changes of structure be merely to bone, or cartilage, structures which exist naturally in some parts of the body, then the formation is called *analogous*—that is to say, it is analogous to a structure of the body; but if the change of structure produced do not naturally exist in the body, then it is called *non-analogous*. The same occurrence takes place with respect to the fluids which are poured forth in inflammation. Serum, fibrin, and mucus, which are occasionally poured forth in an increased quantity in inflammation, are analogous to what exist in the body naturally; but that is not the case with pus, and consequently it is non-analogous. You therefore see, that in the case of both the fluids and the solids, the results of inflammation are similar,—the productions may be analogous to what occur in the body, or they may be non-analogous.

Now it is to be remembered, that these

CLINICAL LECTURES,

Delivered in the Dispensary of the London University,

By DR. ANTHONY TODD THOMSON.

LECTURE VII.—Nov. 29, 1831.

Melancholia—Chorea—Epilepsy.

various changes of size—hypertrophy and atrophy; of consistency—induration and softening; as also these transformations; the occurrence of hæmorrhage, the occurrence of effusion,—dropsy, the occurrence of a discharge from a mucous membrane—a gleet, and perhaps adhesion, ulceration, and mortification—nay, even some say suppuration—may take place without any inflammation. They are continually the effect of inflammation; but certainly most of them do sometimes occur without our being able to discover that any inflammation existed. In the case of hæmorrhage, though it is frequently the result of an active inflammation, and generally too in a mucous membrane, yet it is often passive, and is poured forth without either pain, heat, swelling, or general irritation—nay, it is frequently poured forth without any fulness at all to be discovered in the vessels of the part. I have opened persons who have died from hæmorrhage, in whom there was no collection of blood to be found in the parts which afforded the hæmorrhage. While every part of the body from the head to the foot was perfectly pale, yet there was hæmorrhage from time to time until death ensued; and it must have come from small vessels, because no large vessels could be discovered ruptured. Certainly you will have chronic hydro-thorax and ascites without any inflammation of the serous membrane—no pleuritis, no peritonitis. With respect to mucous membranes, you continually see a gleet without the occurrence of any one symptom of inflammation. It is even thought by some, that suppuration of the lungs will take place without inflammation—that where a single romica of mere tubercular matter has been generated, and suppuration has taken place, there has often been no previous inflammation. Whether that is correct, I will not say; but certainly large adhesions continually occur in the pleura, in persons who have no recollection of having been the subjects of inflammation within the chest. Occasionally portions of fibrin are spit up from the bronchial tubes, without a person having beforehand suspected the slightest inflammatory state of those parts. Ulceration is thought by some surgeons to occur occasionally in the cartilages of joints without inflammation; and certainly ulceration will now and then take place in the skin and throat, with little or no inflammation. Such ulceration is continually seen in scurvy: ulcerated, like softened, mucous membranes, are sometimes quite pale. Mortification will occur every day without inflammation. Instances of this are seen in the lungs, and more rarely in other viscera. It will also occur in the extremities, from bad food, and more especially from that description called *ergot of rye*. But I must resume this subject at the next lecture.

GENTLEMEN,—The case of Melancholia to which I alluded in my last lecture, and which is still under treatment, has afforded you an excellent opportunity of observing the characters of that disease. It is one which seldom presents itself in the practice of a Dispensary. I will first read to you the successive reports subsequent to that of the 17th, up to which point they have been already given; and then offer you a few remarks on the general nature of the disease, its causes, and its probable termination.

On the 22d—the patient, Ann Rooke, stated, that her melancholy feelings and forebodings of evil were rather worse than better; but that she had slept well on Sunday night after the dose of the Tincture of Foxglove was increased. On examining the pulse, the system appeared to be under the sedative influence of the Foxglove. She, however, still suffered from pain at the occiput; the bowels were open, the motions dark, and the urine also was scanty and high coloured; the tongue was, as before, furred, and studded with red papillæ. She was ordered the following mixture:—

R Tincturæ Digitalis, ℥i. xl.

— Hyosciami, ʒij.

Infusi Calumbæ, f. ʒjss. M. Suman-
tur Cochli. iij. majora 8va. quæque
horâ.

She was also directed to take at bed-time, nightly, gr. xij. of Camphor, and gr. iv. of Extract of Henbane.

On the 24th, the report is—Has not been so sick, although the nausea continues. She has slept better for these two last nights, but still complains much of her head, with a creeping sensation over the body, great languor, and the same confusion of ideas and despondency as before—that is, of a constant apprehension of death. She also speaks of an uneasy sensation at the chest until she takes food; yet the appetite is indifferent. She has no religious misgivings to alarm her. The tongue is rather cleaner; the pulse small and quick; the alvine discharges are still dark and offensive. The pills, with Camphor and Henbane, were desired to be continued, the mixture with the Foxglove omitted, and the following mixture to be taken in its stead:—

R Animonii Tartarizati, gr. vj.

Aquæ Distillatæ, f. ʒvj. M. Pars 4ta,
primo mane, quotidie, sumenda.

On the 26th she stated that her anguish of mind was greater than ever, and occasionally despair took possession of her; her sleep was also more disturbed by frightful dreams. She admitted, however, that she was daily more comfortable, in the mornings, as long as the vomiting and sickness caused by the mixture lasted. The Antimonial mixture, besides vomiting, purged her freely; the stools were still dark and offensive, and the urine was high coloured; the pulse small, but less frequent and more regular, and the tongue as before.

She complained of continual restlessness, and a desire to move from place to place.

To-day, 29th, she says she feels much better; and although she is still haunted by horrifying thoughts, yet, she has slept better for three successive nights. Her bowels are now rather costive; she has been ordered to take a brisk purgative, and to continue the use of the Antimonial mixture and the Aodyne pills.

Such, gentlemen, is the state of this poor woman at this time. There is no doubt that her disease is *Melancholia*, a variety of Insanity, which has, in many respects, a close similitude to *Hypochondriasis*; but which, requiring a different mode of treatment, is necessary to be distinguished from it. Let us examine, therefore, in the first place, the circumstances which enable us confidently to pronounce this case to be one of *Melancholia*: they are these. In the history of the case, we find, that, at the time the patient was attacked, she was in good health; that her spirits became suddenly depressed from some family matters, and she has since been utterly uncomfortable in her mind. This, therefore, is one of the circumstances which determines the disease to be *Melancholia*; the attack was sudden, the exciting cause a mental one, and there were no premonitory symptoms of *Dyspepsia*, which always precede attacks of *Hypochondriasis*. And here let me impress upon you, gentlemen, the importance, in your inquiries after causes, to ascertain accurately the state of the patient previous to the attack, as it is only by doing so that you will be enabled to form a correct opinion of the changes which the morbid causes have produced in him. In the second place, the symptoms of the case before us are, various phenomena of *despondency* and *despair*; the patient merely says that she "is uncomfortable in mind;" it is only by a kind of cross-examination that we discover the existence of the cerebral and gastric symptoms; whereas, in *Hypochondriasis*, the mental phenomena consist, almost wholly, of erroneous ideas of the corporeal health, or of the make or the body of the patient. It is, however, necessary that you should be made aware that the absence of dyspeptic symptoms is not essential, in a case of *Melancholia*; and, although they have

not displayed themselves in the instance, before us, yet, it sometimes happens that the symptoms of both diseases are seen in the same person. At one time, the illusions of *Hypochondriasis* prevailing, at another time the despondency and despair of *Melancholia*. In such a case the attack is still to be regarded as the latter malady, and treated as such. In the third place, we are guided in our diagnosis also, in some respects, by observing the predisposition of the patient to the disease, as indicated by those appearances which are admitted as characterizing the melancholic temperament—which are, a lean and dry frame of body, small and rigid muscles, deep black hair, a pale sallow complexion, the eyes sunk in their sockets, with large veins, denoting, as Cullen would express himself, a balance of the sanguiferous system upon the side of the veins, with a constant expression of anxiety and dejection. With such a tendency to the disease—when sudden or violent, and especially painful domestic changes, or other causes of grief or despondency occur, then *Melancholia* rapidly displays itself.

This disease, like many others, shews itself in various forms. In the present instance, the constant restlessness, and desire for a change of place, arranges our case under that variety which has been termed *irrabunda*, or *Restless Melancholy*. In endeavouring to account for the corporeal symptoms, it is necessary to take into consideration the effect of the depressing passions upon the body, in producing a torpor in almost every irritable part; thence the paleness of the countenance, the contraction of the features, the sensation of languor and coldness of the extremities. To the same cause also is to be attributed the feeling of anxiety and oppression at the chest, and the sensation of a fulness at the heart, as if it would almost burst—a feeling which you will find very often expressed by melancholic patients. The sighing that accompanies this state of the circulation is an effort of nature to restore the ordinary pulmonary circulation, and to promote that due change in the blood which is retarded in *Melancholia*. It is not difficult to account for the languor always more or less accompanying *Melancholia*. Sudden grief often operates on the system as a powerful sedative; the corporeal strength of the patient appears suddenly to leave him; his knees tremble; the muscular power of the limbs seems to fail; and in some instances he falls to the ground in a state of syncope. When the mental impression, however, is less powerful, no sudden effect of this kind takes place; but corporeal symptoms are gradually induced, some of which are present in the case before us. Thus we find such an irritable state of stomach, that, along with a sensation of pain in it, there is occasional vomiting after taking

food, which would almost lead to the belief of the existence of inflammation in the stomach; this symptom is more common in females than in males. The load of blood which is thrown upon the liver readily explains the condition of the biliary secretion: as in our case, it is changed in colour and in consistence; and it is highly probable that, although this depraved state of the bile is evidently the effect of the unhealthy condition of the brain, yet to it may be attributed many of the other symptoms—the irregular action of the intestines, the flatulence, and the faulty state of the digestive organs which sometimes accompanies the mental dejection. The headache is generally regarded as altogether sympathetic; but, in the case before us, we might suspect some obscure inflammatory state of the cerebellum, as the acuteness of the pain referred to the occiput can scarcely be accounted for on any other supposition. If this opinion were correct, it might be asked, are not the symptoms, as connected with such a state of the cerebellum, at complete variance with the doctrines of phrenology? But this is a part of the subject which I confess my inability to discuss; and I must also admit, that there is no demonstrative proof of such a state of the cerebellum, which is only presumed from the acute pain at the occiput. It is also true, that lunatics have expired with symptoms indicating affections of the brain; and yet, on dissection, the keenest eye has not been able to discover any appearance that could explain them. There is another physiognomical characteristic of this affection, which, although unnoticed in the reports, yet is present in the case under treatment, and must have struck you—I mean the expression of frowning, which the drawing together of the eyebrows never fails to produce, when the attention of the patient, as is generally the case, is absorbed with the prevailing idea which fills the mind. It is the despotic ascendancy of this prevailing idea also over the mind, becoming an habitual subject of nocturnal meditation, which is the cause of the obstinate watchfulness that always accompanies this form of Melancholia. The idea which, in the case before us, overshadows the enjoyments of life, is not unfrequent; and what may appear remarkable, this very dread of dissolution has driven the wretched sufferers into the arms of that awful being, whose imaginary advance embitters every moment of their existence. Such is the brief view of the *ratio symptomatum* which I have to present to you; and it will not be out of place, even whilst the case is under treatment, to mention the usual terminations of this variety of Insanity.

The usual termination of Melancholia, when it is not averted by medical treatment, is either in a state of mild, or of furious delirium or mania, or of despair. In which of

these states a case of Melancholia is likely to terminate, may in some degree be foretold from a knowledge of the natural disposition of the individual. The first state is most likely to occur in habits endowed with the power of patiently sustaining pain, whether corporeal or mental; the second and the third, in those in whom, from the structure of the nervous system, any description of pain cannot be long borne without exciting an increased arterial action in the brain. Of these three terminations the most common is that of despair.

It is only by reflecting upon the many painful feelings, both corporeal and mental, constantly preying upon the wretched subject of Melancholia, that can afford you any accurate idea of the real origin of despair in these cases. The sense of moral duty and of religious obligation, however strong its influence may have been in regulating the conduct of the sufferer before his attack, is gradually weakened by the contemplation of the wretched prospect which appears to surround him on every side; he discerns nothing but a dreary and comfortless desert, without a single oasis to refresh the mind or admit of even a momentary feeling of enjoyment. Restless, anticipating no respite from bodily and mental suffering—tired out—hopeless—the victim of despair can perceive no prospect of relief from his sufferings but in the grave; and he resolves on suicide, not so much from the desire of death as of rest. On the one hand, he sees in every object only the misery of life; whilst, on the other, he believes that he can obtain relief by withdrawing himself from it. He is incapable of exercising a correct judgment; and, tired out with the buffetings of the storm, he seeks the only port in which he conceives shelter is to be found—the quiet of the grave. The beauty and truth with which this feeling is depicted in Lord Byron's poem of the Giaour, will, I am certain, plead my excuse for quoting a passage from it.

“Waste not thine orison—Despair
Is mightier than thy pious prayer.
I would not, if I might, be blest—
I want no paradise—but rest.”

Such is that state of mind which leads the victim of despair to form the resolution of suicide; and you have more than once, gentlemen, heard our unfortunate patient declare, that ideas have passed through her mind “that make her shudder even to look back upon them.”

With regard to the treatment of this case, you perceive, gentlemen, that although I think it possible that a low degree of sub-acute inflammatory action may exist in the cerebellum or its membranes, yet I have not ordered blood letting either general or local. I wish to direct your attention to this circumstance, to impress on your minds the

fact, that insanity always more or less implies debility; indeed, the general appearance of the patient, the absence of any thing like acute symptoms, and the feeble beat of the pulse, render general bleeding inadmissible; and as there is neither turgescence of countenance, nor sense of heat in the scalp, nor noise in the head, I have felt disposed rather to trust to the effects of emetics and purgatives, than to abstract blood even locally.

Emetics are very generally employed in the acute form of Insanity; and, in Melancholia, when the constitution of the patient is unimpaired by intemperance—when the torpor of the liver and alimentary canal indicates oppression rather than exhaustion of the vital powers, as in Rooke, I am of opinion that the excitement given to the stomach by a moderate dose of Tartar Emetic, operating in some measure as a counter-irritant, is always beneficial. In this case, the daily repetition of it has been productive of comfort to the patient, whose mental feelings continue calm and undisturbed as long as the sickness and vomiting continue. It is not uncommon to combine emetics with purgatives; but in the present case the Tartar Emetic has both evacuated the stomach and the bowels, and has rendered such a combination unnecessary. I have considered it also advisable to administer a narcotic in the evening, and the emetic in the morning, not merely with the view of procuring sleep, but to insure the emetic influence of a small dose of the Antimonial; for I have had many opportunities of verifying the remark of Dr. Cox, that one-third of the usual dose of Tartar Emetic is sufficient when a narcotic has been taken on the previous night, and a better effect has followed than when no such precursor had led the way. The purgatives which had been previously administered, and which are always indicated by the torpid state of the bowels, in this variety of insanity, were continued as long as the nature of the evacuations required their employment; and I was desirous, by their means, to remove any congestion that might have existed before the use of the emetics was commenced. The continuance of the purgatives longer might have too much reduced the strength, whereas the emetics have had no such effect; the pulse is much slower, but at the same time it has filled, and has become more regular than before their commencement. It is true that Melancholia is what is termed an asthenic disease; but it is also one that is never benefited by tonic or stimulant remedies.

The constant watchfulness, and the necessity of procuring repose in every form of Insanity, are obvious; the only difficulty is in determining which of the narcotics ought to be employed. In acute Mania, digitalis effectually assists in reducing the furor, and promoting refreshing sleep, after the lancet and purgatives have been freely employed.

In Melancholia, such a previous reduction of excitement is not generally requisite. The Tincture may be administered at the outset; and on this account it was ordered the first day that I saw the patient, but its effects did not permit it to be continued, even after the dose had been reduced to forty minims in the twenty-four hours; a result which can only be referred to idiosyncrasy. I am, indeed, most anxious to impress upon your minds the fact, that Digitalis is not a direct sedative; its first action is decidedly stimulant, and, like all other narcotics, it depresses exactly in the ratio of its previous excitement. Wherever strong arterial action exists, its hurtful effects are almost instantaneously perceived; thence the necessity of reducing the force of the pulse previous to its employment; and, when this has been attended to, its calming and soporific effects are certain. You should, however, be made aware that, in Melancholia, the sedative or debilitating influence of Digitalis should be narrowly watched; for, if admitted to proceed beyond a certain point, a morbid action is readily induced, and congestion is the consequence. In Melancholia, opium is, undoubtedly, a hazardous narcotic; not less because of its tendency to produce over-distention of the cerebral vessels, than on account of the torpor of bowels which usually succeeds its use. There is much less objection to Camphor, which somewhat resembles Foxglove in its mode of operating; but when its use has been continued for some days, and, after acting as an anodyne, its effects begin to be perceived on the countenance, inducing pallor and lividity; and when the extremities become cold and insensible, its further employment would only be productive of much mischief—congestion of the lungs, suspension of the power of the heart, and general torpor of the system. In the present instance, its conjunction with Hyoscyamus has answered well.

I should now, gentlemen, proceed to give you some idea of the moral management necessary in Melancholia; but I will reserve what I have to say on this part of my subject until the termination of the case before us, and at present only remark, that the nature of the moral treatment must depend greatly on the prior mental habits or propensities of the patient. The circumstances under which patients are attended at Dispensaries are unfavourable to the moral treatment of the disease; and, although the condition of the poor, and the constraint of labour which necessity imposes, generally secures them from feeling the burthen of *ennui*, and suffering from a refined sensibility, yet, when they are too ill to follow their ordinary occupations, the general vacuity of their minds tends to aggravate the excess of their sufferings, owing to their attention being wholly absorbed in them.

Chorea.

The girl who was labouring under this disease is convalescent, the irregular movements having gradually yielded to the influence of the purgatives and the carbonate of iron.

Epilepsy.

Leni has continued free from any return of the fits, but to-day he experienced something like a threatening of them; for instance, in the morning he felt giddiness and temporary dimness of sight, with great confusion of intellect, which continued for nearly two hours, during which time he heard the voices of those who spoke to him, but was unconscious of the nature of the subject of their discourse. As this state declined, he felt cold, and experienced a rigor. The pulse is small and irritable; the tongue furred; the urine natural; and the bowels are open.

He has been ordered to take directly a brisk cathartic; to have a seton introduced into the nape of the neck; and to take a pill, containing gr. v. of the oxide of zinc, three times a day.

CHARACTER AND TREATMENT OF CHOLERA.

By C. NEGRI, M.D.

[Continued from the preceding Number, p. 351.]

As my object throughout the preceding letter has been to direct the attention of the Medical Board to facts, statements, and authorities, which did not appear to me to have received sufficient notice, I have refrained from encumbering the narrative with any observations of my own, either by way of corroboration or of commentary. But as Torti's views on this subject are not generally known in this country, I may, perhaps, be excused for laying this letter before the profession with a few explanatory remarks. I am not ignorant that some writers of the present day have taken a very similar view of the nature, and have proposed a very similar treatment, for the cure of malignant cholera with those which I have now been giving. I am aware that Mr. Searle's theory of this disease is not essentially different from that entertained by Torti, and I believe that had he known Torti's works on Pernicious Fever, he would have acquiesced in most of the sentiments of that author. Mr. Searle notices the great resemblance of malignant cholera to

intermittent fever; but speaking of the stage of re-action, he compares it to the congestive typhus of this country. Not having yet witnessed any cases of malignant cholera, I cannot positively object to the justice of this comparison; but I am quite certain that these pernicious fevers differ from the congestive fever of England in more respects than one. The pernicious fever seizes the patient suddenly, when in apparent health; it more or less assumes the intermittent form, the intermission varying, however, very much in length, and occasionally being so incomplete as to be almost imperceptible; it seldom presents at any periods of its course symptoms of re-action; it is uniformly fatal if left to itself; and, when properly treated, its disappearance is almost quite as rapid as its progress, if neglected. In all these respects, and in others unnecessary to mention, these two affections are essentially different; and I think it will be allowed, that when compared in these points with spasmodic cholera, the *febris perniciosa* of Torti, is not extremely dissimilar. I have seen several cases of this fever, and although their symptoms somewhat varied, their general character is so peculiar, that when it is once witnessed, it can never be again forgotten. It usually commences with the ordinary symptoms of a single tertian or quartan ague in a severe form. The second paroxysm is invariably much more malignant than the first, and the disease is so strengthened at every new fit, that the third or fourth is in general quite sufficient to destroy life, if the proper remedies be not had recourse to. Occasionally it wears the form of a double tertian, or quotidian, with *accessiones subintrantes*, as the old writers would say, and with such imperfect intervals, that its periodical character may be very easily overlooked. There is no subject on which opinion is less divided, at least in Italy, than that of the treatment of these pernicious agues. Since Torti wrote, no one thinks of employing any other remedy, in the first instance, than bark, in large doses; and the only point of difficulty to be considered by the practitioner is the recognition of the disease, for it not unfrequently stands in hazard of being confounded with diseases of a different character. I was in the habit of mixing an ounce of bark with a drachm of laudanum for a dose, which

was given at the first appearance of the disease, and was repeated at intervals, proportioned in length to the intensity of the symptoms. Mr. Annesley, I am aware, has employed bark in India, and Ponpiriff in Orenburg, but rather as a tonic, in the advanced stage, than with any more curative or active view. The sulphate of quinine has also been tried in Petersburg, but with the same view; and Mr. Searle has given this salt with advantage, but towards the close of the disease.

There are several reasons, however, for objecting to the use of this form of the bark, at least in preference to the common powder. In the first place, cinchona, besides a certain quantity of quinine and cinchonine, contains some other principles of an astringent nature, which may render it more suitable for a disease, in which the mucous lining of the alimentary canal is in a state of extreme excitement. Secondly, I apprehend it has not been given in sufficiently large doses, nor early enough in the disease. Lastly, I have tried it, and its success has not been by any means so complete as that which followed the employment of the bark in a simple state.

Towards the close of the summer of 1821, when in Italy, I was requested to see a lady of a delicate constitution, and about 50 years of age. She was labouring under the symptoms of a *febris perniciosa dysenterica quotidiana*;—cold shiverings; profuse evacuations from the bowels; nausea, but not vomiting; tongue pale, but moist; small, weak pulse; cold, clammy perspiration. She was so weak, that *deliquium animi* occurred at almost every stool. Towards evening, these symptoms wholly ceased to re-appear on the following day, with increased severity. After the second fit had made me certainly acquainted with the nature of the malady, I lost no time in having recourse to the bark; but as the sulphate of quinine was then coming into notice, I thought proper to employ it. I do not exactly recollect the dose in which it was exhibited, but it was given in the form of pills, at short intervals. No paroxysm occurred on the day following its first employment, but a fit took place the day after, only in a mitigated form. The simple powder was now substituted for the salt, and the patient was speedily cured.

From the result of its action in this case, together with the reasons above given, I have been induced to suspect, that in cholera, where there are copious evacuations from the bowels, the sulphate of quinine may not only aggravate that symptom, but be carried off before the necessary effect has been produced upon the system. These consequences I do not apprehend from the pure bark, either in the form of powder or tincture; more especially where it is combined with opium or laudanum. The only reasonable objection which occurs to me against its applicability in cholera, is the extreme rapidity of this disease, and the very short period which it allows the practitioner for the employment of any remedy. In many instances, however, its course is less rapid, and I do entertain great hope that it will be found highly beneficial in these, if not in all cases. In this opinion I am the more encouraged since I have read, in the *Morning Herald* of the 1st instant, that Dr. Barry, at the public meeting held on the 26th of October, at Newcastle, admitted that “there was more analogy between it (malignant cholera) and the worst forms of tertian ague, or intermittent fever, than between it and the ordinary cholera of this country.”

To give some idea of the extreme severity of the cases in which Torti successfully employed the bark, I beg leave to condense from that author's work the two following histories of the *febris algida perniciosa*; some points of difference will be discovered between them, but these, in no degree, affect their essential features, which in both are precisely the same. “When I reached the patient, he had been several hours labouring under the disease. I found him universally cold as marble, with the pulse altogether, if I may so say, absent, breathing laboriously, and having a leaden-coloured countenance. There was some torpor, but no confusion of intellect (*he never mentioned delirium*), and his urine was secreted in a small quantity. . . . I prescribed the bark in large doses. A gentle heat soon pervaded his entire frame; the pulse gradually returned; the respiration became natural; the face lost its leaden hue; the urine was secreted in its ordinary quantity, and in three days he was quite recovered.” Lib. iv. cap. iii.

In the second case, Torti found the

patient "quite altered in countenance, and as exhausted in strength as though he had been confined with illness for two months. On endeavouring to take the number of the pulse it could not be perceived; the skin was universally cold as marble; the urine was scanty, but highly coloured; the eyes were vivid, and unusually glistening; there was little thirst, and the mind was quite clear, but depressed; the patient saying, '*jam senti o me mori.*'"—Id. The bark was given in large doses, and at short intervals, in the usual way; every unfavourable symptom gradually declined in a few days, and ultimately vanished. In the fourth book of Torti's work, he relates very many similar and equally interesting histories of this pernicious form of ague; but as we trust enough has been already said to shew the striking resemblance of this affection to malignant cholera, and its perfect manageability under Torti's treatment, we shall leave the profession to peruse the author at their convenience, with this concluding observation—that the circumstance of Torti, Morton, and Mercatus, being somewhat less modern writers than some might wish them, cannot in the least detract from either the value of their experience or the veracity of their testimony. Facts are unchangeable things, which neither time, nor place, nor circumstances, can alter; and, although I do not pretend to say that the oldest writers are the best, much of the value of many of our modern publications, and much of the distinction of many of our living authors, are derived from works and writers now almost equally forgotten.

MALIGNANT EUROPEAN CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

IF you consider the following additional observations worthy of a place in your valuable periodical, I shall be glad to occupy a small space in your next number.

In my letter to you of the 18th instant, I have stated it as the conviction of my mind, that the modified cholera which has appeared on the continent of Europe, is a fever, differing only in de-

gree, and in a very few symptoms, from some other fevers of a malignant nature; but more intent upon the practical application of the doctrine which I had ventured to broach, than upon the establishment of a new theory, I said but little on that part of the subject. That I may not now, however, be suspected of theorising, or of offering opinions without due consideration, I will just mention a few of the opportunities I have had, from my public situation in his Majesty's navy, of seeing and treating various fevers in all parts of the world.

In the winter of 1799, after the disastrous retreat of our army from Holland, an hospital ship was fitted out at Portsmouth for the reception of the sick and wounded Russians, then our allies. In that hospital I had but *too many* opportunities of witnessing typhus fever, in its worst shape; and the opinions I then formed were fully confirmed in the summer of 1801, during my attendance upon upwards of a hundred French prisoners of war, who were sent to our floating hospital, in the river Medway, with malignant typhus. Whilst in charge of his Majesty's Naval Hospital at Madras, I had also an opportunity of witnessing, upon an extensive scale, the fevers peculiar to India, (including dysentery, that greatest scourge of India, until the introduction of cholera.) During a residence of more than ten years in this town, the fenny country in the neighbourhood has been a prolific source of fever, chiefly of the continued, remittent, and intermittent types; for when I state that I have prescribed for fifteen thousand fever patients in that period, I shall be considerably under the truth. Few cases of typhus (and those of a mild kind) have come under my notice; indeed, I am inclined to hope that the malignant typhus, but too frequently met with thirty years ago, is now of rare occurrence, owing, I trust, to better management and free ventilation.

It is my firm opinion, after all that I have read or seen, that the miasm, or poison, producing typhus, malignant European cholera, and intermittent fevers, makes its primary assault upon the "nervous power" in general, and not upon any particular class of nerves; and by its powerfully sedative effect deprives the muscles of the only influ-

ence by which they are enabled to act. Hence the sudden prostration of muscular power, the spasms, &c. The suspension of the "nervous power" also puts a stop to the evolution of caloric from the arterial blood. Hence the sudden coldness of the surface of the body, the blue skin, the diminished capacity of the blood-vessels near the surface, the consequent retreat of the blood into the more sheltered and warmer parts of the body, and the fearful internal congestion, and interrupted function, of the various vital organs. I should also point out the total suspension of the secretions of bile and urine as a consequence of the loss of "nervous power," and which will also serve to strengthen my opinion, that the fluid ejected from the stomach and bowels, in malignant cholera, is not a *secretion*, but a *transudation*, occasioned by the gorged state of the vessels of the stomach and intestines: at all events, it does not resemble any known secretion of the human body, and I am disposed to attribute its presence rather to the cause above-mentioned than to consider it as a secretion occasioned by the introduction of the poison of cholera into the alimentary canal. I have been the more particular on this point, because some practitioners have considered this harmless fluid as being of an acrid, irritating nature, and consequently have recommended it to be purged off, an error which I consider would be fatal in the exhausted state of the patient; indeed, purgatives are out of the question, in my opinion, until re-action has been *completely* established. Some of your correspondents are of opinion, I perceive, that the poison of cholera vitiates the whole mass of blood; but I think the interruption given to the healthy function of the lungs, together with the diminished heat of the body, will satisfactorily account for the dark and thick state of the blood, without the actual introduction of the virus into the circulation.

This theory, of which I have attempted to give a brief outline, appears to me to explain, in the most satisfactory manner, the phenomena attending the attack of malignant cholera. There is one striking difference between the attacks of typhus and cholera: in the former the "sensorial," as well as the "nervous" power, is laid prostrate, whereas in the latter, the "sensorial

power" is little, if at all, affected. This fact, I think, proves pretty clearly that these two fevers arise from different poisons. Were it not for the acknowledged fact of the non-contagious nature of intermittent, I should say that I have seen (in this country) no disease so nearly resembling the cold stage of cholera as the cold stage of severe intermittent.

With respect to the treatment of cholera, I would strongly recommend the plan proposed in my last letter. In severe cases, where the heat of the body cannot be restored by the hot air-bath, I trust that galvanism will be found a most powerful auxiliary, by restoring the *vital action*, through which caloric can alone be evolved from the blood, and the absence of which (vital action) no artificial application of heat can compensate for any length of time. As the cold stage of cholera is confessedly the most important, I shall say but little upon the subject of after-treatment. Great care should, however, be taken to moderate re-action, for in all other fevers the hot stage bears a due proportion to the stage of oppression, and as in cholera that stage is severe, so we may expect violent re-action. I have not yet had sufficient opportunities of judging of Dr. Stevens's plan, but am disposed to think highly of it in the treatment of all malignant fevers.

With respect to the cholera, as it now exists in Sunderland, it appears to me to be of little consequence whether it has been imported or generated on the spot. Is it contagious, or is it not? I think no one will deny that typhus *has been* generated in England, or will say that it is incapable (under certain circumstances) of being communicated from man to man. And why may not a fever, with symptoms resembling those of cholera, also be generated in England?

In concluding this letter, I will just mention a case of cholera which occurred in this town in the early part of October. The symptoms were *completely* those of "Asiatic cholera." The patient a debilitated, tippling, old man of sixty, and death took place within thirty hours, yet there was not the least reason to suspect contagion from abroad, nor did any other individual of the family suffer from it.

It is beyond all doubt that cases do now and then occur in this country, in which it is utterly impossible (as far as

symptoms go) to say whether the disease is of Asiatic or of English origin.

I have neither time or space for any observations on fever in general, or on those more commonly met with in this part of the kingdom; I must therefore defer any thing I could wish to say on the subject till some future opportunity.

I remain, sir,

Your very obedient servant,

JOHN TWEEDALE, M.D.

Lynn, Norfolk, Nov. 28th, 1831.

DISEASES OF THE BRAIN.

To the Editor of the London Medical Gazette.

Wolverhampton, Oct 5, 1831.

SIR,

I SEND you notes of two cases of ramollissement and hypertrophy of the cerebrum, observed by me at La Pitié, in the service of M. Louis, who had never seen but one similar case; and I am not aware of any author who has mentioned such a morbid appearance. If you think them worthy of it, I shall feel honoured by their insertion in any number of the Gazette not occupied by more important matter.

CASE I.—On the third day, after a natural and easy confinement, this patient was seized with a pain in her head, but not sufficient to prevent her from getting up, and attending in some degree to her household affairs. The next day, while up and dressed, she became suddenly insensible, and the day following was brought in that state to the hospital. At the visit on the sixth day from her confinement, she was comatose, pupils contracted, mouth slightly drawn to left side, right arm and leg rather rigid, but sensible when pinched. During the visit, her breathing became stertorous. Ordered to be bled.

Next day, eyes wide open, and not fixed, but she gave no answer even by signs when questioned; breathing natural. Died during the night.

Autopsy.—The skull-cap and dura mater were adherent at the top of the left hemisphere, and the latter was torn in two places in removing the cranium. The suarachnoid tissue was full of vessels, and distended with serum. Upon

tearing away the pia mater, a good deal of the cortical part of the left hemisphere came away adhering to it. The whole of that hemisphere was softer than the right, and the corticle was red, and even purple in parts, from an appearance of venous effusion into its substance. The medullary part of that side had a grey tinge, and the red points seen upon slicing it were more numerous and darker in colour than usual. These appearances lessened as the brain was sliced lower; but, upon making a section through the corpora striata, both that and the thalamus of the left side were softer *and a third larger than those of the right side*. Indeed, a vertical section would have fallen far to the left of the usually central parts of the brain. The left side of the pons was in the same state, but the medulla oblongata and cerebellum were unaffected. The heart was rather softer than usual. This patient was rather thin, of middle height.

CASE II.—No history of this patient, a prodigiously fat woman, could be obtained: she came in, in a comatose state, with paralysis of the right side, and died in a few hours. The membranes of the brain were injected. Upon making a section of both the corpora striata, the same enlargement and softening were found as in the former case, but the medullary matter was of a yellow tinge on the left (the affected) side. The rest of the brain was healthy. In both cases, the convolutions of the affected side presented a flattened appearance.—I remain,

Your obedient servant,

T. OGIER WARD, M.B. Oxon.

NEURALGIC AFFECTION OF THE LEG,

With Chronic Thickening of the Periosteum,

CURED BY THE SULPHATE OF QUININE.

To the Editor of the London Medical Gazette.

SIR,

THE sulphate of quinine is a remedial agent so well appreciated by the profession, that there are few who have not observed, at some period of their practical career, the beneficial effects arising from its employment. That it is a most va-

luable medicine in the treatment of intermittent fevers, and of other fevers after the subsidence of the inflammatory stage, is a truth which cannot admit of doubt or controversy; but its virtues, in the cure of various other diseases incident to the human fabric, have not been so fully experienced, nor so highly lauded. However, it will be readily admitted that the celerity and permanency of relief afforded by any particular plan of treatment, furnish the most incontrovertible arguments that can be adduced to prove the rational and consistent principles upon which that plan is founded. From several cases that have fallen within my own observation, I am disposed to think that the sulphate of quinine, administered in neuralgia; will often be found an efficient method, not only of alleviating but of removing this most acute and distressing affection; and I have ventured to forward for insertion in the Gazette the accompanying case, as tending to evince its efficacy in certain chronic diseases, exhibiting somewhat of an intermittent character.

The subject of the following account may be said to have obtained a radical cure, inasmuch as she has had no recurrence of the symptoms characterizing the disease for the last six weeks.

I have the honour to remain, Sir,

Your obedient servant,

CHARLES THORNHILL.

Darlaston, Staffordshire,
Nov. 12, 1831.

Mrs. H. M. ætat. 55, of stout make, and sallow complexion, has for the space of three years suffered considerably from swelled legs, accompanied with violent and frequent pain in the muscles of the right leg, and with such extreme weakness in it as to render her incapable of performing her domestic duties. The pain has usually commenced about the neighbourhood of the heel, pursuing its course in the direction of the gastrocnemii muscles; but during the last six weeks previous to my being consulted, it had increased to such an intolerable degree as entirely to destroy her nightly repose. She states that it has been of a burning character, and that she has been obliged to keep a tub of cold water constantly in her chamber, into which she might immerse her leg to obtain temporary relief. The periods of exacerbation have been about sun-rise and sun-set; but a

distinct and well-marked interval has always been maintained between the paroxysms.

On examination, Aug. 3, 1831, (the time of this report,) a great thickening in the periosteum of both tibiæ was observed, and the veins of the leg were tortuous and varicose. The heat was much increased above the natural standard, and on making pressure along the seat of pain, extreme tenderness was manifested. In the right leg there was the cicatrix of an old wound, which had occupied the muscles of the inner calf about two years back, and which she described as having been healed with difficulty; and the heel itself seemed to be drawn up to the extent of an inch and a half, from a shortening in the tendo achillis. With respect to the constitutional symptoms it may be remarked, that the health had not undergone any material change. The pulse was rather increased in frequency, and the tongue coated with a whitish fur; the appetite was not much impaired, but the liver was deranged in its functions, as was pretty evident from the jaundiced appearance which the skin presented. In answer to several enquiries, she stated that twelve years ago, while resident in the West Indies, she had had a most violent attack of ague, since which she has been occasionally subject to a return of the symptoms resembling it. As the disease in question appeared to bear some comparison to ague, in regard to the exact approach of the paroxysms, and the remarkable interval between them, I was induced to direct my treatment accordingly.

R Pil. Hydrargyri gr. iv.

— Saponis c, gr. v. M. fiant pilulæ
ij. Omni nocte, horâ somni sumendæ.

R Quinina Sulphat. gr. ij.

Pulv. Cinnamomi c. gr. v. M. fiat pulvis, ter. die sumendus.

5th. The relief obtained has been but of trifling importance. Her appetite is a little improved, but the pain has returned as heretofore, and the legs continue at about the same temperature as at the last visit.

Cont. pilulæ.

R Quinina Sulphat. gr. iij.

Pulv. Aromat. gr. vj. M. fiat pulvis,
ut antea sumendus.

She has been directed to apply con-

stantly to her legs folds of linen-cloth, dipped in vinegar and water.

7th. Evidently better, having passed, to use her own expression, a more comfortable night's sleep than she has done for the last twelve months. The legs are not so tender on pressure, neither is there so much thickening of the periosteum; the pain still returns at the usual periods of accession, but does not remain so long as it was wont.

Cont. medicamenta.

Directed to roll the legs with flannel bandages, which are to be drawn as tightly as she can bear them.

11th. The improvement is still apparent; the return of pain is scarcely perceptible, and she can walk with comparative ease. Her countenance and skin are not so much tinged with bile as when I first saw her, and her sleep is less disturbed. The bandages have afforded wonderful relief.

Cont. pulveres, sed omitt. pilulæ.

21st. Up to this period the amendment has been gradual; last night, however, the pain returned at the accustomed time, but subsided after continuing for an hour; the bowels are rather in a constipated condition.

R Quinina Sulphat. gr. iv.

Pulv. Cinnam. c. gr. v. M. ft. pulvis ter. die sumendus.

R Pil. Rhæi Comp. ʒi. Fiant pilulæ xij. quarum duæ horâ somni pro re nata sumendæ.

Oct. 1st. Since the last report, every unpleasant symptom has disappeared, and she can walk without the aid of her stick.

MEDICAL GAZETTE.

Saturday, December 17, 1831.

“Licet omnibus, licet etiam mihi, dignitatem *Ar-tis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso.”—CICERO.

PROSPECTS OF A NEW ANATOMY BILL.

MR. WAREURTON is going to do something at last in the way of agitating the anatomical question: what that something is, however, we are not sufficiently fortunate to have yet discovered; but

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this we may venture to say, that in all conscience it is full time that something were really done. If the slowness of the process of bringing it forward is to be taken as any earnest of its perfection, this new measure must assuredly be very perfect indeed: the protracted nature of its progress would strongly remind one of what is said of the tedious, though triumphant parturition of the lioness—*diu parturit læna catulum, sed leonem*.—only that one part of the comparison unluckily still remains to be made out.

When Lord Calthorpe, on the night on which the former bill was withdrawn, was endeavouring to persuade their lordships to give it a second reading, he ventured to throw out the following prognostications:—“As to postponing the measure, he begged of their lordships to consider how the interval between this period and the *next session* of parliament would be spent. It would be an interval, not only of consideration, but of the continuance of a practice which all agreed in reprobating: it would perhaps be an interval during which *those crimes would again be committed* which had long been suspected, and which last year were brought to light. It would be continuing, and continuing unnecessarily, an odious stigma upon a liberal and enlightened profession—upon a profession which existed only for the public welfare, and whose services and whose respectability gave them a claim upon the protection of their lordships. He had no hesitation in saying that *the grievances* that might be *caused by delay* would be *infinitely greater than the benefit*.” And if his lordship has not proved himself a true prophet in saying so, never was any prediction justified by the event. We have had, however, a much longer time to test the accuracy of the prognosis in the present case than Lord Calthorpe had any idea we should. It was uttered in 1829, (June). Two complete sessions have

since gone by, and nothing in the shape of remedy has been attempted, while the evil has been advancing at a frightful rate of progress.

Yet there are those to be dealt with, it would seem, whom apparently no train of "grievances" can move. "Wait a little longer," "the time is not yet fitting," they tell us, "for entering upon this serious discussion;"—"the subject is one, the difficulty of which is equalled only by its importance; however, in the present excited state of the public mind it would be well to avoid all agitation of the subject, and it would in especial be *wise to delay* any legislative measure regarding it till that excitement has abated*." Now we would humbly presume to differ with the propounder of this advice: we make bold to question the prudence which dictates it; and we fearlessly appeal from the Lord Chancellor to *Mr. Brougham* to decide whether the urgency of the matter at issue is not more than sufficient to justify the overlooking of a mere temporary excitement. That excitement, we hold, makes for the active rather than the contemplative bearing of the friends of anatomy. Now, if ever, may the necessity of legislating for the regulation of the schools be properly appreciated—be *felt*—be set in the strongest light—be enforced. Excitement! Why, so there is an extraordinary degree of public excitement abroad; but let us understand what is supposed to be the object of it? what is the crime—who the guilty individuals that have originated this strong feeling in the public mind? Assuredly not the profession: the lowest rabble in the community are not so stupidly insane as to impute the dark atrocities of the burking system to the practitioners of physic: they are not so destitute of the least spark of reason as to be unable to draw a line of distinction

between the most unnatural of murderers and the benevolent alleviators of their infirmities. The wants of the profession are far better understood by the lowest vulgar than the interested preachers of *excitement* are willing to admit, and we are confident, that if the present opportunity of legislating be allowed to slip by, without making the requisite provision for the interests of science, government will be accountable for a most serious oversight, the public will remain exposed to the most grievous calamities, and the profession will certainly—not be the losers.

But Lord Brougham is for delay. Now what is more easily accounted for than this disposition of his lordship? At the present juncture, the state of public business is arrived at its height and most pressing urgency: along with the alarm naturally excited by the advancing ravages of a new pestilence, the necessity of deeply weighing those great political changes which are forced upon their consideration, devolves a heavy duty upon his Majesty's ministers, and upon none more imperatively than the noble lord on the woolsack. In a parliament assembled unusually early for the despatch of business of great national moment, the introduction of a *new* measure is any thing but welcome; however urgent, it is permitted to divert attention with reluctance, and the possibility of any reasonable excuse or delay is eagerly grasped at. Mr. Warburton may be very much at leisure—*degagé*, as he has been for the last two sessions, from the plague and annoyance of getting up a new bill for anatomy. The Lord Chancellor, however, is placed in quite another predicament; any new bill for regulating the schools must throw the main responsibility upon government; and we all know the extreme trust reposed in the exalted individual in question, both by his Royal master and his noble colleagues in the cabinet. To gain time then, rather

* Lord Chancellor's speech in the House of Lords (Thursday, 8th) on Lord Harrowby's motion that the petition of the Hunterian society be read.

than to eschew any prevailing excitement, may not unreasonably be conceived to be the real object of his lordship. As an individual, when he was plain Mr. Brougham, and that not above three years ago, no man could have shewn himself more sensibly impressed with the difficulties which impeded the study of anatomy, and the importance of having those difficulties removed. In that trial at Lancaster, in which Messrs. Davis and Blundell were fined and put to serious expense, for "having in their possession a body which had been disinterred for the purposes of *illegal and indecent* dissection," Mr. Brougham, who was for the defendants, displayed a thorough knowledge of the bearings of the subject, and set before the jury, in the clearest light, how "without dissection, anatomical and surgical studies could not be carried on; how professorships of anatomy had been established by the crown in our universities; how those professorships were useless without dissection; and how dissection could not be carried on without a supply of dead bodies." The climax was complete. But the law—the law—took its course; and the learned Baron (Hullock) who presided, stuck to the very letter of it:—"However necessary it might be," said his Lordship, "for the purposes of humanity and science, that *these things*" (his Lordship means dissection) "should be *done*, yet, as long as *the law* remained as it was at present, the having of bodies for dissection was an offence *liable to punishment*."

It will be a difficult matter, no doubt, to please all parties, with whatever bill Mr. Warburton may please to provide. His last had many faults, some of them of too obvious a nature to be retained; but yet the principle, we will maintain, was unexceptionable; it went to the removal of absurd and fatal inconsistencies from our statute books; and though the remedy was not as complete as the

necessity of the case required, the putting forth the hand to the cure was good, and gave more than a mere promise of success. Many symptoms of conversion too, we are happy to state, have been manifested in certain high quarters, since the last discussion of the bill in the House of Lords; and Lord Tenterden is not the only example among the lay peers, who has expressed his willingness to support a *new* measure, introduced with certain equitable conditions; while the Right Reverend Bench are said to have modified greatly their repugnance to the question. The Bishop of London will give it his decided support.

LAST night (Thursday, 15th) Mr. Warburton obtained leave to bring in his new bill. From what fell from him on the occasion, we learn that the present measure is to be *more simple* than the last; that instead of licenses being required, inspectors, appointed by the Home Secretary, are to visit the schools and make returns; and that, instead of drawing a line of distinction between the rich and the poor, "*nobody*, under the provisions of the new bill, is to be given for dissection without the consent of the person when alive, or of his nearest relative after death." We have not room for remark.

DRS. RUSSEL AND BARRY.

WE understand that Dr. Russell is to be created a baronet, and that Dr. Barry is to receive the honour of knighthood, with promotion.

LIGATURE OF THE SUBCLAVIAN ARTERY.

THIS, certainly one of the most difficult operations in surgery, was performed by Mr. Brodie last Tuesday, on a patient at St. George's Hospital. The case is one of subclavian or axillary aneurism—for the disease extends very low down the vessel—occurring in a man about fifty. The external incision was made pretty free, and great care taken, in the subsequent dissection, to interfere with the nerves as little as pos-

sible; a point to which Mr. Brodie has been led to attach considerable importance. Considerable dexterity was required in detaching the vessel from the surrounding parts, as the artery was enlarged, and there was a good deal of thickening in its vicinity. The end in view, however, was fully accomplished, and we need scarcely say with the utmost skill. We shall give the details when the result is known.

DISSECTION OF BISHOP.—PHENOMENA ATTENDING DEATH BY STRANGULATION.

On the occasion of examining the body of Bishop, at King's College, Dr. Watson, the Professor of Forensic Medicine, stated, that he had been requested by his colleagues to make some observations on the phenomena presented by certain modes of death, but more particularly by suffocation. Although he could not hesitate in complying with their request, yet, being thus suddenly called upon, he must bespeak their indulgence for appearing before them for the first time in a manner so unprepared.

Dr. Watson then alluded to the infrequency of opportunities of examining cases of strangulation in the human subject, with an accurate knowledge of the precise mode and time at which death had taken place; and remarked, that the information obtained under such circumstances was often of much use in throwing light upon other cases, of a doubtful nature; with regard to which, medical men were frequently called upon to give evidence. He then proceeded to speak of death by *suffocation*—a term which, used generally, he preferred to asphyxia, as this last meant literally absence of pulse, and therefore was most applicable to death beginning at the heart. Suffocation, it might be observed, presented several varieties; such as that produced by suspension, by submersion, and by the inhalation of gases destitute of oxygen. He remarked that the older physiologists entertained erroneous ideas as to the cause of death when the supply of air was cut off. Haller, for instance, conceived that the blood, under such circumstances, was no longer able to pass through the lungs, and that the circu-

lation in these organs was arrested by the mechanical condition of their vessels. Goodwyn made a step towards the truth, by shewing that the blood continued to pass through these organs; but he imagined that, arriving at the left side of the heart, destitute of its usual stimulating properties, this refused to contract upon it, and thus ceased to transmit it to the brain and other parts; death then resulted from syncope, the brain and heart being simultaneously affected. It was reserved for Bichat to demonstrate the exact mode of death in such cases. He, by very simple but convincing experiments (which Dr. Watson briefly detailed), was able to prove that the blood continued to be propelled by the heart, though it had lost its arterial properties. Thus coming into contact with the brain, it was first destructive of the functions of animal life; and being circulated also over the whole body, including the structure of the heart itself, it acted as a poison on the various parts, till at length the organic life yielded to its pernicious influence; death, in fact, ensuing from the circulation of venous blood in all parts of the body, which produced its effects by killing first the brain and nervous system, and subsequently the heart and muscular system; the insensibility beginning as soon as the dark blood arrived at the brain, but the heart continuing the circulation some time longer.

Dr. Watson next proceeded to inquire what evidence was to be derived from the appearance of the body, as to whether an individual had been destroyed by suffocation. Various criteria had been mentioned by different authors, but these were, for the most part, very uncertain and inconclusive. Among these were, the body remaining long warm and pliant; partial contractions, as of the hands or feet; the appearance of spots upon the surface, &c.; even the appearances about the face of lividity and tumefaction, with projection of the eyes, though of more importance, were nevertheless not to be depended upon, inasmuch as they are occasionally absent. Upon the whole, the most important phenomenon was the congested state of the venous system generally, but more particularly of the lungs, of the left cavities of the heart, and of the brain. A circumstance, however, which prevented such appearances from

being conclusive of suffocation, was, that they were met with in cases of death beginning at the head, as in coma; in which cases also the circulation of black blood took place, but which was then to be regarded as the effect, not the cause, of the cessation of animal life. The appearances of congestion, however, were not to be taken as conclusive, because it had been shewn by Bichat that they were generally absent in those in whom the suffocation had been rapidly effected; as he had ascertained in examining the bodies of some persons who had been suffocated by the fumes of a furnace, and of two who had been hanged. He also established the same principle by experiments, in which he found that venous congestion did not shew itself when animals were suddenly suffocated, while it was met with in others in whom asphyxia was slowly produced, as by confining them in a limited portion of air, which gradually became deteriorated, till it was no longer capable of supporting life.

Morgagni was aware of these facts, and has offered what appears a satisfactory explanation of them. He states, that the blood in those suddenly cut off, does not coagulate, but, remaining fluid, becomes distributed after death, although congestion in the venous system *has been* present. Thus he opened the chest of a person who had been hanged, about two hours after his death, and found the lungs and right cavities of the heart gorged with blood; the right auricle, in particular, was prodigiously distended, and seemed ready to burst; but on looking at the body next day, these appearances were entirely gone: the vessels not having been opened, and the blood which remained fluid having found its way by various channels, had left the heart comparatively empty and flaccid.

Dr. Watson then remarked that considerable difference of opinion had existed as to the exact mode in which death was produced by hanging. Some held, and he believed correctly, that it depended upon the circulation of black blood through all parts of the system; others, again, regarded it as the effect of apoplexy, produced by the pressure of the rope on the veins of the neck; and yet others sought for the cause of death in the fracture of the spine, and consequent injury to the spinal cord, which sometimes took place. He

stated it to be his opinion, however, that though one or both of the latter circumstances might concur with the circulation of venous blood, and might even be conceived sometimes to anticipate the slower death from this, yet that generally they were to be looked upon only as secondary and accidental causes. In corroboration of this opinion, he mentioned the experiment which had been made by Mr. Brodie, amongst others, in which a dog had been suspended for a considerable time, but an aperture having been made in the trachea, lower down than the rope, the animal continued to live. The external pressure doubtless has a tendency to produce insensibility, as is shewn by compressing the neck by way of experiment: besides which, he adduced the case of a patient, brought to Middlesex hospital, who had attempted to hang himself: he was cut down while yet alive, but continued insensible, though several hours elapsed before death took place. He also referred to a case which occurred some years ago in Edinburgh, where a felon had been cut down by the rabble, and remained nearly an hour in a state of insensibility, but still living, until the execution was completed by the proper authorities.

Dr. Watson was anxious to press upon his hearers that, as shewn by Bichat, an interval occurred between the cessation of animal life and of the organic functions, so that the heart continued to act after sensibility had ceased, and that this interval was of great importance, inasmuch as, by artificial respiration, individuals might possibly at this period be restored. From the rapidity with which consciousness was destroyed, it was evident that suffocation was one of the easiest modes of death, and it was probable that hanging was particularly so; as the external pressure co-operated with the other circumstances in producing insensibility. There was reason to believe that the sufferings of the individual were entirely at an end as soon as black blood reached the brain (a period generally marked by the occurrence of convulsions), although life was not yet extinct, as the circulation continued for some time longer. The idea of the comparative easiness of this mode of death was in conformity with the account of their feelings given by all those who had been restored after suffocation. Many such cases are on

record, all very much resembling each other. Morgagni, for instance, states that a person who had thus been resuscitated told him, that he had felt no pain, but that he had perceived flashes of light before his eyes, and speedily passed into insensibility. When Dr. Watson was at Edinburgh, a man was brought into the Infirmary who had attempted to hang himself. His account was, that he had seen a dazzling brightness before his eyes—that the windows of the room appeared larger and more numerous than natural; after which he “forgot himself.” Lord Bacon, in his *Historia Vitæ et Mortis*, gives an account of a friend of his, who, being desirous of ascertaining what were the sensations of those who were hanged, suspended himself, thinking he should be able to regain at pleasure a stool on which he had been standing; but he soon became insensible, and would have died had not some one come to his assistance; and Dr. Watson remarked, that probably most of his hearers might be able to call to mind instances where this sort of experiment had come to a fatal termination—analogous accidents being from time to time recorded in the newspapers. As Lord Bacon’s account was quaint and short, while it afforded a fair sample of such cases, he quoted his words:—

“*Memini me accepisse de Generoso quodam qui lubricundus ex curiositate desiderabat scire qualia paterentur in patibulo suspensi; sesèque suspendit, super scabellum se alleuans, et deinde se demittens; putans etiam penes se futurum, ut scabellum pro arbitrio suo, recuperaret id quod facere non potuit; sed tamen ab amico præsentè adjutus est; ille interrogatus quid passus esset? retulit se dolorem non sensisse; sed primo obseruansatam sibi fuisse circa oculos speciem ignis et incendii; deinde extremæ nigredinis sive tenebrarum; postremo coloris ejusdam cærulei pallidioris sive thalassini; qualis etiam conspicietur sæpè animo linquentibus.*”

The Professor next spoke of the special marks to be expected on the body after hanging. 1st. A depression corresponding to the size and position of the rope, generally with ecchymosis extending more or less into the subjacent parts. Where this mark was strictly horizontal, it afforded proof that the strangulation had not been produced by *hanging*.

Fracture of the spine Dr. Watson stated to be less common than was generally imagined, and its presence had been thought to shew that the individual could not have committed suicide; but if the person had thrown himself from any height, especially with a long rope, it was quite evident that the occurrence in question might take place. He next adverted to the appearance of the face, which was generally purple or livid and swollen, with the eyes blood-shot and staring. Sometimes the tongue protruded, but this would depend upon the spot at which the rope was applied, for if it encircled the neck high up, *as in the case before them*, it necessarily pulled the os hyoides and parts attached to it backwards. Shakspeare has well described the ordinary appearances under such circumstances:—

But, see; his face is black and full of blood,
His eye-balls farther out than when he lived,
Staring full ghastly—like a strangled man.

He pointed out, however, as necessary to be kept in mind, that these appearances of turgescence and lividity of the lips and countenance, even when present at first, were likely to disappear when the blood remained fluid, particularly if the head had been in an elevated position. Dr. Watson also alluded to the occasional erection of the penis, described as occurring in such cases. With regard to the internal appearances, they were principally those of venous congestion, in the head, chest, and abdomen. Dr. Watson particularly mentioned the cases related by Dr. Yelloly, wherein he had found in the stomachs of five criminals, who had been hanged, great congestion in all, while in two, there was blood extravasated and coagulated upon the membrane itself. These cases he thought extremely important, as shewing the danger of mistaking such appearances for inflammation produced either by poison or otherwise.

Mr. Mayo then proceeded to examine the body.

The integuments of the face and head loaded with blood; some tumefaction and lividity of face, but not so much as there had been just after the execution; tongue not protruded; mark of the rope just under the chin. Part exposed by dividing the scalp excessively gorged with blood; some appearance of ecchymosis. On removing the skull-cap, little appearance of congestion; slight serous effusion beneath the arachnoid;

numerous bloody points on slicing the brain; a little water in the ventricles; surface of cerebellum gorged; perhaps a little extravasation. Lungs and investing membrane perfectly healthy; back part of these organs loaded with blood, but not more than large; cavities flaccid; blood perfectly fluid, running into large vessels when the heart was raised, and back again on letting it fall. No particular congestion about the stomach. Colon much-distended with flatus. Other appearances natural.

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TRANSFUSION OF BLOOD TRIED IN CHOLERA—ITS FAILURE.

At a late meeting of the Academy of Science, in Paris, M. Arago read a letter from Professor Scoutetten, of Metz, on the transfusion of blood in cholera. We extract the following passages:—

I have the honour of addressing to you some observations, which I have made at Berlin, on the transfusion of blood: the facts speak for themselves: be good enough to read them at the next meeting of the Institute; they may be useful to science and humanity, and perhaps prevent some unfortunate patient from being the victim of new experiments. The complete absence of blood in the arteries of the limbs appears to me a very remarkable fact, and the more astonishing that some lived in this state during five or six days, the capillary circulation being restored, with the heat of the limbs, without the pulse being perceptible. I have numerous facts of this kind, particularly the history of a Pole, who was able to walk about without the pulse having returned. He died suddenly, when it was least expected; and this was the result in all those cases of which I speak.

CASE I.—After having tried all the ordinary means in a case of cholera, it was thought right to have recourse to transfusion of blood. October 15th the operation was tried for the first time, by M. Dreffebach, in the hospital of M. Boehr. The patient, Frederick Muller, of good constitution, aged 27, had been ill seven hours and a quarter, and he was in the following state. Eyes half shut, sunk in the sockets, ball turned upwards; nostrils pinched; cheeks hollow; mouth par-

tially open; tongue cold, as well as the face; respiration short and quick; blueness of hands and feet; complete absence of pulse; skin of fingers shrivelled. The patient, however, retained perfect consciousness of what was passing around him. The right jugular vein was laid bare to the extent of an inch, and opened in a longitudinal direction, and the barrel of a quill introduced into it. Blood was furnished from the median vein of a robust young medical man. It was received into a small brass syringe previously warmed, and injected into the vein of the patient to the extent of an inch and a half. This was followed by almost complete insensibility; after which the patient made two deep inspirations in succession; the eyelids were rapidly opened, and again shut; in five minutes there were convulsive movements of the head, which was drawn powerfully backwards; next followed convulsions of the legs, arms, and trunk, alteration of the features, plaintive cries, and groans. These symptoms lasted less than a minute, then ceased all at once, and the patient was dead. It is said that no appearances different from usual were found on opening the body.

CASE II.—The same day, transfusion was tried on a female, named Veber, aged 65. She was taken ill during the night; the operation was had recourse to at ten o'clock next day. The following is an account of her condition. Eyes sunk, and surrounded by a brownish circle; cheeks hollow; bones of face prominent; tongue hard, and feels cold; complete absence of the pulse; vomiting and purging rare; consciousness entire. The only remedy used had been a vapour-bath.

M. Dreffebach proceeded to the transfusion. The median vein of the left arm was opened to the extent of half an inch: very little blood flowed from it. A quill was inserted, and blood furnished by a student 23 years of age. The first injection, of an ounce of blood, produced no effect; the same quantity was thrown in a second time. The patient then made two rather hurried inspirations; the eyes were somewhat agitated. She had some mint-tea, and drank it easily. I asked her if she suffered, and she answered, "No." The operator wishing to introduce more blood, opened the left jugular vein, into which he injected about a drachm

of tepid water, to be assured that there was no obstacle. He then threw in, at two injections, two ounces and seven drachms of blood. The patient felt nothing. The day was passed tranquilly; the pulse did not return. The usual symptoms went on, and she died six hours after the operation.

CASE III.—A man, aged 61, seized with cholera, was admitted into the hospital on the 16th of October. All the symptoms were well marked. The attack had come on about midnight. At ten o'clock in the morning the operation of transfusion was decided on, but, before putting it in practice, it was resolved to ascertain whether the circulation did or did not go on; with which view M. Dreffebach laid bare the brachial artery, to the extent of an inch, at the lower part of the arm. The vessel did not exhibit the slightest pulsation. An opening, five lines long, was then made in it, when it was found not to contain one drop of blood: having within only a small red clot, not larger than a sewing-thread. The parietes of the artery were clean and white. The patient, meantime, was perfectly sensible, spoke of the operation, and answered with precision all the questions put to him. The deep-seated textures were as cold as the surface; the veins of the fore-arm were filled with black blood; the median was opened, and two ounces and a half of blood were injected at three times. The patient felt nothing; complained of no pain, except very slightly of the wound made to expose the artery. After the third injection the pulse returned in the axillary artery of the fore-arm; it beat sixty times in the minute, but this only lasted five minutes. Even then, not a drop of blood escaped by the wounded artery. Some appearance of contraction of the iris, it was thought, was to be perceived during the transfusion, and the expression was a little more animated. The patient died two hours after the operation, which seemed to have had no influence on the course of the disease.

for Wednesday the 14th, to take into consideration the petition to be addressed to parliament on the subject of anatomy.

After which the question of the nature of malignant cholera, particularly with reference to its identity with the disease in England, was taken up by

MR. SEARLE, who read the following paper. Having stated it to be his opinion, that "cholera spasmodica and cholera morbus were intrinsically the same, but not identically so," he held the cause of the former to be a poison of the sedative class, operating immediately on the brain and nervous system. Its *modus operandi* he illustrated in the following manner, having premised that the action of the capillary vessels was independent of the heart and brain, the only essential stimulus to their function being the presence of duly arterialized blood. The poison causing cholera, he regarded as a miasm similar to, if not identical with malaria, and this he conceived to be received into the lungs by the act of respiration, and thence to contaminate the blood; its effects being particularly directed to the general capillaries of the system, destroying their functions—viz. nutrition, secretion, calorification. From this follows primarily, the languor, indigestion, giddiness, &c. and secondarily the purging, vomiting, coldness, spasms, &c. From the progressive depreciation of the functions implicating the respiratory process, the blood becomes surcharged with carbon, from which the evil becomes extended, causing the lividity of surface; and the numbness, pain, and cramps, are produced by distention of the veins of the spinal marrow at the origin of the nerves. Such is a general outline of the extended detail into which Mr. Searle entered in explanation of the cause of Indian cholera, and its mode of operation. He had no doubt but that sporadic cases of the disease often occurred in this country, and this led him, in the second place, to inquire in what the above disease differed from ordinary European cholera? "They differ little (said Mr. Searle) but in intensity of disease, as dependent upon the difference and virulence of the cause which gives rise to it, as we see illustrated in the operation of various other poisons of the sedative class upon the system;" and "it is to this difference of cause, or character of the poison giving rise to these analogous affections, to which I am of opinion the specific difference in the character of the disease may be fairly attributed." The poison producing the Indian cholera, Mr. Searle stated, as above, was applied from without; the cause of ordinary cholera, he believed to be "a poison generated within;" or that it resulted from the blood's "imperfect decarburation." This, again, being the result of the biliary derangements resulting from heat, cold also, by causing "cuticular capil-

WESTMINSTER MEDICAL SOCIETY,

Saturday, Dec. 10, 1831.

DR. STEWART IN THE CHAIR.

AFTER some discussion, it was agreed that a special general meeting should be called,

lary torpor," was pointed out among the agents capable to produce cholera morbus; and this cause co-operating with exposure to malaria, was stated to be productive of an aggravated form of disease; in illustration of which he read an extract from his pamphlet, beginning at page 34.

In concluding (said Mr. Searle) this part of the subject, sir, I think I have made it appear that the cause of cholera, whether spasmodic or otherwise, is that of a sedative poisonous agency upon the system, in support of which opinion, and that malaria is the cause of the former species, or very frequently so, I would adduce the fact that at Berlin the post-mortem appearances of the choleric invariably exhibited an affection of the mucous membrane of the bowels, which I have since found, on reference to the London Fever Hospital, almost invariably to attend upon fevers which are generally acknowledged to originate in malaria. And I would adduce also the cases of the disease occurring at a school at Clapham, which were so much alike in character with the spasmodic cholera, and which there was demonstrative evidence were dependent upon a cause of the same kind; and that it is to the presence of accumulated carbon, resulting from the blood's defective oxygenation during the summer months, acting upon the system as a poison, that cholera morbus is to be attributed, may, I am of opinion, fairly be inferred, by the fact of other poisonous agents, of the same character, producing analogous effects upon the system; as I have frequently witnessed in India, in persons who have partaken of oysters, which it may be supposed were not perfectly fresh; and the same thing Dr. Prout mentioned to me he had witnessed in all the members of a family who had eaten of a turkey which it was supposed could not have been fresh; and I have the authority of Sir Astley Cooper for saying that the effluvium of the dissecting room, when he has had a putrid subject, invariably produces the same effect with him. The opinions, sir, I have now had the honour to communicate to this Society, although not hastily formed, have been hastily committed to paper; which I hope will plead my apologies for their being not quite so perspicuously worded as I could have desired.

After the conclusion of Mr. Searle's paper, a very long pause ensued. At length

Dr. MACLEOD rose and said, that he offered himself to the notice of the society with great reluctance; but, as the pause was becoming quite formidable, he would offer a few remarks on the paper, rather than suffer the silence to be protracted still longer. He was afraid that he had not clearly understood Mr. Searle, as that gentleman had begun by holding, that the Indian and English cholera were "intrinsically the same," while

he had afterwards gone on to prove that they were "specifically different," and this appeared to him to be a contradiction in terms. That there was great resemblance between certain cases of the Indian disease and that which occurred in England, was quite evident; had it not been so, the same name would never have been applied to both. Yet, admitting this, he still thought, that, viewing the two diseases, each as a whole, sufficient discrepancy might be observed to show, that they were not the same. Thus, in English cholera, vomiting and purging, with cramps, formed the assemblage of symptoms characterizing the more severe cases; in Indian cholera, not only might all these be absent, but their absence actually marked the most severe form of the disease. In the English disease, as a general rule, bile presented itself in the evacuations; in the Indian cholera, the absence of bile was one of the peculiarities of the disease. In the former, the secretions of the liver and alimentary canal were increased; in the latter, they were arrested, the matters passed being merely certain constituents of the blood, apparently suffered to exude from the vessels without undergoing the changes we recognize in secretions. Still it was held by some that these differences depended upon different degrees of severity; but he doubted the accuracy of this, because he had seen patients die of English cholera—which, he supposed, was as strong a proof of its severity as any gentleman could desire—yet, in these cases the individuals did not die as they are said to do in the Indian disease; they became weaker and weaker, under the continuance of the vomiting and purging, and at length sunk exhausted, as one might be supposed to do from hypercatharsis, or any other immoderate depletion; but they did not exhibit any shrivelled appearance, they did not become blue, nor shew any signs of defective arterialization of the blood, which appeared to be one of the great characters of the Indian disease. He was aware that others had met with cases in which some degree of this blueness was to be seen, and he admitted that it was the occurrence of these sporadic cases, so closely resembling the malignant disease, which constituted the greatest difficulty in deciding this question. But even admitting the great resemblance in extreme cases, still it remained to inquire, whether similarity of symptoms marked identity of disease. Mr. Searle had taken his illustration from the effect of narcotics; but adopting his own instance of opium and belladonna, he (Dr. M.) contended, that though there were certain points of resemblance, as to their effects on the nervous system, yet these by no means went so far as to constitute their actions "intrinsically the same." Besides, a man might be found in the street in a state of stupor,

and it might be impossible to say from his appearance whether he had fallen down in apoplexy or had received a blow on the head, or was only very particularly drunk; yet no one looked upon a fit of apoplexy, a blow on the head, and intoxication, as "intrinsically the same." Again, a female might have convulsive paroxysms, of which no one, regarding the present appearances merely, might be able to say whether they were epileptic or hysterical; while it was farther notorious, that tetanus and hydrophobia frequently resembled each other so closely as to have been actually confounded; and this last instance bore directly on the point at issue, because the former, like English cholera, was produced from accidental causes; the latter, like the malignant disease, arose from a peculiar poison. The speaker inferred from all this, that similarity in certain symptoms was not sufficient to show individuality of disease. But if they who founded the doctrine of the identity of English with malignant cholera on their resemblance, did not on this point prove enough, there was another on which they proved too much, for some of them also held the new cholera to be the cold stage of a malignant fever. Now he was far from objecting to this doctrine; but as it was an admitted axiom, that "things which are equal to the same, are equal to one another," so, if English cholera be equal to Indian cholera, and if malignant fever be equal to Indian cholera, English cholera must be equal to malignant fever! He by no means objected to the comparison of the new disease to fever, because he thought it the more correct view; but by how much the more any one succeeded in establishing this doctrine, by so much the more did he disprove the former part of the proposition, because any one who had seen common English cholera must perceive that the attempt to prove it to be the same as malignant fever, was an *argumentum ad absurdum*.

Dr. JAMES JOHNSON objected to the presence or absence of bile as forming any diagnostic mark of English cholera; bile was not generally present in the disease of this country, at least it was not so at the commencement, but was produced by the succussions of the vomiting. He held that the vomiting of bile in the cholera morbus was only a ternary link in the chain of phenomena. The contents of the stomach were first discharged, then the mucous secretions of that organ, and lastly bile. He remarked farther, that there was something superadded to the causes of common cholera, which rendered that disease epidemic; but what that something was he (Dr. J.) did not pretend to know.

Dr. GREGORY rose to animadvert upon two points touched upon by the preceding speaker. Dr. J. Johnson had then reiterated a sentiment on which he seemed to lay

great stress—the absence of bile in the first discharges from the stomach in cholera morbus. It was Dr. Johnson's object to shew, that the vomiting of bile, when it *did* occur, was merely the result of the continued vomiting, and that bile *never* appeared in the first discharges from the stomach. This position Dr. G. would by no means consent to admit. In common with many other members of the society, he had seen numerous instances of the ordinary cholera of this country, and he was perfectly satisfied that bile was often seen in the very first discharges from the stomach. He was ready to concede to Dr. J. that these first discharges consisted of the *contents of the stomach*; so did the second; so did the third and the fourth; but the question was, whether bile is not often in the stomach *prior* to the first attack of vomiting? Dr. G. was firmly convinced that it was; and that nausea and headache are often evidences of such an occurrence. How often do we find persons bringing off bile—pure bile, from the stomach, by a single effort of vomiting; and if this can happen independent of cholera, why may it not happen in that disease? [Dr. J. Johnson here expressed some dissent.] Dr. Gregory added, that he spoke not of *possibilities*, but of every-day occurrences, and he left it to the members of the society to judge between them. Dr. G. then proceeded to comment on that part of Dr. Johnson's address which referred to the obscurity in which the causes of epidemic cholera was involved. To this sentiment *in itself* Dr. G. had nothing to object. Whatever notions he himself entertained on the origin of this disease, he was quite ready to admit the *obscurity* in which the subject was involved; but he was astonished that Dr. Johnson should thus profess his ignorance of that *something* which rendered the cholera epidemic, when, in the first of those nineteen propositions with which he had favoured the society, the whole matter was explained. Dr. J. Johnson's words in proposition 1st, are—"In epidemic cholera, a poisonous, or sedative principle, originating in the earth, or emanating from animal or vegetable substances, strikes a predisposed individual, and produces the disease." On numerous occasions Dr. Johnson had advocated the *terrestrial* origin of cholera; and he (Dr. G. G.) could not understand with what consistency he now maintained that the causes of epidemic cholera were beyond his knowledge. He had appealed to the new volcano in the Mediterranean in proof of the reality of such terrestrial changes as were adequate to the explanation of the origin of cholera. This theory he (Dr. G.) discredited altogether; he could not see any plausibility in the supposed connexion of cholera with terrestrial emanations; and if it depended upon subterranean heat, it would be incumbent on Dr. Johnson to prove the springing

up of a new volcano every year since 1817. Be this, however, as it might, we either did know the sources of cholera, or we did not. The observations which had just fallen from Dr. Johnson, when compared with his written statement, left it in doubt in which horn of the dilemma he desired to be placed. Dr. G. next proceeded to offer some remarks on the proposal of Dr. O'Shaughnessy, to inject oxygenating salts into the blood, as a remedy in the Asiatic cholera; but upon learning that that gentleman had gone to Sunderland for the purpose of prosecuting his researches, he desisted.

DR. GRANVILLE begged to put a question, through the Chairman, to Dr. Barry, whom he saw in the room—namely, “Whether the common autumnal, or English cholera, and the disease which he had seen in Russia, were identical?” Thus called upon, and amid the manifestation of much anxiety on the part of the meeting to hear his opinions,

DR. BARRY rose, and after acknowledging the compliment paid him, proceeded to state, in the strongest terms, his conviction that the diseases were different. He remarked that the essential characters of English cholera were often entirely absent in the other—in which patients frequently died in a few hours, and without having vomited or been purged at all; but that they were in a manner stricken down at once, and exhibited more the appearances of a corpse than a living being; with the eyes sunk into the sockets, the skin dark as if from nitrate of silver, the toes and fingers shrivelled, and the tendons standing out like rigid cords along the limbs; while the very breath was cold, and the pulse scarcely to be felt, or not to be felt at all; and this led him to observe, that the disease he had seen in Russia, and more lately in Sunderland, bore much more analogy to the cold stage of a malignant fever than to ordinary cholera; and, indeed, it was thus further different from it, that, to be successful, you had to save your patient twice—first from the blue or collapsed stage, and then from the typhus fever which succeeds. He strongly expressed his conviction that neither Celsus nor Sydenham had ever seen the disease which had lately attracted such fearful interest, because, if they had done so, it was not to be conceived that they would have omitted to mention the blue colour—the corrugation and shrivelling of the extremities—the coldness of the tongue and breath, or the alteration of the voice; yet not one of these things had either of them enumerated. Dr. Barry then proceeded to speak of the anatomical characters, the most constant of which, he said, were distention of the gall-bladder, with a most peculiar shrinking and emptiness of the urinary bladder, which more resembled a dried fig than any thing else, and was scarcely larger. He also particularly dwelt upon the phlogosed state of the spinal marrow, and its investing mem-

branes, to which, in some cases, was super-added distinct ramollissement. The observation of this circumstance had led Dr. Lange, of Cronstadt, to try the use of the actual cautery to the spine, opposite the part where the inflamed or softened appearances presented themselves. This was done in fourteen cases, twelve of which recovered; and this too at the commencement of the epidemic, the very time at which experience had shewn it was violent and intractable. In several of these cases the patients were so much satisfied of the relief afforded, that they begged for the repetition of the operation; but such were the prejudices of the people against the medical men—such their persuasion that the disease was produced by poison, and that this method was a kind of torture by burning, that it was found necessary to discontinue it. Vomiting and purging, he asserted, were by no means the cause of death in the Russian disease; the paralysis of the heart—the thickening and stasis of the blood—the embarrassed and altered respiration—were the fatal alterations of function which hurried on the fatal event, and in many cases with such dreadful rapidity.

DR. COPLAND called the attention of the society to the circumstance of his having, some years ago, as well as recently, advocated doctrines similar to those so ably supported by Dr. Barry.

A gentleman, whose name we did not learn, spoke in favour of the identity of English with Indian cholera.

MR. SEARLE made some observations in farther elucidation of his views. We understood him to say, that he regarded the disease as modified by climate, being intrinsically but not identically the same. With regard to the appearances of the spinal marrow, alluded to by Dr. Barry, he had often seen the vessels of the meninges of the brain very much loaded, and with serosity between the membranes; these appearances often extended down into the spinal canal; but nothing like softening was to be found in recent cases; and he regarded the affection as much too general to be cured by the local application of the cautery, as recommended by Dr. Barry.

—
The proposed meeting took place on Wednesday, and was adjourned without any decision having been come to regarding the anatomical petition.

NOTE FROM MR. KING.

To the Editor of the London Medical Gazette.
SIR,

IN your report of the debate at the last meeting of the Westminster Medical Society, there is an important omission with respect to the treatment adopted in the case of cholera which I related; and there is also a charge of “vociferating” brought against me.

Have the goodness to supply the former, and be assured I am not conscious of having deserved the latter. I said, that the serious symptoms in the case alluded to were checked, not merely by the application of a large mustard poultice to the abdomen, but by this means, combined with the administration of very small doses of the Tinct. Opii.

I remain,

Your very obedient servant,

THOMAS KING.

10, Hanover-Street, Hanover-Square,
December 13, 1831.

MEDICO-CHIRURGICAL SOCIETY.

Tuesday, Dec. 13, 1831.

MR. LAWRENCE IN THE CHAIR.

Fracture of the Spine—Tumors.

A PAPER by Mr. Barlow, giving an account of a case of fracture of the spine, was read. The principal feature in it was in the treatment, and that was by, in the first instance, restoring the fractured and projecting parts of the vertebrae to their proper position, by means of extending the upper and lower extremities at the same time—which gave no pain, but rather great relief; and, with the aid of mechanical contrivances for supporting and moving the body, enabled the patient to sustain life, under circumstances of considerable promise and amelioration, for upwards of twelve months. His urine he was able to evacuate by pressure on the hypogastric region. The body was carefully examined after death, and much softening of the medulla spinalis was clearly observable opposite the fracture. Drawings and preparations of the bones were exhibited to the society by Mr. Langstaff, who had communicated the paper.

MR. STANLEY considered the treatment in this case rather unjustifiable; he did not see, notwithstanding the favourable circumstances which ensued, that the method of forcibly extending the extremities was consistent with the principles of judicious management. He was surprised too at the little attention which seemed to have been bestowed on, or required by, the urinary apparatus. In a case of a similar nature which had occurred to his observation in St. Bartholomew's, a material cause of death was disease of the bladder: an ammoniacal state of the urine very early discovered itself, and in spite of all the care that could be given it, by the introduction of the catheter two and often three times a day for the last three weeks, symptoms of inflammation of the bladder supervened, and, upon examination after death, considerable sloughing was observed.

MR. LANGSTAFF presented and gave explanations of various dry and wet specimens of anchyloses, and diseased growths of bone.

He submitted to the society the proposition—that no ankylosis is ever effected in a scrofulous state of the bones; at least he, with very extensive opportunities of observation, had never found it to be the case; and he wished any gentleman present, that knew an instance to the contrary, to be so good as to come forward and state it.

Nobody gainsaying the proposition, the second part of a paper by Mr. Lawrence, on tumors, was read by the Secretary.

The relation between the tumors of which this part of the paper treated, and the pancreatic sarcoma of Mr. Abernethy, was pointed out. The tumors in question were situated for the most part about the localities of the submaxillary structures, and generally close to the mastoid process; and it might be observed of them, that they seemed invariably to partake of the nature of the tissues in the neighbourhood of which they were situated; those, for example, in the vicinity of the salivary glands bearing an analogy in their structure to those glands. A small quantity of bony deposition was also sometimes detected in them. The knife was the most speedy and effectual remedy in these cases. The paper then went on to give an account of five or six examples of the occurrence of these tumors; one in a young man, in whom the new growth attained the size of a walnut, in the angle of the jaw, close by the mastoid process, and involving the parotid gland. In the operation for its removal much hæmorrhage ensued, and a portion of the facial nerve was unavoidably cut away; the cure, however, was complete. A tumor of a similar description, and similarly situated, but of the size of a hen's egg, was removed from a woman, by a single incision. Another growth of the same character, but formidably larger, gave Mr. Lawrence much more trouble. He was obliged to cut through it in the removal; and it was seen that a considerable cavity existed in the centre, containing a fluid which seemed yellowish, but the exact composition of which could not be ascertained, owing to its mixing with the blood which flowed copiously at the time. After cutting out the remainder of the tumor, Mr. L. found it necessary to get rid of a quantity of blackish tissue which composed the nidus of the tumor, about the parotid, by breaking it down; the external carotid was completely bared in this operation. All the cases related by Mr. Lawrence were attended with successful results; the last of them was one in which a young lady, who had an ugly tumor, the size of a walnut, on her upper lip, near the angle of the mouth, submitted it to the knife; and it was readily removed from the inside, leaving the patient completely well in a day or two. The tumors in question, it was further observed, were all situated on the left side of the jaw, and none of them were of a malignant character.

DR. GORDON inquired whether any medical treatment had been tried in these cases previously to the use of the knife being adopted; and he wished particularly to know, whether iodine had been resorted to. He related the case of a woman who had a tumor removed, by an operation, from her breast, at one of the dispensaries, and upon returning some time after to the same establishment with another tumor of the same sort in the other breast, and declining to permit its removal by the knife, was induced to apply in another quarter, where the disease was treated with iodine, and with the most successful consequences. The whole tumor was not yet removed, but it was most remarkably reduced.

MR. LAWRENCE said that his cases were managed at a time when the use of iodine was scarcely known; but that various remedies had certainly been employed, such as leeches and constitutional treatment, though without any promise of success.

MR. LANGSTAFF did not think that iodine could have the least good effect in tumors, such as those described by Mr. Lawrence, any more than in growths of the nature of fungus hæmatodes.

MR. STANLEY thought that iodine was only efficacious in overgrown structures and enlargements of glands, as in bronchocoele. Before sitting down, he wished to call Mr. Lawrence's attention to a case of diseased mamma, on which an operation had recently been performed at St. Bartholomew's, for the removal of a tumor which he thought of a malignant kind. But what he wished particularly to notice, was the rapidity with which the wound, twelve inches in length, healed up—almost completely by the first intention; at least, there was not above a spoonful of matter discharged: upon this he begged to have the President's opinion, whether it was not a bad symptom, indicating the return of the disease in the part?—his own experience would induce him to conclude that it was; and he would further be glad to know, whether an issue in the neighbourhood of the part might not have a good effect in obviating a recurrence of a similar diseased growth?

MR. LAWRENCE did not think the tumor in question one of a malignant sort. As to the rapid healing of the wound, he thought it entirely owing to the constitutional healthiness of the woman, and not to any connexion with the supposed latent morbid structure. The issue he thought very advisable if a recurrence of the disorder was anticipated.

Some conversation ensued on the question—whether there really was any deficiency of bony matter in scrofulous patients; and whether, as was stated by some German surgeons, iodine was likely to conduce, in those cases, to the formation of osseous deposits? Adjourned.

DR. STEVENS IN ANSWER TO DR. JOHNSON.

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[TOGETHER with the following letter, some documents were sent for our perusal, which appear to us fully to bear out Dr. Stevens in his statements:—1st, That the plan of giving neutral salts, in a manner not previously practised, was adopted at Trinidad on his suggestion; and 2dly, that a great diminution in the rate of mortality followed thereupon. There may be difference of opinion as to how far the success depended upon the above, and how far upon other means; but the evidence must be very different from that contained in Mr. Greatrix's letter (the original of which we have seen), to warrant the imputations which Dr. Johnson states his correspondent to have thrown upon the honour and veracity of Dr. Stevens.]

To the Editor of the London Medical Gazette.

SIR,

I HAVE had reason to believe that, for some time past, reports similar to the accusations contained against me in Dr. Johnson's late letter, have been circulating in some of the private medical circles in this city, but in so cautious a manner that they were not tangible, for I found it impossible to trace them to their real source. With a belief that such a cause was in operation against me, I was not sorry when I saw the same accusations openly and boldly stated in a public journal. Unfounded attacks, like venomous reptiles, are only dangerous when they crawl in the dark; when they venture to shew themselves in open day, the one, like the other, can easily be crushed.

At the time that my paper was written for the Royal College of Physicians, I had a thorough conviction that every syllable I had stated in that document was most religiously true: I thought so then, and I must say (though I may be mistaken), that I think so still, even in the face of the bold assertions which Dr. Johnson has made in the last number of your journal.

I have written a statement of the facts relative to this subject, but for the moment, at least, I will defer sending it to the press. Were I to do so now, it is more than probable that it might be the means of preventing the publication of those valuable Trinidad documents, which contain such a mass of important information. But, independent of this, though Dr. Johnson has stated his charges distinctly enough, I am still left in the dark with respect to the real nature of the facts by which these charges are to be proved; when we know what the proofs are, we shall then be more able to judge of their value. Dr. Johnson, however, has promised to publish the said documents within fifteen days from the date of his letter. When this

is done I pledge myself, on the other hand, to lose no time in producing sufficient evidence to prove that all I have stated, and perhaps even a great deal more, is perfectly correct. By inserting this in the next number of your journal, you will oblige, sir,

Your most obedient servant,
W. M. STEVENS, M.D.

Dec. 14, 1831.

TREATMENT OF CHOLERA.

THE following official letter has been addressed to the Chairman of the different Medical Boards throughout the kingdom.

Central Board of Health, Council-Office,
Whitehall, Dec. 13, 1831.

Sir,—Agreeably to the intimation given by this Board in the concluding paragraph of their circular, dated 14th ult., I have the honour to transmit the subjoined “sanitary instructions for communities supposed to be actually attacked by spasmodic cholera,” with some observations on the nature and treatment of the disease, drawn up by Drs. Russell and Barry.

Every individual being deeply interested in the preservation of the public health, it is the bounden duty of all to endeavour to arrest the spread of disease at its very commencement. In order to attain this important object—

1st. The most efficient arrangements should be made by the local boards of health, and other authorities, to obtain the earliest and most correct intelligence of every suspicious case which may occur within their jurisdiction.

2d. All unnecessary communication should be prevented, as far as possible, between the infected and the healthy.

3d. As space, cleanliness, and pure air, are of the most vital consequence, both to the recovery of the sick person and to the safety of those about him, the patient labouring under spasmodic cholera should either be placed in a separate, well-ventilated apartment of his own house, if it afford such accommodation, and be attended by as few persons as the circumstances of his case will admit, or be induced to submit to an immediate removal to such building as may have been provided for the reception of persons whose circumstances will not afford the advantages at home, of space, air, and separation from the healthy.

4th. When an individual shall have been attacked with this disease, and placed under the most favourable conditions, as already pointed out, both for the recovery of his own and the safety of the public health; the room or apartment where he may have

been attacked, and from which he may have been removed, should be purified by scrubbing, lime-washing, free ventilation and fumigation by heated sulphuric acid and common salt, with black oxyde of manganese, or the same acid with nitre; or, when these materials cannot be obtained, by strong vinegar thrown upon heated bricks. The bed, bedding, and clothes, should be immersed in water, washed with soap, and afterwards fumigated as above.

5th. To correct all offensive smells, chloride of lime may be applied; but great caution is recommended in the use of this material, its fumes continued for any length of time having been found highly prejudicial to health—more particularly in delicate persons.

6th. A number of steady men, proportionate to the district in which they are to act, should be appointed to lime-wash and purify, as ordered above, under the direction of medical authority, such apartments as may be pointed out by the inspectors of the local board.

7th. Those who die of this disease should be buried as soon as possible, wrapped in cotton or linen cloth saturated with pitch, or coal tar, and be carried to the grave by the fewest possible number of persons. The funeral service to be performed in the open air.

8th. It is of the utmost importance to the public health that an improved diet, and flannel clothing, at least flannel belts and woollen stockings, should be given to the poor. No person should ever allow himself to sit down and get cool with wet feet: indeed the most particular attention should be paid to keeping the feet dry and warm. Repletion and indigestion should be guarded against; all raw vegetables, acescent, unwholesome food and drink avoided. Temperance should be most rigidly observed in every thing. In short, no means should be neglected which may tend to preserve individual health. The neglect of any or all of these cautions would not of themselves produce the specific disease called spasmodic cholera; but such neglect would most assuredly dispose the individual living in an infected atmosphere to be attacked by this disease, when most probably he might otherwise have escaped.

The most effectual means by which this disease may be prevented from extending, is to enable the poor, who are generally the first attacked, to oppose to its influence, as far as practicable, those ameliorations in diet, clothing, and lodging, which public and private charity will, it is hoped, not fail to produce.

Observations on the Nature and Treatment of the Disease, drawn up by Drs. Russell and Barry.

“Of the two great classes of functions

performed by the organs of which man is composed, one only is attacked in this disease. The operations of the senses and of the intelligence are either left untouched, or are affected but in a secondary manner.

Those functions, on the contrary, by which existence as a living being is preserved; those complicated powers, by means of which we are for ever appropriating and converting into a part of ourselves portions of the matter around us; are all and at once deranged by the attack of this terrible malady. Nutrition is annihilated; respiration becomes difficult, irregular, and inefficient; the involuntary muscles no longer perform their task; the voluntary are drawn into contractions by other powers than the will; the blood ceases to circulate; its physical properties are altered; its serous portion is suddenly thrown out upon the intestinal mucous surface of the body; the secretions are all arrested; and animal heat is no longer produced.

Under such rapidly destructive, and almost universal derangement of function, the most energetic efforts should be directed to reproduce what the disease has rendered nature unable to keep up—viz.

1st. Fluidity, heat, and motion in the blood.

2dly. Regulated action in the voluntary and involuntary muscles.

Lastly, but above every other consideration, renewed energy in the nervous centre, the source of all vitality and function.

No remedy at all approaching to the nature of a specific has been as yet discovered for this disease. In fact, no one mode of cure can be usefully employed under all the circumstances of any disease. The grades of intensity, and the grouping of the symptoms with which spasmodic cholera makes its attacks, vary with the conditions of the subject; its treatment, therefore, must vary with these grades and conditions.

The leading preliminary symptoms generally are, either diarrhœa, spasms, apoplectic vertigo, with nausea, imperfect vomiting or various combinations of these symptoms.

When the diarrhœa affords time for distinct treatment, it ought to be arrested at once by the most prompt and efficient measures;—by opium in moderate doses; astringents; local bleeding by leeches, if the subject be plethoric; by cordials and sulphate of quinine, if there be cold sweats; by confining the patient strictly to bed, and keeping up heat; by diet; by emetics.

Should spasms be the first and leading symptom, subnitrate of bismuth, cupping along the course of the spine, cordial, and antispasmodic medicines, opium, frictions, and dry warmth, are indicated.

But when the patient is suddenly seized with vertigo, nausea, coldness, loss of pulse, blueness of the skin, shrinking of the features

and extremities, with more or less watery discharges and cramps; constituting an aggravated case of the worst type; whether this state shall have come on without warning, or shall have supervened upon either, or both of the preliminary sets of symptoms already mentioned, time must not be wasted upon inert measures. Such a patient will inevitably perish, and within a very few hours, if the paralysed vital functions be not quickly restored.

Let him then be immediately placed between warm blankets; and should no medical person be at hand, let two table-spoonfuls of common kitchen salt, dissolved in six ounces of warm water, be given immediately, and at once, if he be an adult. Let dry and steady heat be applied along the course of the spine, and to the pit of the stomach (if no other means be at hand), by a succession of heated plates or platters. Let the upper and lower extremities be surrounded with bags of heated bran, corn, ashes, or sand, and assiduously rubbed with a warm hand, and a little oil or grease to protect the skin. Energetic, complete vomiting, will probably be produced by the salt; and perhaps bilious purging, with tenesmus.

Should a medical man be on the spot, a moderate bleeding, if it can be obtained, would be desirable, previously to, or immediately after the administration of the salt, or of any other emetic which may be preferred.

The extensively deranged action of those organs, whose nerves are chiefly derived from, or connected with, the spinal marrow; the anatomical characters found about that great source of vitality, after death, in many cases of this disease; together with the success stated by Dr. Lange, Chief Physician at Cronstadt, to have attended the practice mentioned below, founded upon these views, in twelve out of fourteen aggravated cases, fully justify the following recommendation.

In cases such as those just described, let the actual cautery be freely applied to one or two, or more places on either side of the spine, as if for the purpose of forming good-sized issues. Should the heated iron have produced any excitement of the nervous power, and the salt emetic have caused any portion of the bile to flow through its proper duct, a great step will have been accomplished towards recovery from the stage of collapse. Cordials and opiates judiciously administered; sinapisms and other external stimulants; mercurials, with mild aromatic aperients, which the intelligence and activity of British medical practitioners will not fail to adapt to the actual circumstances of each case, will conduct the patient safely to the stage of re-action.

The organs, during the collapse of this disease, probably owing to deficient vitality, often give no indication of having been acted upon by repeated doses of certain

powerful medicines, which, under other circumstances, would have produced the most pronounced effects. It is therefore suggested, that this temporary insensibility of the system should not inculcate the administration of such repeated quantities as could, by accumulation, when the organs begin to recover their vitality, give rise to unfavourable results.

Thirst being a most distressing symptom of this disease, the quality and the temperature of the drink should perhaps be left to the choice of the patient; but the quantity taken at a time should not exceed four ounces, and should be acidulated with nitrous acid, if the patient will bear it.

Should the disease prove extensively and rapidly epidemic in a large community, it would be prudent to establish stations at convenient distances from each other, where medical assistance and medicines might be procured without the risk of disappointment or delay. The details of these arrangements are left to the wisdom of local boards of health.

As the symptoms of the consecutive stage of feverish re-action in cholera differ but little, if at all, from those of ordinary typhus, except perhaps in the greater rapidity with which they but too often run to a fatal termination; and as this kind of fever is treated in no part of the world with more success than in England, the entire management of this stage of the disease is left to the zeal and science of the profession at large.

Attentive nursing, and assiduous well-directed rubbing, are of the utmost importance; a strictly horizontal position, however, must be maintained until the heart shall have, partly at least, recovered its action. An erect or even semi-erect position, during the collapse, has been often observed to produce instant death. Warm baths, therefore, for this and other reasons, are worse than useless; evaporating fluids, and indeed all moisture applied to the skin, seem to be contra-indicated for obvious reasons. Hot air baths, so contrived as to be applicable in a recumbent posture, and admitting access to the patient for the purpose of friction, may be of use."

I have the honour to be, sir,

Your most obedient servant,

E. STEWART, Chairman.

MAGENDIE'S DESCRIPTION OF CHOLERA.

A FEW days ago, Magendie, in returning to Paris, from his visit to Sunderland, was asked, by a distinguished physician in London, what he thought cholera?—"I think (said Magendie) that it is a disease which begins where others end—with death."

CHOLERA—VARIETIES.

IN a letter from Dr. de Karttsoff, dated St. Petersburg, October 28th, he states that cajepout oil was fully tried at Astrachan, without being attended with any benefit.—*Gazette Médicale*.

The same writer asserts, that the muriate of soda completely failed at St. Petersburg, in all the cases wherein it was tried.—*Ibid*.

Two women, who were suckling, were seized with cholera: two young dogs were fed on their milk; both animals died within twenty-four hours, having convulsions.—*Ibid*.

It is not true that chlorine failed to counteract the infectious principle of cholera at Moscow and St. Petersburg.—*Ibid*.

The offer of 25,000 roubles, by the Russian government, for a cure of cholera, has already produced 125 essays, pointing out infallible remedies. Unluckily almost all of these have been discovered in the closet of the writers, and never tried in the sick chamber.—*Ibid*.

In consequence of a case of poisoning, at Paris, having, in the first instance, been declared to be cholera, the Prefect has addressed a circular letter to the Mayors of the provinces, directing them to be on their guard against similar mistake or imposition.—*Ibid*.

METEOROLOGICAL JOURNAL

Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.

Dec. 1831.	THERMOMETER.	BAROMETER.
8	from 43 to 55	from 29.15 to 29.22
9	46 55	29.10 29.20
10	43 54	29.32 29.43
11	39 55	29.47 29.35
12	40 54	29.33 29.16
13	42 53	29.32 29.48
14	33 48	29.55 29.60

Wind S.E. and S.W. the latter prevailing.

Except the 13th and 14th, generally cloudy, with frequent rain. On the evening of the 12th from 7 to 7h. 15m. a violent thunder-storm, accompanied with vivid flashes of lightning, and a smart shower of hail: immediately after the first peal of thunder, the wind rose terrifically, carrying away tiles and branches of trees: this, however, was but of short duration. Faint flashes of lightning in the S.W. from 5 p.m. till after midnight.

Rain fallen, 1 inch and 25th of an inch.

ERRATUM.

In our last number, p. 360, l. 9, for "Polynesian," read "Eastern."

W. WILSON, Printer, 57, Skinner-Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, DECEMBER 24, 1831.

LECTURES
ON
THE THEORY AND PRACTICE OF
MEDICINE;

Delivered at the London University,

By DR. ELLIOTSON.

PART I.—LECTURE XII.

Inflammation—(continued.)

At the last lecture, gentlemen, I spoke of the usual terminations of inflammation, as they are called; and I likewise spoke of several other effects,—consequences of inflammation, which are observed, but still are not all among those usually mentioned by old authors—they are only recently enumerated terminations, viz. hypertrophy, or over-nourishment — atrophy, or defective nourishment, and consequent increase or decrease of size; induration, softening, and transformations of structure.

I mentioned that those which are usually called the terminations of inflammation, and those other changes which frequently are its results, may take place, at least many of them, without inflammation; that although they generally arise from inflammation, yet they may occur without it, or at any rate with an exceedingly slight degree of it, and be out of all proportion to it.

Mortification independent of Inflammation.

Having spoken of ulceration and induration, and other things, as occurring occasionally without inflammation, I was speaking, at the close of the lecture, of mortification as occurring under similar circumstances, or at least with an exceedingly small degree of inflammation, not as the result of inflammation, but only an accompaniment, if there be any at all. When the chief vessel of a part is obstructed by a ligature, this will frequently produce mortification of those parts below to which the ramifications of the blood-vessel are distributed. Sometimes the legs will mortify from bad food, and the most common kind which produces this effect is diseased grain,

Spurred rye, or *ergot of rye*, as it is called, causes mortification, I believe, in all animals, though the quantity requisite for that purpose varies in different species. If an animal live upon this, there is sometimes a reddish fluid observed to ooze from the nostrils, sometimes there is violent pain of the extremities, and sometimes there is also heat and redness, actual inflammation, and then gangrene takes place. Frequently, however, it does not cause heat and redness, but a withering of the part, which will dry and look like the extremities of a mummy. These become cold, dry, shrivelled, and withered; and at the same time, diarrhoea, vertigo, convulsions, and delirium, very frequently take place. There is internal inflammation frequently induced in the alimentary canal, but the extremities generally mortify, and look as I have stated. I need not say that this state is not contagious. It is common in France, and formerly it was common in England; and when the latter was the case, the cause appeared to be diseased wheat.

I believe that other grain, diseased in a similar manner, has a similar effect; the ergot of maize in Colombia is said thus to act: at least it will cause the hair to fall off, sometimes the teeth to drop out, and frequently induces sudden death. It so happens that brutes are exceedingly fond of it; they will therefore frequently gorge themselves with it, and then die suddenly. It was observed that the hens which ate of it in Colombia discharged their eggs prematurely before the shell was formed; and it is imagined, at any rate it is possible, that the fact of its occasioning the abortion of eggs in hens, led persons to employ it in the case of the human subject, for the purpose of emptying the uterus speedily. I may mention that these properties appear to be destroyed by cold, for it is found that if this diseased maize be carried over cold mountains—over the Paramos, it may afterwards be eaten with impunity—so at least it is said.

Mortification will sometimes occur, not from inflammation, but from ossification, it is said, of the minute arteries. Some say that

mortification will arise from the ossification of the larger and more distant arteries, but certainly it is generally believed to be sometimes occasioned by the ossification of the minute arteries. Cruveillier, by artificially inducing a similarly obstructed state, produced the same effect. He injected mercury into the arteries, so as to fill the capillary vessels with it; and when an obstruction was thus produced, mortification was the consequence.

It is possible, that, when extreme congestion of blood takes place in parts, and continues for a length of time, it may produce mortification, in some measure from the blood coagulating in the minute vessels: nothing is more likely than this.

The mortification which arises from an obstruction in the minute vessels, induced by an ossification of the minute arteries, most frequently takes place in males, and especially in the old and voluptuous. Generally the gangrene is dry, the part shrivels up, and the state is preceded by very great pain. There are two varieties of dry gangrene—the pale and the black. Some believe that mortification never occurs without inflammation, but I do not think this is proved. Many things are asserted in medicine without, I think, sufficient proof, and occasion a great deal of trouble to those that follow the asserters, by making it necessary for them to prove that the assertions are not true. I do not mean to apply this remark to the case before us, because it is only an opinion, but what I complain of is too common an occurrence. I do not know that it is proved that mortification is always preceded or accompanied by inflammation, but if it be, still it is often not by any means in proportion to the inflammation. You see the severest mortification with the slightest or no inflammation, and you see the most intense inflammation without inducing mortification. Some inflammations have a great tendency to end in mortification. The inflammation of glanders in the human subject, though not more intense than many other inflammations of the face, nevertheless has a great tendency to induce mortification.

With respect now to all the effects of inflammation, and those changes which, though they frequently are its effects, yet sometimes appear not to be exactly so, I may mention that induration is generally the result of slow inflammation, but that softening is continually seen without any signs of inflammation. Hypertrophy—over-nourishment, like induration, is, I believe, generally the result of slow inflammation; but atrophy, the wasting of a part, is frequently seen without inflammation. I may mention that in phthisis the heart is usually small, flabby, and perhaps shrivelled, though not previously inflamed. The transformations of one structure into another which is natural to the body, though not to the individual part, are certainly more frequently the result of inflammation than not; but the new formations, those

which are not analogous to any thing already existing in the body, are far more frequently not the result of inflammation. Continually there is not a vestige of inflammation to be found during their production; they rather appear to be alterations of action than the result of an actual inflammation. The part is entirely altered, a new product is formed, but that is all: it does not appear to be the result of inflammation. When inflammation does accompany those new formations which are called non-analogous, such as encephaloid disease, or cancer, it is often only an accompaniment, and not their cause. Sometimes it is only the predisposing cause: inflammation throws a part into a predisposition to disease, and then a fresh disease is set up. That these things are not the result, at any rate, of mere inflammation, is shewn by the fact that ten times more inflammation continually occurs without such effects being induced. If there be inflammation which goes on *pari passu*, still you are not justified in saying that they are the result of inflammation; and in these affections, if you employ the most vigorous antiphlogistic measures, in the most persevering manner, still you do not cure the disease; you scarcely, or not at all, control it. The morbid tendency to the morbid process is continued, and the disposition to it would often appear to excite the inflammation—the inflammation to be the consequence of the disposition to the disease, and not the disposition the result of the inflammation. Indeed, inflammation in many cases is only the ultimate effect. The new formations irritate the part so much that inflammation is excited, and an attempt is made by nature to discharge the formation. In this way scirrhus has sometimes been completely separated by nature from the body.

The existence of tubercles in the lungs, which are new formations, not analogous to any thing in the body, cannot be the result of mere inflammation, because you see inflammation occurring every day without them, and you see them produced without signs of inflammation. Occasionally, people who are disposed to this disease will, by having the lungs thrown into an unhealthy state by inflammation, become the subjects of tubercles from that time, because any thing which throws the body out of health gives a predisposition to disease of some kind or other, or if there be a predisposition, it is increased, and the new disease is excited. If you feed a rabbit badly, keep it in a damp place, and in the dark, so that it has a deficient quantity of light and heat, and unwholesome food, it may become the subject of tubercles; a new depraved secretion takes place, but this is not the result of inflammation. If you were to give an animal stimulants, you might produce an inflammatory state of the

whole body, or of any organ, but not induce tubercles. Certainly the attempt to explain every thing by inflammation appears to me to be an absurdity, and I am happy to find my opinions on this subject coincide with those of Andral, who in his *Morbid Anatomy* argues successfully against Broussais, that inflammation is not always the cause of tubercles; that it occurs continually without inflammation; and that when it does occur as the consequence of inflammation, it is not so much the result of the inflammation itself as of the part being thrown out of health, and thus easily becoming the victim of the predisposition to tubercle. These are all the observations that I have thought it necessary to make, gentlemen, respecting the symptoms and effects of inflammation, and I shall hereafter speak of all changes of structure in general.

Predisposing Causes of Inflammation.

I shall now consider the causes of inflammation; commencing with the remote; and of the remote causes, the predisposing, of course, come first to be considered. The first circumstance predisposing to inflammation is the sanguineous temperament, and the next may be considered high health. In the sanguineous temperament there is generally a great quantity of blood, at any rate there is a large quantity in the minute vessels, so that the blood circulates to a great extent—the pure blood itself—throughout; the body is readily flushed, and vessels readily contain blood which should not: the circulation is full and rapid. I need not say that high health must have the same effect. A person in a high state of health has a florid colour, there is an increased quantity of blood circulating throughout the body, and to a great extent. Besides these two general circumstances of the body, the same occurrence may take place locally. There may be a local sanguineousfulness—a local fulness of blood, and that may predispose a part to inflammation. The face is certainly more supplied with blood than any other portion of the surface of the body, and consequently when exanthematic affections take place, such as measles and small-pox, most of them affect the face sooner than any other part of the surface, the inflammation which occurs there is severer. The same thing will occur from a particular period of life. In children the circulation appears most vigorous in the head; what is called “determination of blood to the head” takes place, and all that is meant by the term is, that more blood proportionately is circulating there than in other parts. In youth the greatest determination is towards the chest; and in the middle period of life and afterwards, it is in the abdomen. Hence children are particularly disposed to hydrocephalus, to inflammation of the arachnoid membrane of the brain, to cerebral affections, and are con-

tinually having fits—continually having affections of the head; and, consequently, a large number of children die of acute diseases of the head. Hence, too, children particularly have epistaxis. Persons in youth—those who have passed the period of childhood, are certainly more subject to all affections of the chest, to all thoracic inflammations, and to inflammatory spitting of blood. It is adults particularly who have inflammation of the stomach and intestines, both acute and chronic, especially chronic inflammation of the stomach and of the liver—indeed, inflammation and congestions, and structural changes of all the abdominal organs.

Seasons and climates will operate in the same way, by exciting either general or local fulness. Pulmonary inflammation is more common in cold climates, and likewise in the winter and spring; whereas in hot climates, and in the autumnal season of temperate climates, hepatic inflammation is most common.

Occasionally a part will acquire an increased excitability from a previous injury; and this is remarkably shown in the head. If a person have once had a severe injury of the head he may afterwards suffer from a slight cause—a cause which scarcely would have disturbed him before. I saw a person who had had a fracture of the skull, and for many years afterwards he would become completely delirious on taking a glass of spirits. This is an occurrence indeed that I have frequently seen. I very well recollect the case of a man who laboured under rheumatism of different parts of the body, and among others of the scalp, but he had suffered severe fracture of the skull some years before, and the mere excitement of the external part of the head was sufficient to excite it internally, so that he had violent delirium from mere trifling rheumatism of the scalp. Previous inflammation from any cause disposes a part to be inflamed again. Various causes, of an opposite tendency, will also have the same effect. Bad air, copious evacuations, bad food, depression of mind, fatigue of body, fatigue of mind, the frequent exhibition of mercury, an exhausting climate, and previous disease of any kind, will also dispose to inflammation. While all these predisposing causes, however, induce inflammation, its character is totally different according to the nature of the cause. The inflammation induced by the first kind of causes I mentioned is active and tonic; whereas the inflammation disposed to by bad food, bad air, depression of mind, and so on, is either passive or atonic—the inflammation itself is of a passive character, or there is not sufficient power in the system to carry it on, to get the patient well through it, or produce a healthy secretion of pus, if pus be produced. Hence the character of the inflammation varies according as the predisposing cause is one which gives

strength, high health, and excitement, or one which destroys health and strength, and gives a tendency only to an unhealthy excitement.

Some of these latter causes, as the defect of heat and the defect of food, act as predisposing causes to inflammation without producing any debility, or any atonic character of the inflammation. If a part be exposed to cold, it becomes more affected afterwards by a given degree of heat;—if a person be deprived of food, then, when a small quantity of food is given him, and of the weakest character, it will produce an excitement which strong food and a large quantity only would have produced before, and therefore some of these predisposing causes—a defect of heat and a defect of food—may act also by rendering the part more liable to be affected by stimulants; for stimulants act according to circumstances that went before them. According to the first, so is the second, and all that is requisite is, for there to be a great disproportion between the two. It is no matter whether the first is very defective, or the second is very great; for the effect is in proportion to their *relative* intensity, and therefore parts that have been exposed to cold, become violently inflamed if they be exposed to a moderate heat; and a person, who has been starved, has been known to become delirious merely by taking a little real broth. Many of these causes, however, produce a cachectic state of the constitution; and then inflammation, if it occur, is of a corresponding character.

Exciting Causes.

With respect to the exciting causes, we may mention, in the first place, a local stimulus, be that stimulus mechanical or chemical, or what is properly called a stimulus, independent of mechanical or chemical qualities, such as alcohol. The effect of all stimuli, as I have just now said, is not dependent upon the positive degree of force, but upon the difference between these stimuli and the strength of those which preceded them. Every body knows the danger of going from cold to intense heat.

Besides local stimuli, whether mechanical, chemical, or true stimulants, the depression of stimulation in one part will cause an inflammation in another. From having the excitement altogether exceedingly depressed in the feet, by a person getting wet through, and sitting with wet shoes, there will be such increased activity at a distant part, that inflammation may occur, so that many persons always have a sore throat or inflammation of the chest or eyes, if they get wet through, or have wet feet. This application of cold, depressing the action of one part and thus producing an excitement in another, depends very much for its effect upon the previous excitement. If the feet, for example, have been already hot—if they have been heated, and are in a

state of perspiration—then the application of cold produces so great a depression, that the inflammation of the throat, for example, or in the chest, will be so much the greater. Cold, within certain limits, is found to be more injurious in producing distant inflammation the more partially it is applied; but cold will itself, I believe, directly induce inflammation exactly as great heat will do. When we are exposed to cold air—when exceedingly cold air is passing through the nostrils, the nose will run; and it has been found experimentally that cold will really induce the same appearances, in the minute vessels, as heat, and as absolute stimuli. Nothing is more common than for rheumatism to be felt in the shoulders, or for an inflammatory pain to be felt in the ear, when cold air is blowing upon these very parts.

Thus, it appears, I think, that cold may induce inflammation in three ways: in the first place, by rendering the body liable to be exceedingly excited by the stimuli that come afterwards—by depressing the action so much, that when the stimulus of increased temperature is applied, the effect of this is tenfold increased; in the second place, it would appear to act by depressing the action in one part, and thus exciting a correspondent excess of action in another; and thirdly, it appears sometimes to act immediately and directly, by exciting inflammation. I think we have instances of all this:—first, when a person goes from the cold into a warm room, you will then see his face become very red—become exceedingly hot, and frequently he will cough violently, and a catarrh will be set up; in the next place, we see persons have inflammation of the eyes, nose, mouth, throat, or chest, from their feet getting wet and cold; and thirdly, we continually see pain in the ear, rheumatism of the shoulders, and the nose running, when cold air is applied to these very parts.

I believe that sudden refrigeration, and the sudden application of cold after excitement, is more frequently a cause of inflammation than the application of heat after cold. The greater number of cases of inflammation that one sees, are instances where persons have become suddenly cool—perhaps have got wet, as well as suffered the application of merely cold air—when they were over-heated and fatigued. A rigor is frequently induced, and a state arises which is soon followed by inflammation. The effect of sudden refrigeration, therefore, is greater in proportion to the previous excitement; it is also greater if it be partial than if it be general; and it is always greatest if there be much sweating and exhaustion; and, indeed, I may mention that sometimes fatal results have instantly occurred. Many persons, from taking ice when they have not only been hot, but sweating and exhausted, have dropped down dead. The account of Alexander the Great bathing in the Cydnus when exceedingly hot

and exhausted, after great fatigue, is well known: he nearly lost his life by it. However hot the body may be, there is little danger, comparatively, if it be not fatigued. The Russians go from the vapour bath and roll in the snow.

The effect of cold applied to the body is much increased by damp: very often persons do not suffer from the application of cold till dampness is conjoined with it. It may act simply by increasing the cold—by lowering the temperature still more; but I cannot help thinking that it injures to a greater extent than this will explain—that it is injurious out of proportion to the greater diminution of temperature which the dampness occasions. Many persons are not so strong by any means in damp weather. It is possible that it may act by conveying the electricity away in too great a quantity. In dry weather we are all active and vigorous; but in damp weather there certainly is a degree of languor—an inaptitude for action, both of body and of mind, which most persons notice.

The effect of cold upon the body is much increased by sleep. In sleep the powers of the body are less, the pulse is slower, the heat is not so high, and all noxious agents are less withstood. It is a common remark, that if a person go to sleep in a draft, he is sure to catch cold; whereas, if he contrive to keep awake, he very likely escapes. At any rate, every body knows that if a person fall asleep when exposed to cold, he is almost sure to suffer.

Cold, too, appears to be more dangerous when applied to the back part of the body than to the front. I imagine there is less power there than in the front. Persons will escape if a little draft come in the face, whereas, if it come to the back of the head or neck, they almost always experience ill effects.

Inflammation is sometimes excited as a sympathetic occurrence. But when I say that inflammation is excited by sympathy, I do not explain the fact; I merely express the fact. When one eye is inflamed, the other, without any external cause, may also become inflamed; and this is called a sympathetic result—it is said to occur by sympathy. It is common, if the head be inflamed in a child, for the abdomen to become likewise inflamed—either the intestines, stomach, or liver, and *vice versa*. It is of the highest importance in practice to be looking out for this circumstance, or you may be treating only one inflammation when two exist. If you be asked the nature of a case of this description, you will be fully justified in saying that is complicated—that there is inflammation in two parts, though you told the friends with truth at first, that there was inflammation of only one.

Metastasis is also another mode in which inflammation occurs. When inflammation of the parotid glands ceases in mumps, the

brain will become inflamed, or, what is more common, the testicles, or one testicle; and this inflammation, I may mention, often affords an illustration of atrophy as the result of inflammation. It is very common, indeed, for a testicle, when it has been inflamed after mumps, to waste away, so that nothing will be left but the membranes. In gout, metastatic inflammation continually occurs. When the gout ceases in an extremity suddenly, it is very common for inflammation to take place in the head of an apoplectic nature, or for inflammation to occur in the stomach or intestines.

We occasionally have inflammation induced from the introduction into the circulation of an unhealthy or acrid matter. If pus, or any diseased secretion, be absorbed, it is very common to find a deposition at a distance from the part in which it was originally formed. We have at a distance a deposition of pus, or of lymph, in the various viscera, in the cellular membrane, and even in the secreting cavities, or inflammation and disorganization without deposition. Probably inflammation of the veins takes place, and perhaps the pus formed in them causes such an inflammation in distant parts, that new pus, lymph, and other things, are there produced. But such is the fact, that if pus, or an acrid secretion, get into the circulation, these results take place—if not distant inflammation, at any rate a deposition of pus, or lymph, debility, great irritation, and a sallow hue of the countenance.

It has been said, that the predisposing causes to disease will sometimes become exciting, merely by their continuance and increase, so that no exciting cause is necessary. This may be the case with regard to fulness of blood. A part may become more and more filled with blood; congestion or determination of blood, whichever it is called, may increase till at last inflammation occurs; or a state of apoplexy, if it be the head that is affected, may be produced without any actual exciting cause. However, in specific inflammation this cannot be the case. I suppose that all the predisposition in the world would not give a person the small-pox, unless the exciting cause were applied.

Such are the observations which I have thought it necessary to make respecting the remote causes of inflammation.

Proximate Cause.

We will now consider the proximate causes of inflammation—that which is essential to the existence of the disease—in fact, the actual state of the parts in inflammation. In the first place I think it is a fact, that in inflammation blood is found in vessels which are not designed to contain it—blood enters vessels which in a healthy state contain no blood; and, in the next place, those vessels which naturally contain blood are enlarged and contain more blood than they ought to do. John Hunter made an experi-

ment to prove the latter circumstance. He says, "I froze the ear of a rabbit, and thawed it again; this excited a considerable inflammation, an increased heat, and a considerable thickening of the part. This rabbit was killed when the ear was in the height of inflammation, and the head being injected, the two ears were removed and dried. The uninflamed ear dried clear and transparent; the vessels were seen distinctly ramifying through the substance; but the inflamed ear dried thicker and more opaque, and its *arteries were considerably larger.*"

Now that vessels have undoubtedly a contractile power, is shewn both in the large vessels and in the capillaries. I do not know how the action of the capillaries, or of the arteries, can contribute to the circulation in health, but it may alter the circulation, according to the larger or smaller size of the vessels—accordingly as they are constricted or dilated, so that there may be more or less blood in the part itself, and in those parts to which these vessels lead. The whole of our present knowledge upon this subject, together with my own opinions upon it, you will find, like the circumstance of the coagulation of blood, in a note to Blumenbach's Physiology, upon the circulation; I will not, therefore, dwell upon it. Notwithstanding it is quite certain that the arteries and capillaries have this power of contraction—notwithstanding I doubt it can assist in furthering the circulation, although it may and every day does certainly alter it—I cannot conceive that this increased action is compatible with increased redness and fullness; I cannot conceive that, in inflammation, when a part is redder and fuller than it should be, there can be increased action—by which I mean, that the vessels can be more constricted and more dilated, than before; for if they were redder at the one moment they would be paler at the other—the one would counterbalance the other. But whether there is an increased action or not, it would appear, at least, that there is a diminished motion of the blood in inflamed vessels. There appears to be a retardation of the motion, and such retardation is commensurate with the inflammation. This has been shewn by Vacca, Lubbock, Bichat, Allen, Dr. Wilson Phillip, and recently by Dr. Hastings, under the microscope; and still more recently, Gendrin has confirmed the observations which Drs. Philip, Hastings, and others made. They applied mechanical violence, heat, cold, and ammonia, and at first they perceived by the microscope that the blood moved more rapidly; there was an increased velocity, and a contraction of the vessels was seen, and a paleness. This is what we should expect from what is called increased action. After the increased action, after the contraction of the vessels, the motion became slower, the course of the blood was retarded, the vessels became dilated, and inflammation appeared; in this

they all agreed. If the part were already inflamed and then a stimulus was applied, this stimulus had the immediate effect of quickening the blood's motion, constricting the vessels, and then the inflammation ceased. If the stimulus were applied very violently, so as to produce most violent action, and extreme rapidity, of course this lasted a shorter time; and the second stage—that of slow motion, dilatation of the vessels, and inflammation—came on sooner. By applying the stimulus very violently, the second stage might be made to come on immediately. It is said, however, by them, that the oil of turpentine differed from all other stimuli, by causing the first stage only—it was not followed by the second; and if it were applied in the second stage, it always removed it; while common salt, on the contrary, caused instant dilatation—caused instantly the second stage. It would appear from these experimenters, who are very numerous, reside in different countries, and have succeeded each other, and each repeated the experiments with a view to ascertain whether his predecessors were correct or not—it appears from them, that, in the inflamed part, the blood moves more slowly, that the vessels are dilated, and that by applying stimuli you cause the vessels to contract, the blood to move more rapidly, and then the inflammation ceases for a time, till the blood is retarded again, and the vessels dilate. One would not expect that cold would have exactly the same effect as stimuli, but such was found to be the case. We all know, that if parts are cold they become reddish, as is seen in the nose and cheeks in winter: they grow dark—grow more and more red, till at last they become of a mulberry colour; and perhaps, such congestion takes place that gangrene may occur.

You will find, on this subject, very excellent information in Dr. Hastings's work on Bronchitis; in the introduction to that work he treats this subject very fully; and since he published, Gendrin, on the continent, has repeated the experiments with precisely the same results.

You will find some objections to these results—from experiments made by himself—in Dr. Thomson's work on Inflammation (and, by the way, I may mention that that is a work which every person should read, and not only read, but study carefully. I think it is a much better book to read on inflammation than John Hunter's); but the objections he starts are fully answered by Dr. Hastings. Dr. Thomson says that you have, in inflammation, increased action and motion; but Dr. Hastings answers, that Dr. Thomson himself, in some cases of inflammation, saw a slower motion: and that he speaks of seeing red globules in the parts where the blood was in rapid motion, and must have made a mistake, because in the blood of parts inflamed you will not discover red globules at all, as they are mingled together or broken

down. Dr. Hastings concludes, therefore, that in the cases in which Dr. Thomson saw rapid motion, there could not be inflammation, but that he saw the state which precedes inflammation.

The large vessels going to an inflamed part are frequently felt to throb. If there be a whitlow upon the finger, the digital artery behind it throbs violently; and in inflammation of the face the angular artery may be felt doing the same; and in the case of headache the temporal artery is in a similar condition. This, however, is no proof of increased action in them, for the same occurs in an aneurism, or a dilated artery. When an artery is dilated, though weakened, softened, and unable to act as it did before, you will find it throb violently, simply on account, I presume, of its increased capacity. However, in that kind of inflammation which is called passive, and which some say is not real inflammation, but mere congestion, this enlargement of the surrounding vessels is wanting, so that you have not this throbbing of the large vessels.

These appear to be the chief facts ascertained regarding the state of parts in inflammation; but there are many other particulars. In the height of inflammation the open extremities of vessels certainly are affected with spasm; they will not pour forth fluid as they did before. When a mucous membrane is inflamed, its secretion will stop; the secretions of an inflamed part at the very height of inflammation cease, or at least are greatly diminished, so that one must suppose that there is a spasmodic and constricted state. Perhaps the word spasmodic may be objectionable; but a constricted state of the extremities of secreting vessels appears to exist in inflammation. Yet, however, far more changes than these take place; the blood is buffed and cupped, it contains more fibrin, and this is thinner than it ought to be. The blood in the small vessels becomes homogeneous, broken down, the red particles are indistinct, and sometimes flocculi are seen in it which are not visible in the immediate surrounding capillaries. When a part is mortified, then the blood around it is yellow, its various constituents separate, and it is evidently in a state of disease.

Ratio Symptomatum.

With respect to the explanation of the symptoms of inflammation. The *redness* is very easily explained, from the existence of blood in vessels that ought not to contain it, and the existence of too much in those which should contain it. The *swelling* will arise from the same circumstance, and also from the increased secretion that is going on around. The *pain* will arise from fulness, and from the tension of all the parts; this is produced by the accumulation of blood, and by the excessive secretion around. But

the pain will also arise probably from an increased degree of sensibility. There is, in every case, the pain from fulness and tension; but the pain is felt far more severely, on account of the morbid sensibility. With respect to the *heat*, I presume it arises from the increased momentum of blood in the part, the increased quantity, and which is still circulating. If the same quantity of blood were in the part, and motionless, of course the part would grow cold; but as the blood which is in it is far greater than usual, and still moves on so as to come round to the lungs, a greater number of changes giving rise to heat must take place. There is far more blood in the part than there should be: and as heat appears to be connected with the existence of blood circulating, coming round to the lungs and undergoing chemical changes—in proportion to the quantity of blood circulating there must be increased temperature. If the blood cease to circulate, or the circulation becomes languid, so that it does not undergo a regular chemical change, then of course the part becomes cold. Momentum is composed of rapidity and quantity. The former is lessened, but the latter greatly increased, so that on the whole the momentum is greatly increased.

CLINICAL OBSERVATIONS

Delivered in the General Dispensary,

By MR. COULSON,

Dec. 3, 1831.

Hæmatocele and Hydrocele.

GENTLEMEN,—The case of hæmatocele on which you recently saw me operate, is deserving of your notice. The patient, John Vaughan, æt. 34, first laboured under hydrocele about eight years ago, since which he has had the water drawn off six times in all—five times by my predecessor, Mr. Lloyd, and once (on the last occasion) by a surgeon from the country, who was then on a visit at his master's house. The scrotum gradually filled again; and when it had attained its usual size, (30th of October) the patient requested a surgeon to tap him, and the operation was accordingly performed. The water which was drawn off was tinged with blood; and soon after the operation, the patient experienced great pain in the part, and the scrotum attained a large size, and became very black. Great constitutional irritation also supervened. On November 7th I first saw Vaughan, when he was then fast recovering from the effects of the effusion of blood which had taken place. Some discoloration of the integuments from the effused blood remained, and the removal of this I endeavoured to hasten by the use of stimu-

lating lotions, as spirits of wine and water, and vinegar and water. The scrotum was then very large. When all the blood between the tunica vaginalis and the integuments of the scrotum had become absorbed, I advised the patient to submit to another operation, to which he readily consented.

On November 17th I laid open the cavity of the tunica vaginalis, and removed rather more than six ounces of dark-coloured coagulated blood. There was a gentle oozing from some parts of the tunica vaginalis, but no distinct vessel could be seen. The tunica vaginalis itself was very much thickened, and there was an imperfect membranous septum going across the middle of it. A pledget of lint was introduced between the edges of the wound, to keep them open; the tunica vaginalis soon began to granulate, and the wound is at present nearly healed. No hæmorrhage, or any other unpleasant symptom, has occurred, and I hope a radical cure has been effected. The testicle, however, is much enlarged. The hæmatocele occurred in this case from the accidental puncture of a vessel during the operation for hydrocele; no blame is to be attached to the surgeon who performed it. The suddenness of the occurrence of this complaint is its characteristic feature; it comes on rapidly after a blow, or contusion to the part, or subsequently to the operation for hydrocele. When the blood is effused into the tunica vaginalis, you must pursue the plan of treatment which I adopted in Vaughan's case; when, however, it is simply effused into the cellular tissue of the scrotum, the application of leeches and cold lotions, with rest, will be in general sufficient to effect a cure. The two other forms of this disease, (viz. effusion of blood within the tunica albuginea, and into the cellular tissue covering the spermatic cord,) I have not seen. I will not, therefore, detain you with an account of them, but must refer you to Mr. Pott's work on hydrocele, in which they are fully described.

Hydrocele, with Hernia.

John Leeson, æt. 43, was admitted under my care November 20th, 1831. He has a hydrocele on the left, and a scrotal hernia on the right side, which enables you to observe the diagnostic marks of this complaint on the same patient. Coughing communicates an impulse to the hernia, but none to the hydrocele; and the spermatic vessels can be distinctly felt on the one side, and not on the other. Pressure makes an impression on the hernia, but none on the hydrocele; and when the patient lies down the hernia returns, but the hydrocele remains unchanged. You are not likely to confound these two complaints. I drew off a pint and a half of fluid, and should have performed

the radical cure, but he could not conveniently stay from his work.

Hydrocele.

John Reynolds, æt. 60, was admitted under my care, October 19th, 1831, with hydrocele. I had often tapped him; and the only remarkable feature in his case was the situation of the testicle. Instead of being placed at the back and lower part of the scrotum, it was situated in front. I therefore introduced the trocar, not in front, but at the side of the swelling, and thus drew off the water.

Disease of the Hip—continued from p. 293.

In disease of the hip, (to which subject I drew your attention at our last meeting) the parts entering into the formation of the joint, undergo, as you might expect, very considerable changes. The nature of these changes depends in a great measure on the extent of the disease, and on the constitution of the person attacked by it. The complaint which we have been considering is inflammation, shewing itself first in the cancellated structure of the head of the femur, and then extending to the acetabulum and parts in the neighbourhood of the joint. If the complaint occurs in a scrofulous subject, the inflammatory action will be very much modified by this state of the constitution. In scrofulous inflammation of bone, the earthy matter becomes absorbed, and the bone consequently softened, whilst the cancelli are filled with a yellow caseous matter, or a transparent yellow fluid. But the striking feature in this kind of inflammation is the absence of all secretion, or deposit of bone; whereas in simple inflammation, uninfluenced by the scrofulous diathesis, particularly when it becomes of a chronic character, bone is secreted in abundance. Mr. Langstaff, who has paid great attention to this, and, indeed, to every other department of pathological anatomy, says that he never saw a case of bony ankylosis in a scrofulous subject, and he does not believe that it ever occurs. The head of the bone is always changed in structure, and in scrofulous subjects it is much softened.

By the extension of the inflammation the ligaments of the joint become altered in their texture, loose, and spongy; the cartilaginous coverings of the acetabulum and femur are removed; and the two surfaces sometimes become firmly united. Ankylosis between the head of the femur and the acetabulum is the most favourable termination that can be expected of the complaint; for the only effect which remains is a certain degree of shortening, and impeded motion of the limb. In a few other cases the head of the bone is destroyed by caries, and the neck, or even the trochanter major, becomes ankylosed with the acetabulum, which, I think, would

have been the result in Dexter's case, which I mentioned at our last meeting, had she survived. In the majority of cases, however, the bone is dislocated, and either becomes ankylosed with the bone on which it rests, or the attempt at the formation of a new joint is set up. The bones of the pelvis often suffer very considerably in this disease, and the matter which forms near the joint often finds its way into the pelvis. Mr. Brodie mentions two cases in which the matter was voided by the rectum. In fact, in some cases the os innominatum is more extensively affected by the caries than the thigh bone itself. Rust, who has published a minute description of the unnatural direction given to the muscles in this disease, has also described the change which the bones of the pelvis undergo in their form and situation. The pelvis, he says, in those who have for a long time gone lame, is pushed upwards, the sacrum is flat and straight; in a few cases, however, it is more curved than in the natural state. The coccyx is bent strongly forwards, and the connexion of the last lumbar vertebra with the sacrum forms a right angle. The ileum of the affected side stands higher, and has in general a perpendicular direction, and more of a triangular form. The external surface is smooth, whilst the fossa appears more hollowed than usual. This hollowing probably depends on the action of the iliacus internus, which is greater than that of the glutæi. The horizontal portion of the pubis often seems lengthened, and lower than in the natural state, and the ischium is usually drawn outwards and forwards. The perpendicular direction of the foramen ovale is changed more to an horizontal one, and the opening assumes more of a triangular shape, the base of which is turned to the acetabulum. In consequence of the changed situation of the bones of the pelvis, its different diameters undergo an essential deviation from the natural state. The superior apertures of the pelvis are commonly somewhat oblique, and the pelvis broader on the affected side from before backwards. I have dwelt with some degree of minuteness on the disease of the hip-joint, which commences in the cancellated structure of the bone; but the synovial membrane, as well as the cartilaginous coverings of the femur and acetabulum, may also become the primary seat of disease; and to these diseases I will direct your attention at some future period.

I will now mention to you some of the complaints with which the disease of the hip may be confounded. One of these is an affection to which children are subject at the time of dentition. A mother will bring you a child with supposed disease of the hip; on inquiry, you learn that the child could walk at the usual period, but that when eighteen or twenty months old, he was unable to

stand, and that the child at this time was cutting its teeth. On examination of the limb you will find it wasted, the nates of the affected side flat, and the limb, if there be any difference, a little longer than the sound one. When the child attempts to walk, it cannot raise the limb from the ground, but draws it along. Now the diagnostic mark of this disease is the absence of any pain in the joint. If you place the child on the table and press in the neighbourhood of the joint, or rotate the head of the femur, no pain is produced; whereas, in disease of the hip the pain would be great. After the period of dentition the general health is little affected by this complaint. Some years commonly elapse before the child recovers the use of the limb, but ultimate recovery may, in most cases, be confidently relied on. I know a child, now five years and a half old, who was attacked by this complaint in the right lower extremity, when at the age of a year and ten months, but who has not yet quite recovered the use of his limb. There is a case which I will now shew you, of the same affection in the right upper extremity; for it attacks the upper as well as the lower limbs, though less frequently. The child, William Newman, is now three years and a half, and when a year and nine months old, during the time of cutting his teeth, lost the use of his right arm, which was attributed to accident. I saw the case at the time, and have watched it ever since. The child is gradually recovering the use of the limb. In this case the natural temperature of the limb was lessened, but the power of sensation was not impaired. Though it is not my intention at present to enter into the treatment of this form of diseases, I may observe that local remedies are of very little use; you must occasionally give purgatives and tonics, if the child requires them; but above all, you must impress on the minds of the parents, that a considerable length of time will be required for the cure.

There is another form of disease which may be readily mistaken for disease of the hip; and it occurs in nervous females, either just at the time when menstruation may be expected to commence, or afterwards, when some irregularity in this function has taken place. The patient complains of great pain in the hip, which extends also to the lumbar region, and down the thigh to the knee, a pricking sensation, and coldness in the affected limb, and an inability to walk. There is also great nervous agitation, either affected or real, in these cases. Pressure in the region of the hip occasions great pain; but whatever part you touch appears equally painful, whether over the joint or near to it; the bowels are costive, and the catamenia irregular. Now, in these cases, there is a great tendency to contraction, either of the thigh on the pelvis, or of the leg on the thigh; and unless you take means from the com-

mencement of the disease to counteract this, you will have more trouble with the subsequent deformity than the original complaint. I attended, with Mr. Harkness, sen., a young lady at Limehouse, affected with this complaint. Before the patient was placed under our care, the case had been mistaken for disease of the hip, and *moxæ* had been applied. The most positive assurance on our parts could scarcely remove the impression, which existed in the minds of the anxious parents, of there being no organic disease in the joint, and of there being no necessity to pursue the counter-irritant plan of treatment, which had been begun. The knee was very much contracted, and a good deal of trouble was experienced in straightening it. Three years and a half have elapsed since the first attack of the complaint, and the patient (though she has been slowly improving) is not yet recovered. My attention was first directed to this peculiar form of disease by the perusal of an able article in Dr. Johnson's *Medico-Chirurgical Review* for June 1828, in which cases of this complaint attacking the knee are related. I have met with three cases since that time—two of the hip and one of the knee. I hope that from this description, which I have given you, you will easily distinguish between this nervous affection and diseases of the hip. In the former the pain is felt, from the commencement, in the hip, and is not confined to the joint, but is diffused over the whole region, extending to the loins and to the knee. The patient is also unable from the first to walk. The great nervous agitation, the state of the intestinal and uterine secretions, and the unaltered position of the trochanter major, will convince you that the case is not one of disease of the hip. "There is one circumstance (says Dr. J.) attending this hysterical affection of the joints, which is calculated to mislead practitioners—namely, their being, for the most part, consecutive to a blow or injury. The fact is, that it is generally some cause of this kind which determines the local habitation of the hysterical leaven that is lurking in the constitution." Local remedies are, I believe, of very little use. I have tried *Empl. Belladonæ* and the *Emp. Opii*; friction, with both anodyne and stimulating linaments, and electricity; and I cannot say that I have seen any benefit derived from their employment. Our chief dependence ought to be placed on those internal remedies which give tone to the system and improve the secretions from the alimentary canal and uterus. The ammoniated, or the muriated tincture of iron, given in some light, bitter infusion, will be found serviceable; and if the bowels be costive during the use of this remedy, purgative injections, containing a solution of *socotrine aloes*, of a strength proportioned to the age of

the patient, should be daily administered. The bowels should be kept well open, for the symptoms of the disease are invariably aggravated when the bowels become costive. The compound decoction of *sarsaparilla*, combined with the compound decoction of *aloes*, will sometimes agree very well with the patient. If there be much hysteria, the tincture of *valerian*, combined with the *vinum aloes*, may also be tried with benefit. Warm salt-water bathing was tried by one of my patients with great advantage.

The last affection which I shall mention as likely to be mistaken for hip disease, is *psaos abscess*, of which I have a case now under my care in this institution. The diagnostic marks between the two diseases have been thus arranged:—First, in *psaos abscess* the patient complains of violent or dull pain in the region of the loins, which is very much increased in the upright posture of the body, and every motion of the limb, particularly on extending it. In the diseased hip there is no fixed pain in the loins; it is felt more in the neighbourhood of the hip, and especially in the knee. Secondly, in *psaos abscess*, during the whole course of the complaint, there is no deviation to be perceived in the natural situation of the trochanter, and no difference in the length of both limbs. In diseased hip, on the contrary, this is always the case. Thirdly, in the affection of the *psaos* muscle, the patient cannot turn the foot of the affected side outwards, without increasing the pain; whereas, on the contrary, in diseased hip the foot is generally turned outwards. Fourthly, on taking a deep inspiration, on coughing, crying, and in the erect posture of the body, the fluctuating swelling either on the nates or on the front of the thigh increases; and the exit of the matter, if the abscess be burst or opened, will be facilitated; but in abscess of the hip-joint from disease, neither is the case. In both diseases, however, the general health is very much deranged.

Treatment of Disease of the Hip.

It is very material, in our treatment of the disease, to distinguish between those cases which occur in strong subjects and those which occur in persons of a *scrofulous* habit; not only as it regards the employment of remedies, but also the result which we may anticipate. In strong persons, blood should be abstracted from the neighbourhood of the joint by cupping, and the quantity of blood must be regulated by the extent of the disease and the strength of the patient. The abstraction of blood in this manner, together with rest (for this is most essential in disease of the joints), will, if you are consulted early, be often sufficient to arrest the complaint. In *scrofulous* subjects, on the contrary, even in the early stages, the taking of blood is seldom advisable; but the applica-

tion of blisters I always resort to, and have frequently seen blistering, combined with rest, alone sufficient to stop the disease. Even in the second stage of the affection, I should place my chief reliance on these remedies. But if, in spite of the means which you employ, the complaint proceeds, you must, in the first class of patients (I mean those not tainted with scrofula), resort to the use of moxæ, or issues. I should prefer the former. One of the main objects of Professor Rust, to whose work I have often called your attention, is to recommend the use of the ferrum caudens, or the actual cautery, in this stage and this form of disease in the joints. I had, for a long time, the opportunity of seeing this practice at Berlin, and of watching the effect of this mode of treatment, and can bear testimony to its beneficial results. But this remedy is of too formidable a character, even in appearance, to make much way in this country; nor, indeed, would it on the Continent, but the hospitals there are under the control of the government, and the patients must submit to whatever is prescribed for them. The moxæ, however, I think, will answer every purpose; and two should be applied near to the trochanter, and a copious discharge kept up from them. Now, in scrofulous subjects, as we commence with the counter-irritants, and have not succeeded by their use to stop the disease, it is evident that we must resort to some other plan of treatment. I shall be disposed, in future, to give iodine a fair trial in scrofulous diseases of the hip. I shall give it internally; and after trying blisters, shall (if the patient can bear it) employ friction, with the hydriodate of potass ointment, on the joint. From the success which has attended the use of this remedy in other forms of scrofulous disease, I think we may anticipate beneficial results from its trial in this. Indeed, M. Lugol, in his Memoir, which has been recently translated by Dr. O'Shaughnessy, mentions three cases of disease of the hip cured by iodine. The iodine was exhibited internally, ioduretted injections were thrown into the fistulous openings, and the sores dressed with iodine ointment. I must refer you to the work itself for the formula of these different preparations. I may also say that a tract was published by Mr. Buchanan, in 1828, on diseased joints and the non-union of fracture, in which he states one effect of iodine to be that of producing increased action of the arteries in the extremities of bones, and consequently secretion of ossific matter. Mr. Crosse, of Norwich, has also published some cases of non-united fractures successfully treated by iodine. I mention these facts merely to assist you in your reflections on the *modus operandi* of this remedy in scrofulous disease of bone. If the formation of matter takes place, poultices and warm fomentations

should be applied; and when the abscess is in a fit state, it should be opened. Ford was very adverse to the opening of abscesses near the joints, and, at page 87, says, "who has not seen the fatal consequences of opening abscesses of joints, and the quick transition of strumous indisposition of bone, cartilage, or ligament, forms a curable to an incurable caries?" It may not be always advisable to evacuate all the matter at one time from an abscess (for sometimes it attains a very large size), but there can be no doubt but that much pain is saved to the patient by opening the abscess, and not allowing it to break of its own accord. The effects which Ford has described, occurred, not in consequence of opening the abscess, but in spite of it. In this stage of the disease, you must give anodynes to allay the pain, and the quinine internally to keep up the patient's strength. The patient's diet must also be nutritious. When the active character of the inflammation has passed away, the patient will be much benefited (particularly if he has been resident in a crowded city) by change of air and removal to the sea side. The poor even, of this metropolis, can obtain this advantage, through the Margate Sea-bathing Infirmary, which, during the season, can accommodate as many as 180 patients at one time. The building is now enlarging, and a much greater number will in future be able to obtain admission. It is folly, however, to advise a change of air and sea bathing if there be much pain in the joint, and signs of active inflammation going on; it is only when the case becomes of a chronic character, and is uncombined with pulmonic symptoms, that advantage can be expected from the change. Gentle purgatives must be given as often as the case requires them. There are several other points to which I intended calling your attention, among which is Mr. Scott's mode of treating diseased joints; but I must defer the consideration of them to some other opportunity.

CASE OF

MANIA FROM EXHAUSTION,

With Observations.

By MARSHALL HALL, M.D. F.R.S.E. &c.

To the Editor of the London Medical Gazette.

"*Palmar qui meruit, ferat.*"

SIR,

SOME time ago I had to set that eminent lecturer and physician, Dr. Elliotson, right on the subject of an infantile dis-

ease, resembling hydrancephalus, but arising from exhaustion. Dr. Elliotson, and I think Dr. Graves, of Dublin, had attributed the first detection and public notice of this infantile disease to the late Dr. Gooch; whereas, Dr. Gooch himself expressly states, that that merit (be it great or small) is due to me. In Dr. Gooch's treatise, he expresses himself in these terms:—"I sha'll not encumber this paper with a multitude of cases, but state that the above are specimens of a class, of which I have seen enough to convince me, that they deserve the attention of the profession. If I had any doubt about this, this doubt would be removed by the fact, that Dr. Marshall Hall has already recognized them, and described them in a paper which has been read at the Medico-Chirurgical Society*. He has, therefore, anticipated me in announcing them†," &c.

I should not have reverted to this subject, had I not seen a repetition of this error, in a recent work, by Dr. Billing. At pp. 47 and 110, of his "First Principles of Medicine," that gentleman ascribes the first description of the disorder in question to Dr. Gooch. In fact, I first noticed and described it in 1825.

The same unwelcome task of claiming for myself what is given to another, is imposed upon me by a paragraph in Dr. Seymour's lecture, published in No. 104 of your journal. Dr. Seymour observes:—"In the cases [of mania] arising from childbed or during nursing, Dr. Gooch has very clearly demonstrated, that these occur either in consequence of, or during, an exhausting process, and are relieved by tonic and antispasmodic medicines and restorative diet."—P. 118.

Now, sir, if you will turn to Dr. Gooch's valuable work, p. 128, you will find that that gentleman again distinctly gives me the credit of first tracing puerperal mania chiefly to exhaustion.

To say the truth, Dr. Gooch did not do this quite fairly, and on this very point I addressed a note to you, which appeared in the Medical Gazette for July 1829; and this note produced a private letter from Dr. Gooch, which, with the exception of its being addressed to me instead of to you, was highly satisfac-

tory to me, and not a little complimentary.

As I shall probably not notice this topic any more, I may just add here, more distinctly than before, and once for all, that there is a coincidence in Dr. Gooch's views and my own, upon a variety of subjects, which is too extraordinary to admit of being referred to mere accident. And as my publication preceded that of Dr. Gooch, it has always appeared to me that the circumstance should have called either for some explanation, or for some further acknowledgment. For instance:—

I have observed that the effect of remedies, and especially the degree of tolerance or intolerance of loss of blood, becomes a source of diagnosis of cases similar in their general aspect. Dr. Gooch observes, "the effects of remedies on a disease, if accurately observed, form the most important part of its history; they are like chemical tests, frequently detecting important differences in objects which previously appeared exactly similar*." Dr. Gooch and myself have alike cautioned the young physician against mistaking some of the morbid effects of loss of blood for inflammatory affections†. We have both traced puerperal mania, chiefly to mingled intestinal derangement and exhaustion, deducing thence its appropriate mode of treatment‡. We have equally concluded, from our respective observation, that puerperal phrenitis is comparatively a rare disease§. We have alike described two kinds of puerperal diseases, of which the first requires and bears depletion, while the second is characterized by intolerance of loss of blood||. A farther coincidence occurs in our views of the condition of the general system in chlorosis¶. Lastly, Dr. Gooch has made similar observations on a "hydrancephaloid affection of infants," arising from several sources of exhaustion, but "erroneously attributed to congestion of the brain**."

Such coincidences in the conclusions of persons observing the same objects

* Commentaries on some Diseases of Females, (first edition,) pp. 219, 220. Gooch on Diseases peculiar to Women, p. 37.

† Commentaries, pp. 241, 242. Gooch, pp. 38, 364, 365.

‡ Commentaries, pp. 251—253. Gooch, pp. 124, 132, 144, 145.

§ Commentaries, p. 252. Gooch, p. 152.

|| Commentaries, p. 220. Gooch, pp. 33, 37.

¶ Commentaries, p. 62. Gooch, p. 365.

** Gooch, p. 365.

* A short report of the paper will be found in the London Medical Gazette for 1829.

† Account of Diseases peculiar to Women, p. 365.

independently of each other, must generally prove those conclusions to be just. It is true I had sent Dr. Gooch a copy of my work immediately on its publication, and my paper on the hydrecephaloid affection of infancy had just been read before the Medico-Chirurgical Society, and noticed in the journals.

But these are matters of mere private concern, and I hasten to the discussion of a point of more general interest.

Dr. Gooch asks the question—"Would an equal or a greater degree of exhaustion occur at any other time?" (*i. e.* than after delivery). "This is a question of fact which I should answer in the negative*."

I, on the other hand, would answer, yes. And this leads me to the detail of an interesting case which occurred to me, conjointly with Mr. Yates, of 17, Gower-Street, a short time ago.

The patient was in an almost bloodless state, from chlorosis; in this condition she became affected with pneumonia, and bore blood-letting well; and lastly, after perfect recovery from her inflammatory disease, she became affected with mania, which, I believe, no one could have distinguished, except from the history, from puerperal mania. There were considerable powers of bearing the loss of blood, even in chlorosis, under the influence of pneumonia: when the inflammatory affection had subsided, the patient became susceptible of loss of blood, and experienced one of the forms of exhaustion, namely, mania.

But the case is so interesting, as to deserve to be detailed more minutely. For the following particulars I am indebted to Mr. Yates, who conducted the patient through her illness with the greatest attention and skill.

"My dear sir,—The following is an outline of the case you requested me to forward you, illustrative of mania occurring after depletion.

"On the evening of the 22d of January last, I was requested to visit L. A., aged 26, who was represented to have been ill from cold several days. I found her suffering greatly, with impeded and painful respiration, flushed cheeks, accelerated pulse—in fine, with all the symptoms of severe pneumonia. I bled her to fainting, whilst sitting upright in her chair; this took place after the loss of about twelve ounces of blood. Pur-

gatives and sudorifics were prescribed for the night.

"I paid an early visit on the morning of the 23d, and was informed she had remained faint nearly an hour after the bleeding, and had breathed more freely, and with less pain. The blood was cupped and buffed. Twelve leeches were directed to be applied to the chest. During the forenoon the symptoms resumed their formidable aspect; and when I had the pleasure of meeting you in consultation, it was deemed proper to bleed her again, and that she should take a grain of the tartarized antimony every hour until tolerance of the remedy should be induced. Venesection was performed in the erect position, and fainting again occurred when about fourteen ounces of blood had flowed. This with the powerful effect of the antimony, gave relief during the remainder of the day; but when I saw her again late in the evening, I felt called upon by a renewal of symptoms to have further recourse to the lancet; and again our patient fainted and was relieved.

"The 24th was passed tolerably satisfactorily, without depletion. The bowels were relieved by appropriate medicines, and the effect of the antimony appeared sufficient for our purpose.

"On the 25th, the pain and difficulty in breathing were more urgent; our patient was bled again to fainting. In the evening the same remedy was required, and was followed by a similar effect. The tartarized antimony and aperient medicines were continued.

"26th.—The last basin of blood, containing about twelve ounces, was free from any of the inflammatory appearances so strongly characterized in all the former bleedings. Respiration free, with only a little pain in the left side, for which twelve leeches were applied; they bled copiously, and produced fainting. I prescribed the digitalis.

"27th.—Better in every respect; saline and aperient medicines were substituted for the digitalis.

"From this time our patient became, progressively, apparently convalescent. She was soon able to sit up, though strictly enjoined to use no further exertion; her diet was rigidly prescribed, her appetite being exceedingly good; her bowels regularly influenced by medicine, and the secretions healthy. Indeed the strictest attention was paid to every thing connected with her re-

* Account, &c. p. 128.

storation to health, and I observed nothing to call forth particular notice until the 17th and 18th of the following February, when she complained of great nervousness, being giddy, and occasionally forgetful of what she was saying or doing. I attributed this, at the time, to her weak state, and to a little alarm which had been excited by my having been called in during the night of the 15th, to attend the principal member of the family, who had become suddenly indisposed. I prescribed for her small but frequently repeated doses of ammonia, opium and camphor, and a mild aperient, which had apparently the effect of quieting her.

"On the forenoon of the 19th of February I was requested to visit her immediately, but having left home early, I did not receive the message till the afternoon, when I found her in a state of the greatest excitement, with flushed face, hurried respiration, rapid pulse, incoherence of conversation, and confusion of ideas, amounting to mania. I had some difficulty in pacifying her to obtain from the alarmed attendants an account of the accession of these symptoms. It was represented that she had passed a restless night, and awoke in great agitation, and exclaimed she had seen a vision in her sleep, which impressed her with the idea of her having but six hours to live. Under such a delusion, she prevailed upon her attendants to send for a clergyman to administer to her the sacrament, who, seeing the state of her mind, very properly refused the rite. This was a new source of hallucination. She then could not die till I had seen her; and on my visit she hailed me as her deliverer from "the toils of earth and the wiles of Satan." I succeeded so far, by gentle persuasions, and unlimited promises to endless entreaties for relief, as to prevail on her to return to her bed. I prescribed sedatives, and enjoined quiet and restraint. In the evening of this day you saw her, and will, I have no doubt, recal to your memory the religious madness you witnessed, which although wild and fantastical in the extreme, still bore traces of a mind stored with something like sacred philosophy, as well as history, blending and confusing the real and the unreal with a skill which would bid defiance to the powers of a sane and superior order of intellect. A state of watchfulness, with con-

stant raving, supervened during the night, notwithstanding the administration of thirty minims of the tincture of opium, and fifteen minims given every two hours. The hair was cut off, and a spirituous lotion applied to the head.

"20th.—Symptoms little or not at all relieved. The opium was given in effervescing draughts, to allay the influence it had exercised on the stomach.

"21st.—Symptoms the same. Opium prescribed in substance. Cold applications to the head.

"22d and 23d.—State of the mind, if any change, rather more tranquil. Aperients required. Cold to the head. Opiates. Circulation more quiet, with more power.

"For a few days the state of the patient was almost stationary, the treatment remaining the same. About the beginning of March there was evident amendment in the bodily health, and the mind was susceptible of reasonable impressions, if made with calmness, perspicuity, and firmness; indeed, being given to understand that her health was now depending for restoration entirely upon the quiet of her mind, she exerted herself amazingly, and is now gradually recovering, being able to sit up for some hours during the day. Good appetite; quiet nights, with refreshing sleep; and muscular power daily improving.

"Will you allow me one minute more, my dear sir, to mention an anecdote displaying at once the power and the mystery of mental operations. Our patient was asked, in one of her raving fits, what o'clock it was? "So, so," was the reply, "and you asked me because you thought I was mad, and could not tell you." The clock was opposite her. She answered correctly, divined the meaning of her interrogator, and lapsed immediately into a train of maniacal wanderings.

"I remain, &c."

Upon this case I would observe, first, that had there been no pneumonia, a chlorotic person would not have borne so well the immediate loss of blood; and, secondly, that had there been no such loss of blood, there would have been no such occurrence of mania. At least such are the conclusions from the case, drawn both by Mr. Yates and myself. The case illustrates, therefore, many points of great practical importance.

Delirium, or transitory mania, from

loss of blood, is an occurrence of great frequency. That continued mania may arise from a similar cause, is not less true, though it be less frequent.

Puerperal mania was, then, first traced to exhaustion as a principal, not the sole cause, by myself. There are abundant proofs of the fact in Dr. Gooch's admirable volume. That other causes co-operate, is equally true. This is frequently the case with intestinal derangement; and that the state of the sexual system has much to do with it, is obvious from the mere fact and character of that form of the disease termed nymphomania.

I am, sir,
Yours very truly,
MARSHALL HALL.

14, Manchester-Square,
Nov. 7, 1831.

AN INQUIRY
REGARDING
THE CAUSES OF PHENOMENA.

BY

WILLIAM ADDISON, F.S.L. &c.

Surgeon to H. R. H. the Duchess of Kent, at
Malvern.

*To the Editor of the London Medical
Gazette.*

SIR,

THE following observations were not intended for immediate publication; but as I have, in common, no doubt, with most of your readers, been much interested by the remarks of Dr. Prout and Dr. Philip, and as they have some bearing on the matter at issue, I place them at your disposal. I need hardly observe that they were written before the remarks referred to were published in the Medical Gazette.

I am, Sir,
Your obedient servant,
WILLIAM ADDISON.

P.S. I have not altered the word *metaphysical*, to which I perceive Dr. Philip objects.

THE elements of all human knowledge are to be found in the phenomena or effects exhibited by matter to the organs of the senses. These phenomena are arranged into three classes—physical,

vital, and intellectual, and by reflecting upon them, we are led to contemplate their causes in certain energies or forces from which, in conjunction with matter, they seem to arise. But it will be necessary, before we proceed to the consideration of these forces or causes, to shew the different senses in which the word *cause* is used:—1stly, it is applied to various occult and metaphysical energies or forces, such as the electric fluid—magnetic fluid—principle of affinity—nervous fluid—vital principle—organic agent, &c.;—2dly, it is given to any phenomenon which invariably or generally precedes another, as when we say, heat *causes* fluidity—atmospheric pressure is the *cause* of the Torricellian vacuum—gravitation is the *cause* of atmospheric pressure—wind *causes* the waves of the sea—variation of temperature *causes* wind, &c. In these latter instances we find that certain phenomena, because invariably or generally followed by certain other phenomena, are entitled *the causes* of those succeeding. Now the term *cause* here is applied in a way very different from that in which it is used in the instances first mentioned; for we say *atmospheric pressure is the cause* of the Torricellian vacuum, and also *atmospheric pressure is the effect* of gravitation—wind is the *cause* of the waves of the sea, and wind is the *effect* of variation of temperature; so that here these phenomena play a double part, standing as *causes* to those phenomena which succeed, and as *effects* of those which precede: in these instances, then, the word *cause* merely implies that such and such phenomena invariably or generally precede such and such others.

When we treat upon the subject of cause and effect philosophically, it becomes a matter of some moment not to lose sight of this important distinction; and it would tend much to simplify a difficult inquiry, if we were to lay aside the term *cause* when referring to the *order of succession* in phenomena: thus, if instead of saying, the motion of a steam engine is *caused* by steam—steam *caused* by heat—heat *caused* by chemical decomposition, and chemical decomposition by chemical affinity, we were to say, the motion of the engine is *preceded* by the production of steam—steam *preceded* or accompanied by heat—heat by chemical decomposition, and so on, the doctrine of causation would be placed upon a more intelligible basis,

and no phenomenon or object of sense would then be admitted among *causes*; for this term would (under this, as I conceive proper limitation,) always refer us to something preceding every phenomenon, and therefore something very different from any phenomenon or effect whatever.

Subject to this limitation, then, I shall proceed to develop our notions regarding causes, and endeavour to point out the influence these notions produce upon our philosophical pursuits and experimental researches.

With regard to physical phenomena, we speak of *force of gravitation—principle of attraction—electric fluid—magnetic fluid*, &c. as though there were many recondite agents or metaphysical forces engaged in the production of these alone; but the progress of scientific pursuits, and the discovery of many previously unobserved phenomena, have developed so many intimate connexions, that we are induced to believe all physical phenomena subject to one pervading cause, force, or energy, operating in different ways, determined by differences in the conditions or nature of the matter through which they appear.

I consider it unnecessary to detail the several discoveries by which our notions respecting the causes of physical effects have become so generalized as to induce us to admit, that they arise from *one pervading force*, which regulates, at the same time, the motions of those immense masses revolving through space, produces the effects of electricity and magnetism, and controls those extraordinary combinations and arrangements observed in our chemical pursuits; it is sufficient to remark, that every new discovery and every new fact confirms the idea. Whether this all-pervading force, controlling physical phenomena, can exist independent of matter, it is impossible to know, or what matter would be without it, useless to inquire; but as long as they are conjoined, nothing can be more incorrect than to speak of matter as inert. Because we do not see particles and masses wheeling about or stalking around us—because we do not witness one body rising up, and moving towards its neighbour, we content ourselves with regarding them as composed of inert materials; while we neglect the movement of atoms in every chemical change—the marching of particles in crystallization seek-

ing light—and the strides of masses in their revolution through space, and forget that the motionless state of matter which we notice, is one of the most obvious effects resulting from that cause, force, or energy, with which it is endowed. But are we to believe, I would ask, that the operations of this active and *powerful cause* stop here; and to suppose that, although capable of producing all the wonderful phenomena of inorganic matter, it is entirely superseded by another more energetic in the phenomena of vitality? are growth, function, and nutrition, in organized bodies, under the dominion of a totally different cause from that giving rise to gravitation, polarity, and crystalline form? A negative to these questions seems indicated by those striking connexions between physical and vital phenomena, which the progress of science daily reveals, and forces upon our notice. The growth and nutrition of vegetable and other organic matters differ much, it is true, from most physical phenomena; but so does the formation of a salt differ from the combustion of coal; and if, in the course of our inquiries, we see reasons to refer such dissimilar effects as electricity and crystallization to one great metaphysical cause, what are the reasons why we should deny its operation in the formation of organized structure? The analogy between this structure and crystallization may be much more complete than we usually imagine. A crystal cannot increase in size, or become perfect in its form, without being furnished with saline particles under certain peculiar conditions: in fact, a crystal may form in a solution, may continue to increase to a certain bulk, and then diminish or dissolve: in the former case it may be said to grow or be nourished; in the latter, to die or be starved. A plant, to grow and flourish, requires a certain quantity of moisture; and, as long as the required supply is furnished, it thrives, but, deprive it of this, it languishes, and ceases to live; in fact, plants do not possess the power of overcoming the more ordinary physical affinities in a much greater degree than a crystal; for, as a solution, in which the latter is beginning to dissolve, may contain many saline particles, so the earth and the air, in which a plant is dying for lack of moisture, frequently contains a very notable quantity: the

crystal is unable to combat the affinity of the water for the saline particles, and the plant cannot overcome the attraction of the earth and air for moisture.

Crystallization, it may be said, bears no affinity to vegetation, inasmuch as it is merely the aggregation of particles existing previously in solution, and in nowise altered by the process; but this only shews that crystallization is a more simple species of vegetation than that exhibited in the growth of organic matter; for a vegetable body can create nothing new, its productions resulting only from the aggregation of certain particles existing in the soil, air, or water, by which they are surrounded. Still, it may be remarked, that we can accomplish nothing similar to the productions of organic matter by chemical manipulation. This, however, is not altogether true, for some of them have been produced in the laboratory, and probably ere long there may be many more. The efflorescence occurring on the surface of iron pyrites, or alum slate by exposure to the air, and the arboriform production of lead when zinc is suspended in its solution, are phenomena allied to vegetation: decompositions and re-arrangements of particles in these instances take place, presenting all the phenomena embraced by the term growth. From a mingled solution of two or more salts, the crystals of one or more may be obtained separate and distinct by due evaporation and rest: here, then, a crystal may be said to grow out of the solution, certain particles joining themselves together, to the exclusion of others. But living matter, it may be said, is capable of controlling the operations of the cause of physical phenomena; so we find it in bodies of a crystalline structure. A piece of alum suspended in water is not equally dissolved at all points; the same power which preserves the vegetable form from decay, also preserves the crystal from solution, so that we find the surface of the alum unequally acted on, the resisting points presenting a regular and determinate arrangement. If we place any inorganic substance within the influence of electric currents, by a galvanic apparatus, we find its ordinary habitudes and relations destroyed or interfered with, as when an acid passes through an alkaline solution without combination: in this case our instru-

ment produces an effect analogous to that which the vegetable apparatus does, conferring on particles of matter new affinities and new powers of combinations, or, to speak more correctly with the views herein taken, directs the power controlling matter into new channels and new methods of display. The great and powerful influence of light upon many physical and almost all vital phenomena, is another point of connexion not to be forgotten: it induces many chemical combinations, and materially promotes the process of crystallization; it is essential to vegetation, and even animals become sickly and diseased if deprived of it for any time; many saline crystallizations comport themselves with light in a way precisely similar to vegetation, turning towards and seeking the direction in which it comes. The phenomena of endosmose and exosmose also point out various intimate relations between physical and vital effects not to be overlooked or forgotten.

The surface of the earth, I conceive, may be regarded as the bottom of a capacious vessel spread over with various kinds of matter, in contact with superincumbent fluid; while vegetation may be considered as an efflorescence protruding from the ground, and matured in the air in a manner analogous to crystals in a solution, or on the surface of alum, slate, or pyrites—certain forms of matter, in both cases, shooting up or growing with fixed or determinate figures, the elements of which exist partly in the substance or seed forming the basis from which they arise, and partly in the air and moisture surrounding them. Crystals will not grow or appear on alum slate without air and a certain quantity of moisture; so a seed will not germinate, nor vegetation continue, but with the same matters. Alum slate and pyrites contain materials, the chemical repose of which is disturbed by a little air and a drop of water; so a seed may be likened to a little case containing matters so delicately balanced that a little air and a little water overturns their affinities, and germination takes place. In the case of the alum and pyrites, too much water interferes with or stops the process of crystalline efflorescence; so in the seed, too much moisture is unfavourable to vegetation, and, instead of promoting the peculiar changes of the plant, renders their component matters amenable to the more

ordinary phenomena of corruption and decay.

It was by slow steps that the connexion between the various, and at first view, totally dissimilar physical phenomena, were pointed out. Electricity and magnetism for a long time stood aloof from each other, and from the rest of their class; so, therefore, we ought to expect that many hitherto unobserved phenomena must present themselves, before the relations in which physical and vital phenomena stand with regard to each other, are fully acknowledged or understood, notwithstanding the progress of science is daily narrowing the breach, and forcing upon our notice important connexions between the operations of organized and inorganic matter. Nevertheless, as recent discoveries have brought together under *one cause* the dissimilar physical phenomena, so have we every reason to anticipate the same kind of union between these and vital phenomena; so that instead of two distinct classes of effects, with distinct hypothetical and metaphysical causes, we shall find that all we witness agrees in obeying one principle, controlling the phenomena of the world*.

There are some persons, perhaps, who may admit, that the phenomena of organic and inorganic matter have many important connexions, and that *one cause* may operate throughout, who, nevertheless, are not so far influenced by the consequences to which such an admission leads, as to perceive that the terms in constant use to designate the causes now allowed in our reasonings and researches, such as organic agent, nervous fluid, vital principle, electric fluid, magnetic fluid, principle of attraction, &c. are arbitrarily used, serving only to link together a few of the more obviously connected phenomena, and as applicable to one set as another: thus we appropriate to the *organic agent*, circulation, secretion, and nutrition, in animal bodies; to the *vital principle*, the phenomena of vegetation; to the *electric fluid*, electrical phenomena, &c.

But it must be clear that as these terms refer us to agents of which we know nothing, except by the phenomena pre-

senting themselves to our senses, so, therefore, any distinction or difference between them must rest solely upon the distinctions and varieties of these phenomena; and, consequently, according as connexions and relations between different classes of phenomena are discovered or pointed out, so will our terms applied to causes coalesce or diminish, as appears already in the instances of electric and magnetic fluids, principle of attraction and chemical affinity; so many important relations between the classes of phenomena, at the head of which these stood as *causes*, having been discovered, that we are brought, as I have before observed, to refer them to *one active principle*. Under these circumstances, it must evidently be unscientific to declare positively, that there can be any difference or distinction between the object referred to by the terms, organic agent, vital principle, or electric fluid; on the contrary, from what is daily occurring in the progress of philosophical pursuit, we are led to anticipate that these terms, although now (from our not having discovered sufficient connexion between the classes of phenomena to which they stand as causes) they appear to refer to forces or energies distinct from each other, may be traced (when more *hitherto unobserved phenomena* become known) to *the one cause* operating throughout. Indeed, even in these latter instances, such connexions are discovering themselves between the several classes of phenomena at the head of which they stand, that we hear persons affirming that electric fluid, galvanic fluid, nervous fluid, and organic agent, denote the same power; thus, in fact, acknowledging it a mere question of convenience whether we apply the term electric fluid, principle of polarity, vital principle, nervous power, or organic agent, to the cause.

The utility of these speculations upon the connexions between phenomena, as modifying our notions of *causes*, and as leading to the existence of *one energy* prevailing throughout, may not at first view appear; but we should remember, that all our observations and modes of expression in scientific matters, as well as our experimental researches, are materially influenced by our opinions respecting the *causes* of phenomena, and therefore that we ought, where both sides of the question are involved in ob-

* The reader will understand that my observations relate only to physical and vital phenomena. Intellectual phenomena and mind, may form subjects for future consideration.

security, to keep to that which tends most to stimulate us to further exertion. If we acknowledge but *one cause* for physical and vital phenomena, we shall eagerly enough seek for the connecting links between them, instead of slowly and reluctantly admitting those which begin now to forcibly arrest our attention. A reluctance arising from the idea that *the cause* of vital effects is very different from that controlling physical phenomena, and beyond the reach of experiment, is what tends very much to retard our advancement in the higher branches of philosophical inquiry, because it leads us to neglect those connexions or relations between dissimilar phenomena, which will doubtless reward the labours of industrious inquirers.

ABSCESS OF THE BRAIN OPENING EXTERNALLY.

To the Editor of the London Medical Gazette.

SIR,

I HEREWITH transmit a case of abscess of the brain, occurring in a boy eight years of age, which, if you think worthy of record in your very valuable publication, is much at your service.

I am, sir,

Your obedient servant,

S. G. LAWRENCE,

Surgeon, Royal Military Asylum, Chelsea.

Nov. 21st, 1831.

Duncan McCraig, aged eight years, was admitted into the Royal Military Asylum, Chelsea, on the 24th of October last, when he appeared to be in good health. On the following day he was brought to the hospital, on account of refusing his food. He made no complaint but of want of appetite; had no febrile symptoms. Tongue clean; pulse natural. A purgative was given; and on the 26th he was dismissed, having no apparent complaint. On the 31st October he was again brought to the hospital, still having no constitutional disturbance; but it was observed that he was inactive and dull, continuing to refuse his food; he now also complained of headache, particularly on the left side of his forehead; and at this part, about an inch above the superciliary ridge of the frontal bone, there was a small in-

flamed boil, from which a few drops of pus were pressed out. On being questioned whether he could account for the pain in his head, he said he had had a fall in the street a week before his admission here, and struck the left side of his forehead against the ground; but the skin was only slightly grazed, and he neither felt sick at the time nor afterwards. His tongue was clean; bowels torpid. A dose of scammony and calomel was given him, and a bread poultice applied to the boil on his forehead. In three days the boil was healed, very little discharge having come from it, and only a small cicatrix remained.

During this time he did not complain much of his head, and was walking about the ward like the other boys with trifling complaints. No alteration took place until November 6th, when febrile symptoms appeared; the tongue was slightly furred, skin hot, and increased pain in his forehead. Leeches were applied to the temples, a brisk cathartic of jalap prescribed, followed by saline medicine. There was nothing unusual in the alvine discharges. This was the first day that any serious affection of the head was indicated.

7th.—The febrile symptoms were much moderated, and he said his head felt better; he dozed a great deal, and disliked to be disturbed, but was perfectly sensible, and the pupils of his eyes were of natural appearance.

8th.—This morning his pulse was found to be remarkably slow (60 in a minute); tongue more furred, and his bowels were torpid. He did not complain much of his head, but when he did he always pointed to the left side of his forehead. An emetic of ipecacuanha was prescribed, followed by calomel and rhubarb. At the evening visit, seven o'clock, the emetic had operated gently, but little was discharged; and he had had two scanty alvine evacuations. Pulse 60, small, as in the morning; skin of natural temperature, and he appeared to be in a tranquil sleep. During the night the nurse, hearing him make a moaning noise, went to him; he was perfectly sensible, spoke to her, and said he did not want anything. At six o'clock the following morning (Nov. 9th), the nurse found him dying, and he soon after expired.

Examination of the body 29 hours after death.—On reflecting the scalp

from the bone, particular attention was directed to the left side of the os frontis, where the boy had complained of most pain. A small carious perforation of the bone was there perceived, about an inch above the superciliary ridge, which would admit a small-sized probe; and the bone round this hole had a dull red appearance, apparently from increased vascularity. The internal surface of the scalp had a small dimple-like depression, corresponding to the hole in the bone, and the minute cicatrix on the skin of the forehead, left by the healing of the boil. The calvarium was now removed, and which adhered less strongly to the dura mater than is usual in young subjects. On the internal surface of the os frontis a small prolongation, about the thickness of a probe, was seen proceeding from the dura mater to the perforation in the bone, resembling a vein or vessel entering it; immediately above which was a small spot of ecchymosis on the brain, about the size of a sixpence. The dura mater was now reflected; no particular vascularity or turgescence of the vessels of the brain was observed; but the anterior lobe of the left hemisphere appeared of a straw, or greenish-yellow colour, evidently denoting the site of an abscess, with distinct fluctuation when pressed. The posterior lobes were of natural appearance, as well as the whole of the right hemisphere. The right side of the brain was now sliced down to the lateral ventricle, on opening which a small quantity of limpid fluid escaped. Attempting to do the same on the left side, the brain gave way, and about *two ounces* of bland inodorous pus gushed out, and the cyst of an abscess became apparent. The posterior part of the left lateral ventricle was also found filled with pus; but it was difficult to say whether it had existed there prior to death, or took place from the bursting of the cyst of the abscess during the dissection. The thinnest part of the cyst was at the anterior part, near the ecchymosed spot, just above the perforation in the frontal bone. The cavity of the cyst was vascular, of a dark red colour.

The cerebellum was natural. The thorax and abdomen were examined, and the viscera of both these cavities were in a healthy state; but there was a small *diverticulum ilii*, or preternatural pouch, three inches in length, pro-

ceeding from the intestinum ileum, at the distance of fourteen or fifteen inches from its termination in the cæcum—a *lusus naturæ* which, I believe, is not very common. A somewhat similar malformation, however, has once before occurred to me (among numerous post-mortem examinations), and which is recorded in the Edinburgh Medical and Surgical Journal of October 1830.

REMARKS.—The efforts of nature to make an outlet for the matter in this case are curious, and well worthy of observation. A little process or prolongation, like a duct, of the thickness of a probe, extends from the abscess and dura mater to the frontal bone, through which a perforation is made, by the action of the absorbents, to the integuments of the forehead, where a small boil forms and breaks, thus making a direct external opening, communicating with the internal abscess in the brain, which, consequently, may be said to have broken externally.

I think this must have been a chronic abscess of the brain, originating independently of the fall, although that accident may have accelerated the fatal termination; for it is scarcely probable that so large a collection of matter could have formed, and made its way externally, in the manner above stated, in so short a space of time as from the date of the fall (about three weeks), and with so little constitutional irritation.

His mother being questioned about his fall, corroborated the boy's statement, and said that it was only a slight cut or graze; a piece of plaister was applied, it healed in two or three days, and she did not consider the hurt of any importance; but added, that he was always a very delicate child, not only as regarded his food, but in many other respects, and was possessed of great mental sensibility. Having lost his father about three months ago, he had fretted much on that account, and never was playful like other children subsequently to his father's death. With regard to the suddenness of his decease on the morning of the 9th November, may it not have been owing to the abscess having suddenly burst into the left lateral ventricle? for it is to be remarked, that there were no symptoms of effusion on the brain, no dilatation of the pupils, no coma, the boy being perfectly sensible to the latest period at which he was seen prior to death.

ON THE USE OF DRY HEAT-BATHS
IN CHOLERA.

—
To the Editor of the London Medical Gazette.

SIR,

FOR the peculiar symptoms of cholera, heat seems to be regarded by most of those who have seen the disease as clearly indicated. It would be a waste of time to particularize those symptoms, as they are now familiar almost to all persons; and I take it for granted, that heat is generally allowed to be a means of remedy tending to remove them. Nevertheless, some practitioners have recorded their opinions, decriing the use of baths altogether for this disease, and assign as a reason, that they have not been successful when resorted to. This does not militate against the principle of the usefulness of heat, so clearly indicated; it only goes to prove the awkward and insufficient way by which, as a remedy, it has been applied, for as no satisfactory method has been hitherto brought before the profession, the true value of the remedy has not yet been ascertained. It is, however, a sufficient apology for those gentlemen who condemn the use of heat-baths, because much very valuable time would be lost by their ineffectual application, and consequently disappointment to the practitioner and his patient be the result.

My only object in these few sentences, is my desire of contributing some little share of usefulness at this crisis, and I hope no person will attach to me any invidious or sinister motive; for, not being a manufacturer or interested vendor of baths, such charge would be unfair. The possibility of misconstruction, however, ought not to deter me from my purpose, and I say, without hesitation, that all those baths which I have hitherto seen and tried are quite unfit for the intentions designed from their use; without exception, they are all on a wrong principle, and contrary to the laws of science as they apply to heat. Moreover, they are constructed so rudely, from a desire of making them cheap, as to tend greatly to make them useless, although, when properly constructed, they cannot be dear. In addition to the imperfections and drawbacks alluded

to, there is a greater to be named—they are by no means free from danger. During the last fortnight, two respectable and zealous manufacturers have declared to me the danger they have run of setting the bed on fire; and in my various trials, although my servants are much accustomed to these matters, a similar accident has been nearly occurring twice; nor have they, or even myself, escaped without burns. My various and persevering trials have, however, led me, I believe, to the construction of a portable calorific apparatus, free from the before-named objections. Some persons whose characters stand high for scientific knowledge, have seen and proved it, and think it leaves nothing more to be desired for the purposes expected from its use.

Before concluding, allow me to say, that in cases of cholera I conceive the hot air-bath must have many advantages, from its occasioning increased nervous influence and much determination of the circulating fluids to the capillary vessels of the skin. But, even in a hot air-bath, when heated by spirits of wine, there comes over much gaseous vapour, from a portion of water contained in the spirit, which condenses on the skin, leaving a clammy moistness; and which I have often known to be mistaken for perspiration, when none has really been excited, from want of a sufficient heat. The person so misled, has a feeling of chilliness on the surface of the body, no real transpiration or perspiration having taken place.

I wish not to encroach on your valuable space, and therefore at once conclude, remaining

Your most obedient servant,

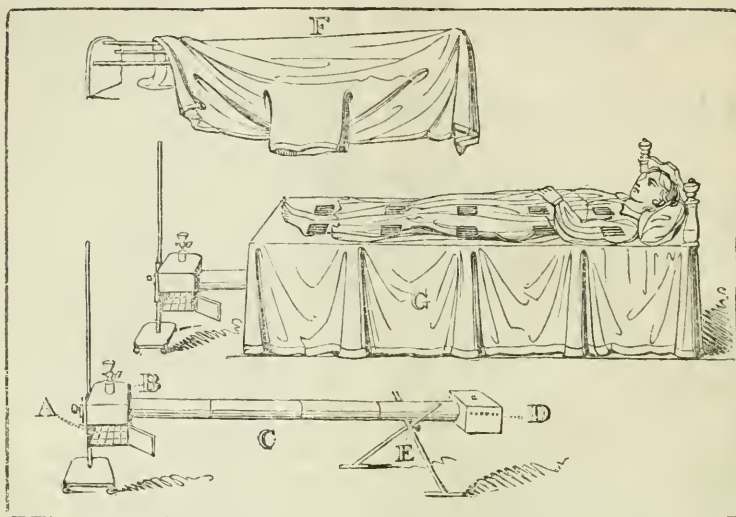
JONATHAN GREEN.

40, Great Marlborough-Street.

—
Mr. Green's Calorific Apparatus, with suitable Appendages.

A, is a double hot air chamber, which can be raised at pleasure, through the door of which a tea-cup, or similar vessel, containing strong spirits of wine, is put to be ignited.

B, a moveable chamber, over which is a small funnel to hold a little water, which may be allowed only to drop by turning completely round the stop-cock, which connects the upper hot air



chamber with the funnel. The vapour immediately generated thereby forces itself through the double hot air tubes. This part need scarcely ever be used, if spirits of wine is burnt, from the portion of water it contains.

C, double hot air tubes; outside circumference twelve inches.

D, a square disperser of the heat, with holes on the upper part of its front, and two sides. The whole is made of sheet iron.

E, a forked crutch, to support and adjust the height of the tubes, by means of a screw attached to it. It is desirable that the tubes should not be smaller, and that wicks common to lamps should not be used, if an *effective hot air-bath is the object*.

G, a design for a suitable bedstead, which folds with hinges and has moveable legs; the squares represent holes in the sacking underneath the patient. The calorific apparatus is shewn as introduced at the foot of the bedstead, but it may also be applied at the side. Two persons extending a sheet at the ends, might hold a patient up, whilst a third withdrew the bed and cut holes in the sacking;—this would make any bed answer the purpose.

F is a frame, not more than nine inches high, with rods connecting the two ends; to make them portable they may be jointed, as a fish-rod, in the middle. Over this is a covering, with a large ooze on each side, to admit of

extensive friction with liniments, &c. at the time the patient is submitted to the hot air, constantly arising from beneath. The materials are all cheap, while the apparatus acts quickly and powerfully, and is *safe*, constituting a hot air-bath, or that combined with vapour, without trouble or alteration.

MEDICAL EDUCATION.

To the Editor of the London Medical Gazette.

SIR,

I HAVE read with considerable pleasure that portion of the letter of Mr. C. Aston Key which relates to provincial schools of medicine and surgery; for I agree most cordially with this able surgeon in all his views of the great advantages which they are calculated to afford, both to students and practitioners; and I feel assured that these institutions, becoming more extensively known through the pages of the *Medical Gazette*, will receive that public attention and patronage to which their great merits and utility entitle them.

Previous to the year 1816 no medical schools existed in any other part of England than the metropolis, and even in London itself they were organized on a less extensive plan than they now are. Since that year schools of medicine have

been established in Manchester, Birmingham, Sheffield, Bristol, Leeds, and Bath; and very lately a beginning has been made towards the establishing one at Hull also. In each of these, excepting that of Bath, students are now instructed in every branch of medical science; and I have particular satisfaction in bearing testimony to the ability and liberal acquirements which candidates have evinced when under examination, who had received the whole of their medical education in these schools.

It is interesting to note the particular period at which these new schools originated. The termination of the war, the consequent diminution of employment for medical officers in the public service, and the return of a considerable number of these gentlemen to private professional life, led, about this time, to a considerable reduction in the number of students; and if some additional stimulus had not been afforded, instead of additional schools being required, those already in existence would have sustained a loss of their accustomed support. That stimulus was afforded by the passing of the Apothecaries' Act; and it is to that event we may justly attribute the great impulse which has been given to the systematic study of medicine in this country, and the consequent necessity for the establishment of new schools and professorships for the education of the general practitioner.

It is very well known that, previously to this time, the attention of the great body of students was chiefly, if not exclusively, directed to surgery; and that an attendance on one or two courses of anatomical lectures, a single course on surgery, and a few months' desultory and superficial attendance on the surgeon's practice of an hospital, constituted the whole of the education of the majority. A few, indeed, attended lectures on the practice of medicine, and a still smaller number lectures on chemistry; but *materia medica* and therapeutics were almost wholly neglected, and the medical practice of our hospitals entirely so. Pupils generally commenced their attendance on lecturers in October, and left town at the end of the following April, during which short period for *walking the hospital* some popular surgeon was followed through the wards by a crowd of admiring tyros, to the greater number of whom both the patient and the surgeon were often alike

invisible; and if an operation of rare occurrence, even in a metropolitan hospital, and but little likely to occur in private practice, was to be performed, the operation room was crowded to its remotest parts, while at other times the wards of the hospital were deserted, and the physicians' practice uniformly neglected; indeed, the physicians rarely had a single pupil*, as the instructive field of observation under their care was not even entered on.

A course of study so little calculated to fit students for the duties of their profession, required alteration; and the Court of Examiners which was appointed under the provisions of the act of Parliament, applied themselves to the performance of this very important duty with zeal and discretion. They were well qualified by their talents and experience to superintend the education of youth destined to practise in the same grade of the profession as themselves; and they were appointed to this task not "by the accidental operation of the law," as stated by Mr. Key, but by the clear, express, and well-defined directions of the legislature itself.

Although fully sensible that medical education was susceptible of much immediate improvement, the Court of Examiners were, nevertheless, unwilling to make precipitately any extensive alterations; the changes have consequently been progressive and slow, each additional demand on the student for an increase of labour having arisen out of a full conviction of the necessity for it, and after its probable effects had been carefully considered; nor has any change of the least importance been ever made, or even contemplated, without much previous inquiry and anxious discussion, or without consultation with many of the teachers engaged in lecturing.

Mr. Key's insinuation, therefore, that

* At one of the largest hospitals in this metropolis (St. Bartholomew's), it was generally understood that no pupil, except a dresser, was permitted to enter to attend the physicians' practice, unless he either intended to graduate as a doctor of medicine, or had actually done so. It was in consequence of this that the Court of Examiners at the Hall were originally induced to admit attendance on dispensary practice into their course of education. I have learned from a source upon which I can rely, that, previous to 1816, the physician's practice of this hospital was seldom attended by more than one pupil, on the average, yearly, whereas now the physicians have between 70 and 80.

the lecturers have been treated with less "deference" and "courtesy" than they were entitled to expect, might unquestionably have been spared; and the only excuse I can find for this indirect accusation is, that Mr. Key, as a lecturer on surgery, over which branch the Court of Examiners exercise no influence, was not himself consulted, and hence inferred that other lecturers were not consulted either. To his complaint that too much labour is now imposed on the pupil, "*by compelling him to attend all the lectures, not within the space of two years, but of two winter seasons,*" I reply, that no such compulsory regulation exists; on the contrary, he has full liberty to attend the required lectures during any part of his apprenticeship, or to diffuse his attendance on them over the whole period of it. The Court consider that the apprenticeship was intended to be a time of professional study; they wish it to be so employed; and they trust that the parties more immediately interested will make such arrangements for the profitable use of it as are consistent with the end in view*. Though some of the lectures can be attended with most advantage in the winter season, there are others which may certainly be attended with equal advantage, and more convenience, in the summer; and if the Examiners have not yet required that midwifery, forensic medicine, and botany, should be studied at that time, it has arisen from a reluctance unnecessarily or prematurely to interfere; and it may readily be imagined, that they did most confidently expect that lectures on the latter subject, at least, would have been very generally delivered at that season, as adequate facilities for the study of it

have been afforded by the opening of the Society's botanic garden at Chelsea. If no lectures on this subject are given during the proper season at the school of Guy's Hospital, that at least is not the fault of the Court of Examiners.

Mr. Key has given a tabular view of the arrangement of lectures, &c. &c. at Guy's, and, by a little mystification of the subject, has endeavoured to lead his readers to infer, that students were unfeelingly oppressed by an unreasonable imposition of labour, as it would appear by that table that they were required to attend the schools for twelve or thirteen hours daily. To do away with this fallacy, and at the same time to point out such an arrangement of his time as I think it desirable a student should make, whom circumstances oblige to attend all the required lectures, as well as the hospital practice, within the space of two years, I will intrude on your pages, and perhaps on the patience of your readers also, by a tabular view; and I will adopt for that purpose the arrangement of the classes at Guy's Hospital, not because it is the best, but as Mr. Key's table is before me, I find it convenient.

The Court of Examiners require of—

Chemistry,	}	Two courses each;
Materia Medica,		
Anatomy and Physiology,		
Demonstrations,		
Practice of Medicine,		
Midwifery,	}	One course each;
Of Botany,		
Forensic Medicine,		

and hospital practice (with clinical instruction), twelve months' attendance.

As the academic season begins in October, the student would attend thus, during—

The first half of the first Winter Season :—

		M	T	W	Th	F	S
†Materia Medica	½ past 9			—			
Chemistry	¼ before 10		—		—		—
Demonstrations	at 11	—	—	—	—	—	—
Anatomy	2	—	—	—	—	—	—
†Materia Medica	7		—			—	
Number of Hours daily		2	4	3	3	3	3

* I select the following passage from the introductory observations to the last regulations of the Court:—"Parents, in selecting a practitioner with whom to place their sons, should ascertain that he is legally qualified to practise as an apothecary, and also satisfy themselves that the nature of his engagements will permit him to regulate and superintend the studies of his pupil. A systematic

course of study should be arranged, by which the pupil might be conducted progressively from elementary principles to the practical observance of disease, neither wasting his time by exclusive attention to pharmaceutical manipulations, nor employing it with as little profit in a premature attendance on the sick.

† Mr. Key has not accurately stated the ar-

The second half of the first Winter Season, beginning at the end of January or beginning of February :—

		M	T	W	Th	F	S
Materia Medica	$\frac{1}{2}$ past 9			—			
Chemistry	$\frac{1}{4}$ before 10		—		—		—
Demonstrations	at 11	—	—	—	—	—	—
Anatomy	2	—	—		—		—
Practice of Medicine	$\frac{1}{2}$ past 3	—	—	—	—	—	
Materia Medica	at 7		—			—	
Hours		3	4	4	3	4	3

During the first period, it will be seen that the student has attended, on the average, three hours daily; and in the second, three hours and a half.

At the close of the season, he may

return to his home for recreation, and reading on the various subjects of his winter studies; or if resident in London, or in the neighbourhood of a provincial school, he may then attend to botany.

First half of the second Winter Season, beginning again in October :—

		M	T	W	Th	F	S
Examination of Patients for Admission...	at 10			—			
Physicians visit the Wards.....	12	—	—	—	—	—	—
* Demonstrations of Morbid Anatomy	1	—	—	—	—	—	—
Practice of Medicine	$\frac{1}{2}$ past 3	—	—	—	—	—	
Hours		3	2	4	2	3	2

During this period, the attendance averages less than three hours daily. It would be desirable, therefore, that, although the Court of Examiners do not require it, the student should employ himself in dissections.

The student has now arrived at the end of January of his second year; has attended all the lectures required of him, excepting two courses of midwifery and one on forensic medicine; and has yet eight months left to devote to these purposes, to his hospital practice, and to reading and reflection, before he can be permitted to present himself for examination. With this statement before him, I confidently ask Mr. Key what spirit would be depressed, what constitution impaired, by the most strict and diligent attendance on each lecture? I presume that a student comes to London with the intention of applying himself zealously

to his pursuits; if so, after allowing eight hours for sleep, and four hours for his lectures, he has yet twelve hours daily at his disposal for private study, recreation, exercise, and meals. In tracing out this course of employment, I may be told that I have taken no notice of surgery, which ought to occupy a large share of the student's time: to this objection I might very fairly reply, that Mr. Key has himself been most cautiously sparing of his observations on this department, and the regulations for the study of it as enjoined by the Royal College, regarding it, I imagine, as *holy ground*, which it might be imprudent in him unadvisedly to intrude upon or disturb. But I may be permitted, I trust, to remark, that the student, by his attendance on the lectures required at the Hall, is storing his mind with knowledge which will most advantageously fit him for the study and practice of surgery, and that by attending the surgeon's practice of the hospital, and giving three hours in a week to surgical lectures, he will perform all that the Royal College requires of him, and not find himself overburthened with employment.

* It appears, by Mr. Key's table, that demonstrations in morbid anatomy are given daily at Guy's Hospital. I may be permitted to doubt the accuracy of this statement, as it appears from the printed list of lectures and fees that nothing is required to be paid for attending them, and I have never heard that such *daily* demonstrations do actually take place.

I feel pleasure in bearing testimony to the truth of Mr. Key's observations on

the improved character of the medical student, taking occasion at the same time to tell him, that the Court of Examiners did not require his assurance of the fact to be convinced that, within a few years, a great change has taken place in the deportment, habits of study, and acquirements of hospital students; and if Mr. Key, notwithstanding his "pretty extensive experience of medical pupils," has not yet become acquainted with the most influential causes of this improvement, he has been much less observant of what has been passing around him than some of his colleagues who discern in these alterations the beneficial influence of the Apothecaries' Act.

I may also observe that, if many now arrive in town with some knowledge of chemistry, botany, and materia medica, as stated by Mr. Key, and to which I would add anatomy also, they are in a great degree indebted for it to the provincial schools, to which Mr. Key has now in the eleventh hour directed attention, but to which the Court of Examiners of the Hall have for *many years* past given their warmest support, from a well-grounded conviction of the great public benefits which would result from their establishment.

I have the honour to be, sir,

Your obedient servant,

JOHN WATSON.

43, Berners-Street,
Dec. 10, 1831.

THE PRACTICE OF MEDICINE, SURGERY, &c.

Among the New Zealanders and Natives of some of the Polynesian Islands.

By GEORGE BENNETT, Esq. M.R.C.S. F.L.S.
&c. &c.

THE diseases which existed among the natives of New Zealand and the Polynesian Archipelago were comparatively few, and most of them assumed a mild character. Among some of the islands the practice of medicine still remains very limited; but at others the list of diseases has increased, the most fatal of which the natives state to have been of recent origin. In part this may be attributed to those whom, from their intercourse with Europeans, have deviated from their former regular course of life, adopted habits to which they had

been previously unaccustomed, and, above all, become addicted to the use of ardent spirits, which alone will bring a long list of diseases in its train.

Dysentery is one of those diseases which I have remarked commits great ravages, more particularly at the islands of Rótuma and Tongatabu, as well as at Tahiti; at the latter island its existence has been attributed (no doubt erroneously) to contagion communicated by Vancouver's ship's crew, when he called there in 1790. At Tongatabu a deficiency of good water will in some degree account for the prevalence of the disease.

At New Zealand and the Polynesian islands (similar, however, to savage nations generally), a person suffering from any disease was supposed to have incurred the displeasure of their gods; their attention was therefore directed to the adoption of such means as would appease their anger. This was done by suitable offerings being made to them, accompanied by prayers. So we read in Scripture of offerings being made to the Deity, to drive away sickness from the land.

The priest thus assumes the *doctorial dignity*; he is well acquainted with the native medicinal remedies, which he administers under the supposed auspices of the gods, attended by suitable incantations. At some of the islands, on the serious illness of a chief, human sacrifices were even offered up; and at Tongatabu a joint of the little finger was regarded as a suitable offering to the offended spirits. At the island of Tahiti, Tama, Taaroatuihono, Eteate, and Rearea, were regarded as the principal gods of physic and surgery; the former was invoked for the cure of fractures and bruises.

At New Zealand and Polynesia I had frequent applications for medicine and advice. Some would readily and with confidence take the remedies prescribed; others again preferred their native remedies, administered by the priests, with the accustomed invocations to the gods; and on those prayers more dependence was placed by the patients, than on the medicine administered; and the cure was usually attributed to the power of the priest, and his influence with the spirits.

Among the natives of Tahiti (during my visit to that luxuriant and beautiful island in September and October 1829), I found the greatest dependence placed

in European medicines; and I believe they now rarely or never have recourse to their native remedies. They seemed to me very partial to the antiphlogistic plan of treatment; whatever the disease might have been, they considered cathartics and venesection indispensably requisite: a very limited materia medica would therefore be required by a Tahitian practitioner. So far was this carried, that natives in perfect health frequently requested me to bleed them; the reason they assigned for which was, that "when they were sick, I might not be on the island; and, moreover, it might keep away sickness."

At the island of Eimeo, or Morea (about four leagues distant from the western coast of Tahiti), I had visited and successfully treated several patients among the natives. This was so gratifying to a chief of secondary rank, named Mare, that he thus addressed me in the beautiful figurative style of his native language:—"You, curer of diseases, stay on this land, that it may not be consumed by death, but that it may go with an upright head." I asked him what advantage I should derive by complying with his request? He replied, "You shall have what the land produces; we can give you no more, nor that which we have not."

A want of employment seemed in one instance to produce some imaginary illness among the natives of the island of Rótuma; this occurred when we had on board the ship *Sophia* (about the commencement of March 1830), nearly two hundred natives of that island, bound to the island of Erromanga (New Hebrides group). Having nothing wherewith to employ their minds, they began to be influenced by imaginary disorders. As they consider the spirits to be the cause of illness, in this instance the demon of ennuï had certainly entered into them; the applications to me, consequently, became very numerous; the treatment in coloured water and bread pills was eminently successful, and the encouragement of their native dances on board frequently during the day prevented any relapse. The chiefs were, however, pleased with my rapid cures; and thinking that I would be a great acquisition to them, could they succeed in persuading me to reside on their beautiful island, began by an address in which handsome offers were made of land, pigs, wives, and other commodities of

that kind; and moreover, as an inducement, to use the language of the chief, "I was to be all the same as one king." A "*Rótuma College of Medicine and Surgery*" would have been new in the annals of Polynesian history, and had I acceded to the request of the chiefs, how much benefit might have been conferred on them by such an establishment, all "*hole and corner systems*" being expunged, and the regulations being on a liberal scale!

When a New Zealander receives a gun-shot or other wound, it is thus treated: a dog is killed, and the blood being collected, is made hot by the aid of heated stones, and administered to the patient, who drinks it as hot as possible; the priest then prays over him, the wound is frequently washed, and all extraneous substances removed; but no other external application is used but water. The invocations of the priests to the spirits are repeated frequently during the cure. No married man or woman (excepting his own wife) is permitted to come near or see the patient during his illness, from a superstitious idea they entertain, that by so doing the spirits would be angry, and retard the cure. The excellent constitutions of the natives prevent that unfavourable condition of wounds which we are accustomed to see in Europeans; and they recover from most serious injuries with comparative ease, and in a very short period of time. This rapidity of the healing process renders them very favourable subjects for surgical operations*.

Fractures are treated without any difficulty; the bones are laid in apposition, and sticks, or pieces of bamboo, placed as splints to keep them so. The splints are seldom removed until re-union has taken place. The inflammatory stage among the abstemious natives is very trifling, and re-union takes place rapidly.

I visited a native during my stay at the river Thames, New Zealand, suffering under a severe rheumatic affection

* This has also been observed in India by H. H. Spry, Assistant Surgeon of the 3d Bengal Cavalry, on a case of luxation of the femur in a native, on which he says, "After the reduction of the luxation, little or no after treatment was rendered necessary, in consequence of the absence of that inflammatory excitement so common among Europeans, and in less than a month the man had the perfect use of his limb, and left the hospital. His age was about thirty-eight."

of the joints, which had become enormously swollen, rendering him incapable of any exertion, and causing excessive pain. The weather was cool and rainy, consequently unfavourable for his cure. When I visited him I found my patient stretched in his hut on a mat, and partially covered by a blanket: a fire was also kept burning in the hut. He preferred placing himself under the native doctor, or priest, in whose prayers he placed greater confidence than in my remedies. From the severity of the symptoms, and carelessness of the patient, I hardly expected that he would survive. In about the space of three weeks he came on board perfectly recovered; and, on inquiry, I ascertained that the only application made during his illness was oil rubbed frequently over the surface of the body; but the prayers of the attending Tohunga, or priest, to appease the anger of the spirits (whom he regarded as the cause of his suffering), was, in his opinion, the cause to which the cure was to be attributed.

The native remedies at New Zealand and Polynesia are chiefly from the vegetable kingdom, which are bruised and applied externally, or infused either in water or the juice of the coco-nut, and administered internally. Some of their remedies are mild, and others very powerful in their effects.

At the Sandwich islands they have the following account respecting the origin of the employment of herbs among them for the cure of diseases; it has been thus accurately related by Ellis, on the authority of the governor of the district of Kairua, at Hawaii:—"Many generations back, a man named Koreamoku obtained all their medicinal herbs from the gods, who also taught him the use of them; that after his death he was deified, and a wooden image of him placed in the large temple at Kairua, to which offerings of hogs, fish, and coco-nuts, were frequently presented. Oronopuha and Makanui-airomo, two friends and disciples of Koreamoku, continued to practise the art after the death of their master, and were also deified after death, particularly because they were frequently successful in driving away the evil spirits by which the people were afflicted and threatened with death. This is the account they have of the first use of herbs

medicinally; and to these deified men the prayers of the Kabuna (priest) are addressed, when medicine is administered to the sick."

I will now relate another instance of rapid cure in a native, from an injury which, in an European, might have been productive of very serious effects. A native of New Zealand came on board the ship when we were at the river Thames, who had met with the following accident. When walking in the forest, a projecting stick had penetrated into the eye to such a depth as to permit the escape of the humours; vision, of course, was completely destroyed. Having heard of the rapidity with which natives recover from these severe injuries, I gave him simply a lotion. After the first day I could procure no intelligence respecting this patient; but after a fortnight had elapsed he came on board in good health, and with the eye perfectly healed, and the lids closed. My curiosity was excited to ascertain what native treatment had been adopted to produce such an excellent and rapid cure. He informed me that he had placed himself under the care of the Tohunga, or priest, who had prayed over him, and, through his *influence with the spirits*, had caused the wound to heal; he further informed me that the applications used were the lotion I had given him, and after that had been expended, the wound had been repeatedly washed with cold water.

At New Zealand the women are attended during labour by their husbands; but if it is a difficult labour, they suppose the spirits to be angry, and therefore send for the Tohunga, or priest. On the arrival of the Tohunga, he strides over the woman, breathes on her, and afterwards, retiring to a short distance, sits down and prays to the spirits. If the labour terminates favourably, it is looked upon as resulting from the influence of the Tohunga in averting the anger of the spirits; but if the termination is fatal, the Tohunga is considered to have incurred the displeasure of the spirits, and to have lost his influence.

Several deaths take place among the New Zealand females (as well as at the island of Rótima, and others of the Polynesian archipelago) from Nui Toto (signifying much blood), or hemorrhage, as well as from difficult parturi-

tion. The position in which the women are placed for labour is on their hands and knees.

If the child is born alive, but the mother dies, it is placed under the charge of another woman, either to suckle or bring up by hand. The placenta is named *Fenua*, which word signifies land; it is applied by the natives to the placenta, from their supposing it the residence of the child. At New Zealand, on being discharged it is immediately buried with great care, as they have the superstitious idea that the priests, if offended, would procure it, and, by praying over it, might occasion the death of both mother and child, by "praying them to death," to use their own expression.

After the child is born they wash themselves in the water, and afterwards use a vapour bath, which is made by heating stones, and throwing water, and three kinds of shrubs in a green state upon them; (which are called by the natives *taramoa* (a species of *rubus* allied to *australis*) *mangeo* and *kótuku-tuku*); from these a steam arises, over which they sit; this is done to promote the lochial discharge, or, to use their own expression, "to make the blood come from them." They use this bath generally twice; once soon after the child is born, remaining exposed to the steam for about the space of an hour, and repeat it for the same length of time on the following day; they also drink at the same time warm water, in which a small fish, called *Mohi*, has been soaked*.

The wife of a New Zealand chief expressed much astonishment when she was informed that English ladies were not attended during labour by their husbands, but by persons who practised it as a profession: she replied that such was not the custom in New Zealand. At the island of Tahiti they are very desirous of procuring the assistance of Europeans, more particularly if it is a case of difficult parturition; but they have had the example of the wives of the missionaries before them; and they are always desirous of receiving assistance from those of the missionaries who have some knowledge of midwifery practice. I recollect shewing a New

Zealand chief woman a fœtus, of about two months, preserved in alcohol. She very innocently asked me whether it was a spirit; but I could not persuade her that it was an embryo human being.

The New Zealanders have recourse to applications of mud for some complaints, and perform blood-letting by making incisions with shells.

The ceremonies connected with the administration of medicines to a sick person at the island of Tahiti was formerly peculiar, and has been related by Ellis in the following manner. "When a person was taken ill, the priest or physician was sent for; as soon as he arrived, a young plantain tree, procured by some members of the family, was handed to him as an offering to the god; a present of cloth was also furnished as his own fee. He began by calling upon the name of his god, beseeching him to abate his anger towards the sufferer, to say what would propitiate him, or what applications would afford relief. Sometimes remedies were applied at the same time, or the relatives sent to fetch certain herbs or roots, but the priest usually went himself to compound the *raau*, or medicine. A considerable degree of mystery was attached to this proceeding, and the physicians appeared unwilling that others should know of what their preparations consisted. They pretended to be instructed by their god as to the herbs they should select, and the manner of combining them. Different *raaus*, or medicines, were used for different diseases; and although they kept the composition of their nostrums a secret, they were not unwilling that the report of their efficacy might spread, in order to their obtaining celebrity and extended practice. Hence, when a person was afflicted with any particular disease, and the inquiry made as to who should be sent for, it was not unusual to hear it said, '*O ta mea te raau maitai no ia mai*'—'Such a one has a good medicine for this disease.'"

At New Zealand and Polynesia, the priests are the physicians; it is their occupation to administer medicines, and pray over the sick person, as well as also to administer to his wants. At New Zealand a single man or woman is permitted to see the patient, but the married (excepting the wife of the patient) are entirely excluded.

The priest at New Zealand is named *Tanaka Tohunga*, or man that attends

* These fish were described as being of a white colour, and usually caught at the mouths of rivers; they are four inches in length, and of proportionate breadth. I have only seen them in a dried state.

on the sick. At the Sandwich islands those who attend on the sick are named Kahuna, rapau mai signifying Kahuna, a priest; rapau, to heal or administer medicine; and mai, disease. At New Zealand they called the European doctors on board the ships Rata, which seems merely to be a corruption of our term doctor.

Among other ceremonies over a sick person at New Zealand, the Tohunga, or priest, during his incantations, waves the frond of a fern called uru-uru-fenua (*asplenium lucidum*) over his patient; if it breaks during the ceremony, it is a sign that the patient will not recover.

The Tahitians use the oil extracted from the nuts of the Tiari, or candle-nut tree, (*aleurites triloba*) as an embrocation for rheumatism. Captain Henry, of Tahiti, informed me that he had been perfectly and speedily cured of a severe rheumatism, which affected his joints, by an embrocation of this oil.

Burning is a remedy used at several of the Polynesian islands for disease. A chief at Rótuma (Marof) once observed to a person who touched in joke his bare skin with the lighted end of a cigar, "that very good when I am sick, but now I am no more sick, it is no good."

Steaming is used by the females at New Zealand, who, from their intercourse with Europeans, have become syphilitic. Gonorrhœa is soon cured by that remedy, and their abstemious habits.

[To be continued.]

CHOLERA AT ALTONA.

To the Editor of the London Medical Gazette.

SIR,

THE following letter, which I shall alter only to make the idiomatical expressions nearer the English, is one for which a regular fee has been given by the friend, who has handed it to me; and, as I think the labourer has proved himself worthy his hire, I have the more satisfaction in offering you the results of the practical observations and the treatment, in the cholera morbus, of an eminent practitioner at Altona.

Your insertion of the following, in your next number, will much oblige

Yours, &c.

JOHN S. GASKOIN.

Among the different modes of treatment of contagious cholera, none can be said to be, in every way, the best; at one time an antiphlogistic, at another an antispasmodic, or an antispasmodic, line of treatment, will be found fully indicated: in short, the general therapeutic rules, in the treatment of cholera, also keep their full value; and those medical practitioners will be the most fortunate ("happiest") in curing, who understand the best to individualize.

Most of those seized here, had either caught cold or had made some blunders respecting diet; and they were treated, of course, conformably to these circumstances.

The first practice is, to put the patient into a warm bed, to put to his feet hot stone bottles, and to lay to his sides gurgions (a coarse bran), or some similar stuff. Dry warmth, in general, did more good than humid; warm-baths being rarely found useful. Frictions of the limbs were best made with warm oil, or Ol. Camphorat. alone, or mixed with the Laudanum Liquid. Sydenh. Frictions with spirituous fluids were seldom found useful. Cramps were best removed by those oily frictions, especially with the naked hand.

Internally, in the beginning of the disease, an emetic of the Rad. Ipecacuanh.—particularly where the anxiety was very great, and no inflammatory state to be recognized, which was seldom the case here, even when the evacuations per os and per anum were very copious—an emetic of Rad. Ipecacuanh. was always found useful, and was sometimes able to cut short the disease. The emetic was followed by infusion of chamomile, and it seemed to be advantageous, principally by its revulsive and diaphoretic powers. Commonly, after a full action of the emetic, the evacuations became bilious, the tongue furred, a general perspiration began, and the pulse became sometimes so full and hard that a moderate venesection was advisable. If sickness remained after copious bilious evacuations, the Potio River.* and similar remedies, were administered with great success; likewise small doses of calomel were useful, the intestines being now often very torpid. Externally, the application of great sinapisms on

the abdomen, after the use of emetics, is much to be recommended. In the last stage, or paralytic form, which sometimes follows very soon, all remedies, even the boldest, were rarely of any use, seldom producing any effect on the constitution; for instance, Tinct. Capsic. Am. ʒss.—j. pr. dos.*; Ol. Cajeput. gtt. xi. pr. dos.; an emetic of Zinc Sulphatis. gr. xx. had no effect. In this stage, external applications are especially to be depended on; for if the sanative powers of nature do not bring help here, the medical man will hardly do it.

Above all, we are to be most carefully warned against applying too exciting remedies, which are not merely useless, but retard the recovery of a patient who may be really restored from the cholera, and may kill the patient by the existing succeeding affections; a too exciting treatment of cholera commonly causing a typhoid fever, inflammation of the brain, &c.

Congestion of the vessels of the brain, which soon follows the cholera attack, requires, of course, local bleedings, blisters, cold fomentations, &c.; in short, every morbid state must be treated according to general and individual indications, as in other diseases.

The patient may drink tea of Flor. Cham. or herb Ment. Piper., milk, or water, with Elixir acid. Haller.† and similar fluids, warm or cold, just as may be chosen; but care should be taken that the patient drink but a small quantity at a time.

A maintenance of quietude is highly to be recommended; by rising too soon from bed relapse of the disease often occurred.

Often, good effect was produced from small doses of camphor, with acet. vin. where the skin was very dry, the cramps vehement, the pulse sunken, and no gastric complication. The following formula was frequently employed:—

R Camphor trit. f. ʒss.—j.

Acet. Vin. ʒij.

Aq. Ment. Piper. ʒiv.

M. A large table-spoonful for a dose.

Venesection in the beginning of

* I consider this to mean, from half an ounce to an ounce.

† Eau de Rabel.

Acidi Sulphurici puri.

Spt. Vin. Rect. graduum. Triginta pondera paria, &c. Vide *Phar. Batava*.

Acidi Sulphurici (66 degrees) une partie.

Alcohol (36 degrees) trois parties.

Mélez dans un matras et conservez. *Jourdan's Ph. Universale*.

the disease was sometimes useful; but seldom in the more advanced periods blood could be made to flow.

Great doses of opium were always found to be pernicious.

In treating the diseases that commonly follow the cholera morbus, we must bear in mind that the intestines have been most vehemently attacked by a morbid affection, and that they require repose to recover.

No better preservative against cholera can be recommended, than the avoidance of blunders in diet, and catching cold.

W. HEYCK, Dr. Med.

Altona, Nov. 18, 1831.

ATTENDANCE OUGHT NOT TO BE GRATUITOUS*.

To the Editor of the London Medical Gazette.

Heu, mihi! me malus abstulit error—amor pecunie.

SIR,

THE daily occupation of the medical man is at once the work of public humanity and of personal profit. His task and duty is to do good, to stand by the sick, to cheer the conscious sufferer from vicious indulgence, and to administer solace to the mind, and ease to the body. In the day of battle, the medical man endeavours to save the life which the soldier destroys; and when a national pestilence is abroad, the medical man is chiefly exposed to the pest while watching and learning its nature and treatment. He always performs the work of charity, because he gains his daily bread by being charitable.

A false notion is now afloat concerning the humanity of medical men. While a disease is threatening to infest our capital, public authorities are wisely convened to forestal and prevent its ravages among the dark and dirty dwellings of the poor; and the poorer people are cleansed and cheered according to the active and diligent instructions of their alarmed superiors. A general feeling of humanity is produced and enforced by private apprehensions. Sobriety, one of the first of christian

* We regret that press of other matter led to the postponement of this very sensible letter: in the sentiments which it contains we entirely concur.—ED. GAZ.

virtues, is now proclaimed, not by the voice of wisdom, but by the shout of calamity. In the discharge of this novel duty, the medical man is called upon to take his part; but his part is to be discharged, not in anticipating the arrival of the disease, but in meeting it when it is arrived. He is to hold himself in readiness to rise by night and by day, to enter the houses of the poor, to detect, to touch, to handle, and to treat, a loathsome sickness, and to lean over the bed, or to tarry by its side, till safety or death shall have ensued. If there be any contagion or infection, he is exposed to the baneful influence; if there be any hazard of health, he is open to the obvious danger. We doubt not the moral energy and the professional avidity of any medical practitioner in the encountering of a new disease; the eagerness with which the philosophic physician would hasten, any hour, to survey, perchance to understand and to cure, a spreading evil fatal to the lives of men; but no man is justified in wantonly exposing his person to mischief; and every master of a family is bound to consider those who depend upon him for support. The danger may be adventured upon, but only with the prospect of a fair remuneration; and those authorities which require the medical man to serve for nothing, dictate an act of humanity to be practised only according to the feelings and the means of the individual dictated to. A medical man's time and judgment are purchaseable articles; and they are, like bread and wine, to be purchased in all seasons, both of prosperity and of national adversity; since humanity is exhibited, not by acting for nothing, but in doing to the utmost what is right and proper, in the hope of a legitimate reward. They who do less than this, are inimical to themselves and to the common weal; for how shall society be held together if mutual advantages be not considered? The medical man must support himself by his labour, and he will soon cease to be able to act gratuitously if his labour do not supply him with the means to live.

But suppose there be no danger of infection, and that the disease were curable by a touch, nevertheless that touch, and that exertion, without hazard, is still worthy of a just reward. So that, if the poor are to be attended gratuitously, let the medical man act for himself, as he frequently does act, without ostenta-

tion, by giving, if he choose, his advice and his medicine as a free gift, merely with the hope of doing good.

These observations are presented to your notice, because some parishes have called upon medical men, and some medical men have voluntarily offered themselves, to act upon a principle of bald humanity—a humanity which is to be exercised according to the authority of a vestry. And it appears to me (perhaps I am sordid) that in exact proportion as we are called upon to do more, so are we worthy of a higher pay; and that when our rest and our health are to be exposed and broken, we are not justified in promising our services without the certainty of an appropriate remuneration. Money we desire for ourselves indeed, but more for those who live by our exertions. Money is the source of subsistence. The days are gone when we might pluck, and live with ease and pleasure, from the tree of life; and in England a man will be arrested for nudity, if he have not money to clothe himself withal. The profit to be expected from an extension of name and reputation, is remote and vacuous; and that policy is truly fallacious which places the well-being of an individual or a nation not in immediate, but in a prospective good; since who can foresee or control the adverse rise of intermediate circumstances?

Having advanced the principle upon which I, as a man, expect the just remuneration of my services, I am not ashamed to say with Horace—“*querenda pecunia primum est.*” I would not let my ear be deaf, nor my eye blind to the sight and the sound of genuine poverty; nor would I spare my best exertions to alleviate the afflictions of the unfortunate, the wretched, and the debased; the knot of my purse can be loosed to give as well as to receive; and I should blush indeed if my hand were not sometimes open to bestow as well as to accept. I have learned, from the practice of my profession, the pleasure of doing good; and I only demand for my exertions, especially public exertions, those pecuniary supplies which may still enable me to practise and to study medicine, to support my family, to benefit my friends, and to give to all those who in the hour of need I know will be relieved and comforted by the silent gift of benevolence.

MEDICULUS.

Nov. 25, 1831.

EXTRACTS FROM REPORTS ON THE
CHOLERA AT DANZIG,

BY JOHN HAMETT, M.D.

Descriptions of the Symptoms of the three Principal Forms of the Epidemic at Danzig; being Extracts from the circumstantial Detail of the Symptoms of it, including its subordinate Features.

PASSING over subordinate features of the epidemic, I shall limit my descriptions here to the three principal forms of it, viz.

1. The rapid and severe cases of fatal cholera.

2. The protracted cases of fatal cholera; and

3. Those less severe, which proved favourable.

1. In most rapid and severe cases of fatal cholera, the patient was suddenly seized with sickness or pain at stomach, occasional pain, or feeling of weight and uneasiness in the hypochondria, the right hypochondrium especially, giddiness, prostration, great thirst and craving for cold drinks, a cold sweat that quickly became colliquative and clammy; at times coldness alone, at others coldness and dampishness of the body—but never with shivering; the pulse was frequent but not hard, and soon became exceedingly reduced; the hands and features somewhat shrunk; the tongue was foul, unnaturally moist, and occasionally tremulous; the voice subdued; the eyes heavy and suffused, and the sight dim. These primary symptoms were in general either accompanied, or immediately followed, by retching and vomiting, and a peculiar watery diarrhoea, that often, however, proved irregular in the order of attack, occasionally even with respect to each other, and oftentimes severe, in hot, close, and electrical weather especially; gripping pains in the abdomen; painful contraction of the muscles at the umbilicus; suppression of the secretion of urine, and occasional pain in the region of the bladder. Cramps in general followed the retching and vomiting, and in most instances invaded the calves of the legs at first; in their attacks of other parts of the extremities they proved irregular, seizing first the fore-arms, calves and fore-arms, hands and fingers,

toes and feet, or hands, feet, and calves, in different instances, indiscriminately; occasionally they mounted up the thighs, but seldom attacked the trunk. Men rarely escaped them, women frequently, and children generally.

The vomited matter in general consisted of undigested food at first, sometimes partially tinged with yellowish green; of fluid ingesta, also occasionally imbued with greenish-coloured matter, and partly of slime and mucus. Often, however, it consisted of undigested food, or of fluid ingesta alone, without being in any wise so imbued. In the retching and vomiting which followed, the fluids taken continued to be rejected with a little greenish-coloured matter, with or without more slime or mucus. The dejections were always watery; sometimes as if coloured with feculent matter; in general they were either colourless, somewhat like whey, or had the appearance of rice-water, barley-water, occasionally somewhat dirty, or an evanescent sediment, after being shaken in water.

After this first advance of the disease, the following symptoms rapidly supervened; viz. increasing oppression at the heart, and short hurried and laborious breathing, ending in complete oppression and weight at the *præcordia*; tossing of the head about; anxious restlessness depicted, often with terror, in the countenance, which in general was of a dark brown, wan, or leaden hue, according to the complexion; insatiable thirst, with incessant craving for cold drinks, and the voice raucous and depressed. The retching and vomiting, and diarrhoea, with occasional *tormina* and cramps, at first only intermitting at short intervals, subsided either abruptly, or gradually as vital exhaustion advanced; the pulse at the wrist, if not extinct—which it was in most rapid and severe instances—was accelerated to the utmost in frequency, and barely felt; the surface of the body quite cold, damp, and clammy, and the feet and insteps marked with bluish streaks and patches; the tongue cool or cold, and in some instances livid at the tip and edges; breath cool or cold; lips blue; nose sometimes bluish; voice below the breath, or gone; cheeks and eyes now quite sunk; pupils at times partly or completely dilated: eyelids half-closed, and encircled with livid rings; the parts of the conjunctivæ exposed being much

the same in appearance as after death. Amid this complicated suffering, the patient was not insensible until just before dissolution, which ensued after some faint convulsive sobs, generally within from eighteen to seventy-two, and occasionally within from eight to eighteen hours after the first attack.

2. In the protracted cases of fatal cholera, which have been few in number compared to the rapid cases, the following febrile symptoms have been observed, more or less, in different patients after the indefinite period of the first stage; namely, marked congestion, with pain in the head, deafness, humming noise in the ears, heavy stupor, continual drowsiness, partial ravings; a dark-flushed, brownish-yellow, squalid or cadaverous countenance; a dark-brownish clammy, or furred tongue; dark sordes about the teeth and lips; eyes heavy and suffused, or dry and parched, often with eventual dilatation of one or both pupils; a hot or cold clammy skin; pulse frequent, with febrile action, or very small; with pain or soreness of the abdomen, increased on pressure—and occasional tenesmus. With these symptoms, the excretions, as may be readily conceived, were scanty and vitiated. The stools dark, dark-green—very fœtid; and the urine in general dark coloured. Delirium generally took place in these before death, and they died within from three or four to five or seven days, or later, after the first attack; more generally on the fifth.

These modifications of particular symptoms, bordering on each other, and referring to individual parts, depend, I need scarcely add, not only on differences in constitution, but, in a certain degree, on the mode of treatment at the commencement, and even on the state of the locality in which the patients happened to be placed.

3. In the cases less severe,—and as I have observed,—of less unhealthy persons in whom the natural powers* of the constitution were calculated to withstand the effects of the shock on the system, giddiness, retching and vomiting, watery diarrhœa, occasional grip-

ing pains in the abdomen, cramps, occasional painful contraction of the muscles at the umbilicus, thirst, and suppression of urine, took place, and proved occasionally severe; but the congestion in the head, and oppression in the chest, were certainly less marked; the pulse, although barely felt, was rarely entirely suppressed; coldness of the body, the cold clammy sweat, and other bad symptoms, were not marked in any great degree. The leading symptoms gradually, or abruptly, disappeared; and more or less of febrile re-action ensued, generally within from eighteen to twenty-four or thirty-six hours, or more, after the commencement of the disease; about which time hiccups, always a favourable sign, were occasionally noticed, and not before. The exact period of the return of the urine was not certain, being sometimes before, at others after, the first appearance of re-action: it was dark or high coloured, voided in small quantities, with occasional difficulty, and frequently attended with some pain in the region of the bladder. The return of urine, though an important symptom, was not always decisive of a favourable result; on the contrary, hiccup, which however was not always observed, almost invariably indicated recovery. The dejections immediately after the commencement of reaction were fluid, scanty, and dark coloured, as if imbued with blackish, feculent matter; but they very soon became successively brownish, and naturally bilious and feculent. Indeed, in the majority of cases of this description, the secretions and excretions soon got into play, and restoration was more or less rapid.

Partial stupor, with little or no delirium, more commonly occurred in children, and spare aged persons free from previous organic or general complaint, and gave grounds for a favourable prognosis: they seemed tranquil, and as if naturally asleep. They were in general affected with œdema in the feet, and more or less in the legs after convalescence had commenced. Œdema also occurred in others after the disease, but not generally. In pregnancy abortion invariably took place, and was always a critical symptom, death or a favourable change soon following.

From the description above given of the rapid and severe cases of fatal cholera in Danzig, its similarity to the In-

* This is certainly true; and yet a woman, named Eliza Brandt, thirty-six years of age, affected with tubercles and vomice, had, in July last, this less severe form of cholera, which soon gave way to all the hectic symptoms of her complaint.

dian cholera appears manifest; and from the descriptions of fevers supervening after the first stage, as given in the second and third forms, its deviation from the Indian epidemic, in which those fevers do not generally supervene, also appears evident. The greater severity in general, which has been found of the vomiting, diarrhoea, griping pains in the bowels, and painful contraction of the muscles at the umbilicus, in the epidemic in India, compared to that in Danzig, is easily explained by the well-known influence of the climate in India on the whole system, and digestive canal in particular.

Extracts from the Pathological Report on fatal Cholera, both rapid and protracted, founded on the Examinations of twenty-one Subjects; the youngest of which was four, and the oldest ninety years of age, the rest having been of adult and middle ages.

Many of the characteristic appearances after death will depend in a great measure upon the number of hours elapsed before the body is opened; the later the examination, the less truly characteristic, so far, are the appearances. Those I have examined were in general opened within, or about, twelve hours after death. Bodies at this season ought, however, to be examined as soon as possible, and always within at least six hours, if it can be done with propriety.

Of all the morbid effects in appearance, which I have observed after death in the bodies of persons who died of cholera in Danzig, the most characteristic, perhaps, has been the great congestion of blood in the sinus venosus and right auricle of the heart, and in the veins throughout the whole body; the next is the invariable contraction of the bladder; and another, which, although not apparently constant after death from this disease, is seldom or never to be met with after death from others—namely, slight spasmodic contractions, or movements, if they may be so called, in the muscular fibres here and there in the body, and more especially in the face and extremities, not only immediately, but some time after dissolution. These resemble galvanic effects produced in the body after death.

The veins, and right auricle in particular of the heart, were full of black blood; some was always found in the

left auricle; while very soft imperfectly coagulated lumps were found either in the right ventricle or within the aorta, either immediately at its commencement, or down below its curvature. These lumps were invariably as black as the blood found in the veins and right auricle; the thoracic aorta uniformly contained some black blood, but was never full, like the veins; the abdominal aorta also contained a little, but very little; the right ventricle had always a small quantity of black blood, the left ventricle a very little. The pericardium seemed more or less flaccid, and very often contained a quantity of dark-brown serous fluid. The parietes of the heart in general seemed soft, and I fancied, in a few instances, that those of the left auricle seemed thickened; this, however, remains to be confirmed or refuted by subsequent examinations. I occasionally observed morbid blackish, or bluish, and, in one instance, whitish spots on the external surface of the heart. The lungs were in general much more bluishly speckled than in most other cases—almost always collapsed, but dense from black blood—not as in hepatization of the lungs—frothy, black blood freely oozing from incisions made into them. The pleura, in its reflections throughout from the anterior to the posterior mediastinum, and over the upper surface of the diaphragm, seemed in general of a dark dull red. The trachea, bronchia, and larynx, contained a little frothy mucus, and were otherwise wet with a compound of serous and clammy fluid; but the internal mucous surface exhibited no vascular appearance. In general there was a considerable quantity of clammy, serous, fluid found effused in the chest; all was wet, exceedingly soft and clammy, more so than I have been used to see after death from other diseases. The vena azygos was invariably full of black blood. The thoracic duct was in general empty, and seemed natural.

On detaching the calvaria from the dura mater, the latter was, in most instances, spotted all over with the black blood that instantly issued from the torn vessels, especially along the lines of the sutures, where they are most numerous, in the younger subjects particularly. The external surface was mostly of a dark bluish colour, and dry, but clammy feel. The internal

surface of the dura mater, and its processes, or continuations, were not marked by any peculiarity, except, perhaps, in the appearances being more opaque, and feeling more clammy than usual. The tunica arachnoidea was in general of a wheyey, glossy colour, and somewhat clammy to the touch. Between this membrane and the pia mater, and more especially in the lower part of the cerebellum, there was occasional effusion or filtration of serous fluid; and in all instances there was considerable effusion of this fluid between the pia mater and the cerebrum and cerebellum both; in most instances it was found in the ventricles, in the fossulæ at the basis of the cranium; and, indeed, wherever this effusion between the tunica arachnoidea and pia mater in parts of the cerebellum, and the pia mater and the brain itself at large, was observed, it was also invariably observed in the same relative situations in the spinal marrow of those bodies in which the spine was examined—which were fifteen in number. In other instances, too, where there was effusion in the brain, we had only to elevate the pelvis and loins in order to see serous fluid issue forth from the spine through the occipital foramen. There was always a considerable quantity of thin black blood in the sinuses, in the inferior more so particularly. In all cases, the congestion of black blood in the veins of the pia mater was great, in the venæ Galeni, and choroid plexuses, accompanied with varicose dilatation of these vessels; and likewise the same relative congestion of black blood in the veins of the pia mater in the spine, especially in the posterior parts of it, where these vessels, being larger and more numerous, varicose dilatation was more conspicuous. The medullary substance of the brain seemed in some instances much softer than usual, but it might have been owing, in part, to the interval elapsed during hot weather between death and the time of examination. In some instances black spots were visible on incisions into the brain; at times, too, the cineritious and medullary substance both seemed relatively altered in appearance as well as consistence. The state of the spinal marrow corresponded in all cases exactly with that of the brain.

After what has been said and implied of the venous congestion in the brain, spinal marrow, and thorax, it will be

readily conceived in the abdomen, in which the large as well as small vessels are still more numerous and varied. The vena cava abdominalis and vena portæ, with the splenic and superior mesenteric trunks, and, in short, all their large tributary branches, invariably contained a considerable quantity of black blood; they seemed at times as if full of it, while the mesenteric veins always exhibited a characteristic black or bluish arborescent appearance throughout. The gall bladder was not only of a deep green externally, but, in some instances, from a deep green to a bottle-green, and occasionally tinged here and there with yellow; and was in general distended, and full, or nearly full of fluid, generally black, and sometimes as if a little of yellow or brownish yellow bile had been mixed up in it. The internal or villous coat of the gall bladder was in general between a dirty yellow-brown and brownish yellow—in a few instances it was a natural bilious yellow. The liver was invariably in a state of *engorgement* from the black blood, which, in all states of it, freely oozed out from the hepatic veins in particular, on incisions into its substance: it was in general discoloured, even after sponging the membrane covering it, and I think most in the younger subjects, and those who had not suffered from previous affection of it. The spleen was also in a state of *engorgement*, and of a black purple colour—and this independently of any alteration in its structure as referrible to other morbid states. The kidneys, notwithstanding the suppressed secretion of urine, did not exhibit any peculiar change in general, further than that of venous congestion. The same was observed in the pancreas. It is not easy to say whether the ductus communis choledochus, and immediate biliary vessels, were in general contracted or not; sometimes I found greenish or vitiated bile at the opening of it into the duodenum, and sometimes I did not. I often found, in protracted cases particularly, the external parts of the duodenum and colon in contact with the gall-bladder, or near it, completely discoloured with yellow bile. With respect to the stomach and intestines generally, I cannot say that I observed any effects of the disease beyond what is referrible to congestion of blood in the veins, and what might be attributed to the sedative nature of the dis-

ease. The mucous coat of the stomach, in particular, and parts of the colon, seemed, in some instances, soft, as if half macerated; indeed, the intestines generally seemed soft, and as if the internal mucous and villous coat could be separated from the muscular coat. The small intestines, I mean the jejunum and ileum chiefly, were more commonly of a dark dull red, or rather of a dark dull slate colour, on their external peritoneal coat, without any positive vascular appearance; sometimes of a pale slate colour, with vascular injection, or vascular congestion more marked; while, on the internal surface, they did not exhibit the same colour generally—still, in some instances, there was in some parts a modified appearance of it; while in various parts in others there was a manifest vascular appearance of the internal mucous and villous coat, though by no means corresponding to that externally. Besides the pale slate or leaden colour, and the dark red slate colour, I have observed a vascular dark red also—facts which will account for that tenderness, or pain, on pressure of the abdomen, so marked in cholera, especially in protracted fatal cases. In one instance of a young woman, who had died of true and very rapid cholera, the general external appearance of the whole of the small intestines was of a pale or light rosaceous hue, while that of the colon was quite pale. The mucous membrane throughout the whole canal was whitish, and as if half macerated. Whether the brown patches, which are at times observed here and there on the internal surface of the stomach and intestines, are effects of the disease, or of previous chronic inflammation, is in some instances not easy to determine. The stomach and intestines, as might be expected, mechanically retained the last fluid ingesta; for, latterly, what came away, did so involuntarily. There were the remains of former mucus, more or less, throughout the whole digestive canal; and in true, rapid, and fatal cholera, little or no remains of feculent matter, except in its usual receptacles, namely, the commencement of the colon, the cæcum caput, in the transverse arch occasionally over across it, and in the sigmoid flexure, in which, in some instances, scanty portions of it were found. The mucous follicles in the internal membrane of the colon, at its commencement, and Peyer's glands in the end of

the ileum, were occasionally found in large compact patches, more or less continuous, distinct, elevated, and somewhat indurated. Brunner's glands, as they are called, were not so observed in the duodenum. The colon externally, as well as the duodenum, particularly at its upper curvature, was discoloured at the upper part of the ascending portion, and beyond in the greater part of the transverse arch; but in the other parts it was of a pale, or pale lead colour. The peritoneum, in all its detached reflections, was more or less opaque, having lost its shining, glossy colour, more so than in most other congestive and sedative diseases of the system attended with fever, more even than in the compounds of remittent and intermittent fevers in tropical climates, with marsh miasmata, in which venous congestion is so very notorious. In protracted fatal cases I occasionally observed chronic discolouration here and there on the internal surface of the stomach and intestines—in some instances of a dark brown, in others of a dark brown red, without being exactly vascular in appearance: at times vascular spots and patches were observed in some parts of the intestines, and the dark brown, and dark brown red in others; they were generally in the colon, the commencement above and below particularly, in the transverse arch, and sigmoid flexure. I observed parts of the colon in a gangrenous state, and chronic inflammation of the whole of the ileum, in one subject. In several instances the lumbricoid ascarides were found in the intestines. In some instances the commencement of the thoracic duct, or receptaculum chyli, seemed quite close and contracted. The invariable close contraction of the bladder, I have not omitted to mention; it was mostly lined with a little whitish mucus.

JOHN HAMETT, M.D.

ANALYSES & NOTICES OF BOOKS.

L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

On Pestilential Cholera; its Nature, Prevention, and Curative Treatment.
By JAMES COPLAND, M.D. &c. &c.

THIS, the most recent volume on cholera, comes before us with no ordinary claims on our attention. We recognise in it, though in an expanded form, a very able and elaborate article in the Foreign Quarterly Review—an article

in which the infectious nature of the disease in question was supported by a mass of evidence, which scarcely leaves any thing to desire. The information compressed into the volume is of the most ample description, though it is of very small size, and moderate price. The disease is divided into three grades:—1st, With marked premonitory symptoms, particularly diarrhoea. 2d, Commencing with giddiness, faintness, &c. the characteristic marks of the disease rapidly following. 3d, Seizure, sudden and intense. The treatment is divided in a corresponding manner—the various measures, as far as possible, to be used contemporaneously:—1st grade, Full vomiting, blood-letting, dry warmth, frictions, diffusible stimuli, calomel and opium; if the stomach remain very irritable, flannels wrung out of hot water, and soaked in oil of turpentine, to be applied to the epigastrium; subsequent reaction to be moderated, and indications attended to on general principles as they arise. 2d grade, Treatment similar to first stage of milder grade, the external and internal stimulants being more vigorously and assiduously applied; the reaction, if it supervene, to be treated on general principles. 3d grade, Stimulating emetics, and the various other powerful means recommended by the author in his paper published in this journal for Nov. 19th. Numerous formulæ are given.

Our object is to direct attention to Dr. Copland's little volume, not to make any pretence of giving a digest of it, for this could not be done without re-printing it. We scarcely think that we have read, or heard, of any thing on cholera, which is not referred to by Dr. Copland; indeed, his list of works consulted seems to include nearly all that have been written upon the subject.

—
An Address to the British Public on the late horrible System of Burking; containing on account of the Methods hitherto adopted for Supplying the Anatomical Schools with Subjects; and Suggestions for Remedying the Evil, &c. &c. By A PRACTICAL ANATOMIST.

WE wish this little pamphlet an extensive circulation; it performs what it professes in its title-page; and the perusal of it cannot but be productive of good, if it only fall into the hands of those for whom it is intended. There

are, however, some technicalities in it, which must have escaped the author through haste and inadvertence; these we hope to see rectified in a second edition. We are much pleased with the striking light in which some of the facts are here set forth; if they be not new, they have at least all the air of novelty given them by this dexterous management. The objections which we could offer to some of the author's suggestions we think it the less necessary to dwell upon, as our opinions on the anatomy question have been so amply and frequently recorded in the pages of this journal; but we are bound to add, that, on the whole, our "Anatomist" seems to entertain tolerably correct views.

—
On Indigestion and Costiveness. By EDWARD JUKES, Surgeon. Second Edition.

MR. JUKES had the merit of inventing—or if not, at least of recalling to the attention of the profession—the pump, or syringe, by which the stomach may be emptied of its contents. This gave rise to numerous others, by Read, Weiss, &c. probably better than the original; but we are desirous of giving the merit of priority to Mr. Jukes, the rather as it is the only point on which we can speak of his performances with commendation. His book is a very poor affair, with one or two coarse, staring, villainously-coloured plates, and much medico-popular discourse, intended obviously for unprofessional persons. It is one of a class with which the press is at the present moment overloaded.

—
A Demonstration of the Nerves of the Human Body; consisting of Four Parts. By JOSEPH SWAN. Part II. Price Two Guineas and a Half.

THIS, which is the second part of Mr. Swan's splendid work, contains five finished plates, with corresponding outlines, representing the lumbar and several portions of the sympathetic and splanchnic nerves. We can only speak of the general display of minute and most laborious dissection which they afford; of their accuracy we can say little, as in this respect we are fain to rely—and we do so with perfect confidence on the fidelity of the author. As works of art, they are, perhaps, superior to the first part, which looked rather flat in some of the figures;

these *rise* better. No anatomical teacher ought to be without them.

The London Manual of Medical Chemistry, &c. &c. By WM. MAUGHAM, Surgeon.

THE objects of this little volume are abundantly explained in the title page. It is intended for students, and by them will be found useful. The plan is much more comprehensive than that of many of the others having a similar object in view, and, judging from some passages which we have looked over, it seems to be well executed.

Practical Examinations on the immediate Treatment of the principal Emergencies that occur in Surgery and Midwifery. Part I. By W. S. OKE, M.D., &c.

DR. OKE gives rather a grandiloquent account of his motive for becoming an author. "It was said by one of the ablest commanders of the British navy, whilst expiring in the arms of victory, from a mortal wound which he had received in his country's cause, that 'England expects every man to do his duty.' This is undoubtedly true, &c." But the reader could scarcely come at the inference without assistance; we must therefore inform him that the above sentiment was the exciting cause of the work to which we now call his attention. The work, however, is much better than this introduction led us to expect; it really contains much useful information, very well arranged, and shews considerable research. We doubt, however, if it will prove popular, on account of its form. Question and answer, to prove attractive in conveying the elements of a science, require to be managed with very great dexterity.

MEDICAL GAZETTE.

Saturday, December 24, 1831.

"Licet omnibus, licet etiam mihi, dignitatem *Ar-tis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendipericulum non recuso."—CICERO.

THE NEW ANATOMY BILL.

FROM all we have been able to learn respecting the provisions of the New Bill*, it seems to be as prudential

* These remarks, it will be perceived, were written before we had *actually seen* the bill; we have allowed them to stand, however, as the assumptions they go upon have not proved to be incorrect.

and guarded a piece of legislation as can well be imagined, embracing just as few topics as possible; and in its tone seeming rather to deprecate than to court discussion. It is laconic and conciliatory—more simple (as it is said) in its form than the last bill, and professing to interfere with nobody's prejudices. This would certainly be all very well, were we sure that the brevity of it was pregnant with wisdom, and that its concession to *feeling* involved in it no sacrifice. But when we find that the only two (it is to be presumed the principal) clauses as yet given to the public, are—the one of them positive, and laying a heavy injunction on the teachers of anatomy; and the other permissive and precarious, allowing bodies to be bequeathed, or given up, by the nearest relative, and pointing out no other source of supply, we must be permitted to entertain some doubts as to the probable efficacy of this long-expected measure of relief.

The objections to the old bill were many and serious; those to the new one cannot possibly be so many, but they may be as difficult to be dealt with;—the mode of obtaining a supply of subjects under each, for example. According to the provisions of Mr. Warburton's former bill, it was to be lawful for the governors of prisons and hospitals to deliver up for dissection the unclaimed bodies of prisoners and patients dying within their jurisdiction; there was no sanction, however, to oblige the said governors to deliver up those bodies; it was perfectly optional with them to supply the schools or not, and on what terms it might seem best to them. Here was a most precarious prospect for the practical anatomist: bodies not to be given up for dissection if they were claimed by a relative (how many times removed the bill did not say); and after all, the disposal of them altogether to depend upon the caprice of those who

had the bodies in possession. It would have been curious to observe how this system would have worked, had the bill passed into a law; the probability is, that it would have operated in favouritism, or in a niggardly doling out of subjects in the quarters where they were most wanted; in Scotland the practice of anatomy would have been completely suppressed; and the schools of Dublin and Paris would have been overstocked with pupils. But what is the mode of supply contemplated by the New Bill?—Why, it actually (will it be believed?) throws open an additional source whence subjects may be obtained: with the utmost liberality, as well as regard for the delicate feelings of the public, it shews itself to be no “respector of persons;” there is not to be one law for the poor and another for the rich; the anatomist may claim one class as well as the other; and may have both equally, or both together—that is, if he can get either. Under the provisions of the new anatomy bill, as explained by Mr. Warburton when applying for leave to bring it into parliament, “nobody is to be dissected unless he has given his consent to that effect when alive, or be given up by his nearest relative after death.” See what an exalted view of the rapid improvement of human society this shews that Mr. Warburton takes: in his former design, he thought it advisable to allow the supply of the schools of anatomy to depend on the good sense of jailers and heads of hospitals: in his present one, he leaves it to the enlightenment of the individual members of society, to evince a proper regard for the interests of science: he puts his trust in the march of intellect, has no misgivings about the fate of one of the most useful of studies, and is careless about any little abhorrence that might be entertained of the details of its processes. But seriously, can Mr. Warburton have formed any estimate of

the numbers who will bequeath themselves to “the surgeons”—especially if that part of the law be not repealed, by which dissection is proclaimed to be a punishment and a disgrace?

The former bill said nothing about repealing that item of the penal law which makes it a portion of the murderer’s sentence that his body be “dissected and anatomised;” the present one, we understand, does. The truth is, that whoever will legislate for the regulation of the schools of anatomy in this country, must take his choice of one or other of two courses: he must repeal the dissecting part of the murderer’s sentence, and then bargain, as best he may, for the bodies of all the unclaimed and unknown who die in hospitals, workhouses, and jails,—or, allowing that absurd and mischievous item to remain in the statute-book, he must insist on having, for the purposes of anatomy, not only the bodies of executed murderers, but of all who are executed, no matter for what crime; also of all who die in jails, prisons, and penitentiaries; in short, the bodies of all those must be available who die within the jurisdiction of the criminal law. There is no middle course. Dissection must continue to be looked upon as a mode of punishment, and of affixing the brand of infamy on the memory of the deceased; or it must be clearly shewn to be what it is, a perfectly harmless, but most useful process—if not the most unobjectionable mode of disposing of the body, certainly the most useful and honourable way of applying the remains of the human being. Mr. Warburton, at all events, may rest assured that he will be able to obtain no supply of any consequence from bequests, as long as dissection is looked upon as infamous and penal.

Another feature in the provisions of the old bill, and which we find has undergone a complete *modification* in the new one, is that which related to the

licensing of the schools. There are to be *no* licenses, it seems, under the new system; free trade is to be the order of the day; and whoever can get bodies without exhumation, burking, or robbery, may dissect them. What can be more liberal than this, considering the profusion of subjects which the recently more enlightened state of the public promises to pour upon anatomists? But in sober earnest, we never yet did hear any reasonable objection urged against the licensing system. Among the many reasons for dissent put forth by the opposers of Mr. Warburton's first measure, we know not that there was one more unfounded than this; and what appeared to us most curious and unaccountable in the objection, was, that it came from the very people who cried the loudest for the introduction of the French mode of management. We are for the French mode too; but in being so, we are decidedly not for omitting the regulating power which makes the most essential part of it. If the government take the supply of subjects for dissection under their own superintendence (as eventually must be the case, though it may not be so provided in Mr. Warburton's new bill) they have a right, and it is their duty, to require that every one who teaches anatomy shall be provided with a license. It is any thing, then, in our opinion, but an improvement in the proposed bill, that it should be without a clause for rendering licenses imperative.

In short, we cannot help concluding with the remark, that we see nothing in this new piece of legislation to induce us to think favourably of it. The supply of bodies, we are persuaded, will be left in as precarious a state as ever; and the dispensing with licenses, we have no doubt, will prove extremely ill-advised—not only from the irregularities which it will give rise to, but from the injurious operation of free-trade principles, in deal-

ing with a commodity so precious as the human body is likely to be, under the provisions of the contemplated measure. It is scarcely necessary to add, how absurd and prejudicial to the interests of anatomy, every person must admit one other particular of the New Bill to be: we allude to the appointing of inspectors, and the insisting of registers being kept, under the new state of things—while there is no step taken for securing any thing like an adequate supply of subjects. What is this but to compel anatomists to engage in a contraband traffic, and then to keep a regular ledger of the goods that may have passed through their hands? Preposterous in the extreme!

ABSTRACT OF THE BILL.

WE have just been favoured with the perusal of this document: it is certainly one of the shortest of the kind we have ever seen, amounting altogether, with all its technicalities, to no more than three folio pages and a half. After a brief preamble, it proceeds at once to business. 1. Inspectors of the schools of anatomy are to be appointed by the Secretary of State. 2. These inspectors are to visit any place where anatomy is carried on; to receive returns and certificates; and are to be paid by salary. 3. Executors and administrators may permit bodies, of which they have lawful custody, to be anatomically examined, unless the person, when alive, have expressed in writing, or orally, in the presence of one or more witnesses, his repugnance to such examination. 4. Those bodies to be removed only by certificate, signed by the medical attendant of the deceased, or some other medical man; the certificate and body going together to the anatomist. 5. Graduates or licentiates in medicine, or members or fellows of any College of Surgeons, or professors, teachers, or students of medicine or

surgery, may lawfully receive, possess for examination, and examine such bodies as aforesaid, provided with the necessary certificates. 6. The Inspector to get the certificates within *twenty-four* hours, and the party receiving the body to enter the particulars in a book kept for the purpose. 7. The Anatomists aforesaid not to be liable to prosecution, penalty, or punishment, for any offence against this act, unless proceedings be instituted by the Attorney-General, or by leave of the King's Bench, in England, or by the Public Prosecutor in Scotland. 8. The act does not interfere with Post-mortem examinations. 9. So much of the 9th Geo. IV., c. 31, as directs that the bodies of Murderers may be dissected, is to be repealed,—the body, instead, to be hung in chains, or buried on the highway. 10. And the act is not to extend to Ireland.

PRETENDED "CURES" FOR CHOLERA.

OUR readers may possibly remember that the week before last we alluded to the astounding fact, that a well-known physician at Sunderland had written to the government, announcing the discovery of a cure for cholera, and intimating his readiness to make known the same—for a consideration. We confess, however, that we were not prepared for the *denouement* which followed a few days after by the appearance of the following letter in one of the daily papers:—

Sunderland, Dec. 17.

Mr. Editor,—I have the satisfaction of informing you that I have completed my analysis of the blood of epidemic cholera patients, and *ere long* shall lay before the public the results of my investigation and method of cure.

I have the honour to be, Mr. Editor,

Your most obedient humble servant,

W. REID CLANNY, M.D.

Member of the Sunderland
Board of Health.

Now we shall dismiss Dr. Clanny and

his pretensions very shortly. The rate of mortality at Sunderland, when cholera broke out there, was as nearly as may be one in three, and *it continues undiminished*. It thus appears by his own shewing, either that he has no "method of cure," or that the said method has been withheld from his suffering townsmen. Is this a moment for any man who conceives he has discovered any thing tending to diminish the danger, and allay the fears excited by cholera, to talk of communicating it *ere long*. We were amused to learn that our notice produced two letters to the Board of Health, putting them on their guard, that if — were the person alluded to as possessing "a cure" for cholera, they must be cautious in admitting his claims, inasmuch as the real merit belonged exclusively to him who now addressed them, and who had communicated his discovery to the above. Would it not be as well for gentlemen to begin by proving that there is a *cure*. As Mrs. Glass says, "First catch your hare," &c.

IS IT COMPULSORY WITH MEDICAL MEN TO REPORT CASES OF MALIGNANT DISEASE IF REQUIRED TO DO SO?

ABOUT ten days ago we received a letter from an anonymous correspondent, in which some doubt was expressed as to whether practitioners were obliged by law to report cases of malignant diseases when required so to do.

Our former observations were made in consequence of having ascertained that the legal authorities who had been consulted had given it as their opinion that the Privy Council had it in their power to compel medical men to make the required returns. On referring to the statute-book, we perceive that there does not exist any specific enactment on this particular point; but the general clause, to make "such order as they shall deem necessary and expedient,"

we are informed, by what we regard as a competent legal opinion, empowers the authorities to require the returns alluded to, the contemning party being subject to prosecution, and amenable to the penalties mentioned in the last of the subjoined extracts.

Extracts from the Statute-Book, in reference to the Quarantine Act.

“And be it farther enacted, that it shall be lawful for the Lords, and others of His Majesty’s Privy Council, or any two or more of them, to make such order as they shall see necessary and expedient upon any unforeseen emergency, or in any particular case or cases, with respect to any vessel arriving, and having any infectious disease or distemper on board, or on board of which any infectious disease or distemper may have appeared in the course of the voyage.”

“And in case of any infectious disease or distemper appearing, or breaking out in the united kingdom, or the island aforesaid, to make such orders, and give such direction, in order to cut off all communication between any persons infected with any such disease or distemper, and the rest of His Majesty’s subjects, as shall appear to the said Lords, or others of His Majesty’s Privy Council, or any two or more of them, to be necessary and expedient for that purpose.”

“And be it farther enacted, that all offences committed against any of the provisions of this act, for which no specific penalty, forfeiture, or punishment is provided by this act, shall and may be tried, heard, and determined before three justices of the peace of the county, riding, division, city, or place, where such offence or disobedience shall happen; and if any person shall be convicted of any such offence or disobedience, he or she shall be liable to such forfeiture or penalty, not exceeding the sum of five hundred pounds for any offence, or to such imprisonment, not exceeding twelve months, for any one offence as shall, in the discretion of the three justices of peace, be judged proper.”

PROGRESS OF THE CHOLERA.

The cholera is spreading progressively, but not very rapidly, in the north of

England. Its course is not essentially different from that it has held in other countries during the winter. The rate of mortality has been a trifle above one in three, as appears by the following table, including the reports received in London to the 23d:—

Sunderland	Total cases, 511—Dead, 188
Newcastle	153 53
North Shields & Tynemouth, 7	5
Houghton-le-Spring	8 3

By a letter we have just received from Mr. Greenhow, of North Shields, we perceive that the chain of connexion, from patient to patient, has in that quarter been, for the most part, complete, and easily traced. Dr. Daun states the same fact with regard to Sunderland, from whence he has returned, satisfied that the disease is contagious.

DR. HAMETT ON CHOLERA.

We beg to direct attention to Dr. Hamett’s valuable papers at page 441; they contain a most excellent description of the symptoms and post-mortem appearances of cholera. The writer is a non-contagionist, and we shall next week give his views upon this part of the question. By the way, we have received a paper against contagion, which the author seems to think we may reject, on account of his opinions differing from our own. Be it known to all whom it may concern, that we have no object in view but the discovery of truth; and this is most likely to be accomplished by hearing both sides of the argument.

ANOTHER DESCRIPTION OF CHOLERA.

We last week gave Magendie’s pithy sentence on cholera: how different the style of M. Lemery, a countryman of his, who lately presented a memoir on the subject to the Academy of Sciences. He began with the words, “*Semblable au papillon léger — !*” His learned auditors, who had composed their faces with due solemnity on the announcement of the paper, were so entertained with this unexpected commencement, that it was received with shouts of laughter, and sent to the “Cholera Commission” without another word being heard.

COLD AFFUSION IN CHOLERA.

THE cholera is rapidly declining at Berlin; the rate of mortality has been above one in two, 1390 having perished out of 2219. The principal fact ascertained, with respect to treatment, is the efficacy of *cold affusions*. This is the most efficient method of cure*.—*Gazette d'Augsbourg*.

EFFECTS OF INTEMPERANCE.

THESE are fearfully illustrated in a report of the American Temperance Society, lately published. It is calculated that between twenty and thirty thousand persons annually die from excessive drinking, and that more than two hundred thousand are thereby afflicted with disease, and plunged into misery. The number of paupers, almost all reduced to penury by drinking, who were admitted into the infirmaries at Philadelphia, amounted in 1823 to 4908; in 1824 to 5251; in 1825 to 4393; in 1826 to 4272. The hospitals and penitentiary at New York contain about 2000, reduced to disease or want by intemperance.

WESTMINSTER MEDICAL SOCIETY,

Saturday, Dec. 17, 1831.

MR. CHINNOCK IN THE CHAIR.

Galvanism in Cholera.

WITH the permission of the society, the chairman read a paper from the pen of Dr. Wilson Philip, on the nature and treatment of malignant cholera, of which we are enabled to lay before our readers the following ample abstract:—

The author began by observing, that it is not until we have become acquainted with the nature of a disease, that we can pretend to restore the functions of the organs primarily affected, on which the affection of the rest depends. With regard to cholera, the attempts hitherto made to elucidate its nature have been unsuccessful; yet it may not be deemed presuming in Dr. Philip to endeavour to throw light on the subject, when it is recollected that for nearly twenty years he has been engaged in inquiries into the laws of the vital functions. He would confine himself to a general view of the nature of the complaint, and point out, on very simple principles, that all the symptoms of

malignant cholera are the necessary consequences of "any cause capable of making the peculiar impression upon the system which that of this disease evidently does;" and for the better understanding of those principles, he would refer to the physiological facts contained in his work on the vital functions, and his papers in the *Philosophical Transactions*.

It appears from direct experiment, that the more perfect animals possess three distinct powers—the sensorial, nervous, and muscular powers; that none of these have any direct dependence on the others, but are all so connected through their organs, that each is more or less directly capable of influencing the others, and none can long survive the destruction of any one. The sensorial power is capable of directly influencing the organs of the nervous power, even to its immediate and total destruction. The same is true of the influence of the nervous on the organs of the muscular power throughout every part of our frame; and although the muscular power has no such direct influence on the organs of either of the preceding powers, it has, through the medium of the circulation, an influence of equal extent on them. It is essential, in either tracing the laws of the animal economy in health, or in unfolding the nature of its diseases, that we should be able clearly to determine what functions belong to each of these powers. The function of the muscular power being simple, and well defined, is easily distinguished. In distinguishing the numerous and more complicated functions of the sensorial and nervous powers, there is more difficulty. Two sets of experiments, conducted on different principles, were instituted for the purpose; the object of the one being to determine what functions cease after the nervous power is withdrawn, and of the other, what functions remain after the removal of the sensorial power; and the results of these sets of experiments in all respects corresponded, and served to confirm each other. From them it appeared that the functions of the nervous power are, 1st, under certain circumstances, to excite the muscles of involuntary motion; 2dly, to cause an evolution of caloric from arterial blood, by which the temperature of the animal body is maintained; 3dly, to form from the blood the various secreted fluids, whether excrementitious, or for the purposes of the animal economy; and, 4thly, to maintain the other assimilating processes, by which the healthy structure of every part of the body is preserved.

Keeping these positions in view, it is next to be considered what are the necessary consequences of an injurious impression made on the nervous system, of so powerful a nature as immediately to impair all its functions. The voluntary powers are of course enfeebled; and from the immediate influence

* This remedy is recommended in cholera by Mr. T. H. Smith, in a communication just received.—E.G.

of the nervous on the muscular system, and consequently on the heart and blood-vessels, the circulation languishes, these effects being greater or less according to the force and suddenness of the impression made on the nervous system, and the ~~ten~~ability of that system at the time to suffer from the impression. In proportion to the debilitating effect produced on the nervous system, the process by which the temperature of the body is supported, fails. In the same proportion, the functions of the various secreting surfaces is deranged. The air cells and tubes of the lungs, for example, become clogged with phlegm, in consequence of which the air can no longer effect the necessary change on the blood; the temperature therefore fails.

The other assimilating processes also necessarily fail, so that the due structure of the vital organs is no longer preserved. The lungs in particular, from their structure, being more evident than that of the other vital organs, when the cause is very powerful, but not sufficient immediately to destroy life, are soon observed to lose the healthy structure. Their air-cells and tubes, it has been found by repeated experiment, are at length obliterated, and they assume a solid and fleshy appearance, as found, it may be remarked, in the lungs of those who die of malignant cholera, strikingly illustrating the similarity of the cases. As these changes proceed, the breathing necessarily becomes oppressed, the whole body cold, and the surface pale, and at length dark-coloured. The body cools on the same principle as that in which life is extinct; but the lungs still more rapidly than in the dead body, because while respiration continues, they are exposed to the cooling influence of the immediate contact of the external air, which is not the case in the dead body. Thus at length the expired air is no longer heated, but remains of the temperature of the atmosphere, and from the moisture it has imbibed from the lungs, to the hand feels still colder. Are not these, which are the necessary effects of a loss of power in the nervous system, the leading symptoms of the more severe cases of malignant cholera?

Dr. Philip then proceeds to state, that, in his opinion, the great peculiarity by which this disease is distinguished from all others, is, that in it the nervous and sensorial powers cease together, the sensorial functions only ceasing in consequence of the ceasing of those of the nervous system; whereas (except in cases when death ensues from an extreme and sudden injury of either of the two systems, when life becomes instantly extinct), in all the modes of death with which we are acquainted, the sensorial functions are always the first to cease. It is owing to this peculiarity that, in cholera, the mental faculties remain to the last; the cause of the disease making its impression on the nervous system alone, and the sensorial having no direct dependence on the

nervous power. With the help of these considerations, Dr. Philip thinks that little doubt can remain with respect to the nature of the disease; nor would it fail to be placed in the clearest point of view, if there were time allowed him to examine each of the characteristic symptoms separately: one of them, however—the discharge from the bowels, which so frequently attends it—should be made the subject of a few remarks. The author has proved, by experiment, that when secreting surfaces have suffered an injury in their nervous power, they pour out an increased quantity of fluid of a depraved character. Thus, when the eighth pair is divided, though without cutting off the influence of the brain, the secretion of the stomach is found much more copious than in health; while the food is found comparatively dry and wholly indigested, when the cerebral communication is completely cut off. It must be on this principle that the diarrhœa is looked upon as rather a favourable symptom than the contrary. It is the consequence of the nervous power of the bowels being to a great degree impaired, yet not wholly destroyed. So also with regard to the vomiting and hiccup; for, in the worst states of the disease, the nervous system is incapable of producing these symptoms. In like manner the spasms of voluntary motion, the peculiar state of the breathing, the intenseness of the dark colour of the blood, and consequent appearance of the patient, the change in the other properties of the blood, its accumulation in the vital organs and recession from the surface—all these, it would be easy to shew, are the necessary consequences of a certain and sudden diminution of nervous power. But a more extended consideration of these heads, as well as of several practical inferences suggested by them, were precluded by the limits of the paper. The author, however, wished to take the earliest opportunity of urging a trial of two remedies: one of these was the application of powerful irritants applied to the occiput and along the whole course of the spine. Near those parts lie the origins of all the vital nerves—where the failure of power evidently exists; and this corresponded with the practice of Dr. Lange, of Cronstadt, recently recommended in strong terms by Dr. Barry. The other remedy was the employment of Galvanism, already suggested by several writers, and particularly by the author of a paper in the *Medical Gazette*, who relates a case in which it was employed with advantage.

When we see that galvanism is capable of perfectly restoring the functions of vital organs after they have been wholly destroyed by dividing and displacing their nerves, we should at first view expect to find it a specific in the disease before us. But there are two circumstances which tend to check this sanguine expectation. We are not sure that

the nervous power is merely withdrawn in malignant cholera: it may be vitiated by the continued action of the poison which produces the disease; and how far galvanism can counteract its morbid state can only be determined by a trial; and if it is capable of counteracting it, it is still doubtful whether the degree in which it can be safely applied will be sufficient for the purpose. In supporting the function of a vital organ, a very considerable galvanic power is required. It is not to be supposed that in our clumsy mode of applying it, the same degree of power will be sufficient as when applied by nature herself.

Dr. Philip concluded with some remarks on the identity of the nervous and galvanic powers.

A very desultory discussion followed the reading of Dr. Philip's paper, and though some few good remarks were made, yet they bore so small a proportion to the whole, that our readers will lose little by our declining to record them; a decision, by the way, which our narrowing space scarcely leaves optional.

Adjourned till Saturday, the 7th of January, 1832; when Dr. WYRTE is to open the debate with an account of his practical experience in the disease.

ST. BARTHOLOMEW'S HOSPITAL.

Removal of a Congenital Tumor.

HENRY HEELIS, *æt.* 15 months, was admitted into the hospital December 15th. This child, who is remarkably healthy, was born with a tumor of about the size of a small hazel nut, situated at the upper and inner part of the left side of the nose. The mother says that it was very firm, and almost immovably fixed. For a considerable time its increase in size was not very perceptible, but within the last two or three months it has grown very considerably. The tumor now acquired the size of a walnut, was very hard, and firmly connected to the parts beneath. The skin covering it had a remarkably shining appearance, and was adherent to the substance of the tumor. No pain was produced even when roughly handled during the examinations which were made of it. The parents of the child had applied to several practitioners, who had employed various embrocations, &c. without effect; they therefore consented to its removal by Mr. Earle; and this was done on Saturday.

Two incisions were commenced near to the inner corner of the left eye, and were continued downwards over the front of the tumor to near the lower part of the left nostril, including an elliptical portion of skin. The integuments were next carefully dissected off the tumor on each side, and, being freely exposed, the connexions of the latter, which existed with the cartilages of the nose, were divided with a small scalpel;

care being taken to dissect it off from the cartilage without penetrating into the cavity of the nostril. A small vessel bled rather freely, but it was not deemed necessary to place a ligature upon it. The edges of the wound were now brought accurately together, and maintained by three sutures.

The substance of the tumor was of a remarkably firm and white texture, and in some parts very similar to the cartilage of the nose, with which the tumor almost appeared identified. The little patient is doing quite well, and the cicatrix promises to be very small.



The case of Extirpation of the Maxilla.

Mary Cave, the poor woman upon whom Mr. Earle operated, on last Saturday fortnight, has daily been gaining strength; her appearance has wonderfully improved, and the whole line of the external wound is completely cicatrized, excepting at one small point in the upper lip. The mouth has been fast filling with healthy granulations, but at one point—viz. from the posterior edge of the palatine process of the superior maxillary bone—there has been a reproduction of the same disease which had been removed. This, which equalled in size a small nut, was separated yesterday with a scalpel, and the concentrated nitric acid applied to the divided surface. The slough has not yet separated. The ligature which had been placed on the carotid artery came away yesterday, and the patient is to-day quite comfortable.

[N.B. We wish to observe, that in the woodcut which accompanied our report of this case, the cheek was by no means sufficiently prominent.]

ST. GEORGE'S HOSPITAL.

Aneurism of the Subclavian Artery—Operation—Pathological Appearances.

THOMAS ANTRIM, *æt.* 50, a stableman, living

at Chelsea, admitted into the hospital November 17, 1831, under the care of Mr. Brodie. A pulsating tumor is to be felt at the upper part and right side of the chest, in the hollow formed by the clavicle, ribs, and axilla. It is about the size of a small lemon, having its long diameter placed in the direction of the subclavian and axillary artery. The pulsation is exceedingly distinct, and the peculiar thrilling sensation usual in aneurism is communicated to the hand. Pressure on the artery above the clavicle completely arrests the pulsation and diminishes the tumor, which, if examined under these circumstances, appears to contain some coagulum.

He complains occasionally of coldness and slight numbness in the right arm, but does not experience pain or uneasiness in any part of the body. He can use the right arm freely, and states, "that he is not conscious of the existence of the tumor, unless he places his back against the wall, or any other firm body, when the pulsation in the part becomes very distinct."

Pulse 62, slightly irregular, equal at both wrists. The arteries throughout the body pulsate more distinctly than natural, and with greater rotundity and fulness. No cough, pain in the chest, or palpitation; tongue rather white; bowels regular.

About eight weeks ago, he experienced violent pain in the right arm, chiefly in the course of the muscular spiral nerve; it was always relieved by moving the limb. First discovered the tumor seven weeks ago, when shaving. It was unattended by pain, and has continued so up to the present time. He has not met with any strain or other injury in the situation of the tumor, and has had light work and good health for the last eight or nine years.

His diet was regulated, his bowels kept open, and he had twenty drops of tincture of hyoscyamus in camphor mixture, twice a day.

A consultation was held, when it was agreed that the subclavian artery should be tied, and Mr Brodie performed the operation on the 13th of December.

Operation.—An incision, about three inches in length, was made in the integuments, just above the clavicle, commencing about an inch from the sterno-clavicular joint, and extending directly outwards and slightly upwards. This was effected by first drawing down the skin over the clavicle, and cutting on the bone itself. This wounded one or two superficial arteries, which were secured. The superficial fascia of the neck and platysma myoides were next divided, together with some of the adipose tissue immediately beneath. This exposed the external jugular vein, which was tied with two ligatures and divided between them. The dissection was now continued with a blunt silver scalpel and forceps, great care being taken to expose as little as possible the nerves which go to form the axil-

lary plexus. Some short time elapsed before the artery was fully exposed, the pulsations of which were now distinctly visible to many of the by-standers. It was situated rather deeply, much cellular and adipose tissue being interposed between it and the skin. The transversalis colli and omo-hyoideus were visible; the scalenus anticus was felt, but not dissected. The subclavian vein was seen below, and a nerve of some size ran just above, and rather before, the artery. A small vein which descended from the neck, was also perceptible. The artery was evidently dilated to nearly twice its natural size; and the parts in the neighbourhood were consolidated by lymph, which had been previously effused. A curved silver needle with a handle, armed with a single ligature, was employed for taking up the artery. In consequence of the artery being deeply situated and dilated, and the surrounding cellular membrane being thickened and consolidated, there was some difficulty in the introduction of the needle. An attempt was made both from before and behind; the latter eventually succeeded. An examination was made to ascertain that nothing but the artery was included in the ligature; this done, the vessel was tied, and the pulsation in the tumor immediately ceased. During the operation several small arteries were divided and secured, but none of any magnitude. The whole arterial system seemed under considerable excitement, and the brachial artery on the right side was observed pulsating, and exceedingly tortuous; this appearance ceased as soon as the ligature was applied, and the limb speedily became blanched. A small piece of lint was placed in contact with the ligature, where it emerged from the wound, to prevent union of the skin by the first intention in that situation. The edges of the wound were brought together, and a compress and calico roller constituted the whole of the dressing, except a flannel roller which was applied round the entire extremity.

Vespere.—Very quiet; no pain or uneasiness of any kind; temperature of limb rather increased, as far as could be ascertained by an examination with the hand.

Gr. $\frac{1}{4}$ of Morphiae Acetat. in Camphor Mixture.

14th.—Some refreshing sleep during the night; slight bleeding from the wound, just sufficient to colour the bandages; tumor diminished in size; no pulsation there, or in the right radial artery. The right arm is warm and moist, equal in temperature to the opposite extremity; complains of some pain in the course of the radial nerve in the right arm. Skin hot; face somewhat flushed; pulse 96, round, full, bounding—irregular and intermitting about every fifteenth beat; respiration free; bowels open; makes water freely.

Venæsectio ad $\mathfrak{z}\text{ij}$.

Blood taken in three cups; the first highly

cupped and buffed; the second less so; the third not at all, but the coagulum firmer than natural.

Immediately after the bleeding the pulse rose to 104, more irregular and intermitting.

Hæustus Salin. c. Vini Antimonii, ℞xx.
4tis horis.

11 p.m.—Pulse 104, full; inclination to vomit.

V.S. ad ̄vi. Intermitt. Vin. Antimon.

Hæbut. Haust. Effervesc. 6tis. horis.

About 12 o'clock he threw up about two ounces of watery fluid, tinged with bile, and slightly streaked with blood; is now perspiring profusely; rather restless; no pulsation to be felt in the radial artery, or tumor; no pain except in the fore-arm. Has passed some urine, which is high coloured, and deposits (apparently) the lithate of ammonia with mucus; respiration slightly hurried; blood last taken cupped and buffed; pulse 104, soft.

15th.—Pulse 96, full; skin not so hot, but still warmer than natural; bowels open; no pain, but has slight catching cough, nearly allied to hiccough; tongue furred, white, dry; no sweating nor sickness.

16th.—Looks rather better; pulse 96, irregular, intermitting; bowels open; tongue white and dry; wound dressed, some union. Fish for dinner.

17th.—Pulse 120, weak, intermitting, very irregular; skin warm; has suffered for some hours from slight hiccough; countenance pallid, and rather anxious; manner hurried; wound suppurating slightly.

Beef-teen, arrow root, &c.

18th.—Passed a restless night; rather inclined to sweat; no sickness or rigor. Pulse hardly to be felt; countenance more sallow and anxious; appears on the verge of low delirium; complains of some pain in the great toe of the left foot, where an erythematous blush of redness has appeared, with swelling and tenderness. The toe is white at the extremity, but not cold.

R Mist. Ætheris Comp.

Mist. Camph. aa. ʒss. 4tis. horis.

Spt. Vini Gallici, ʒiv. Vini Rubri, ʒvj.

Sumat. Coch. j. subinde.

Vespere.—Had some brandy, with egg, and took some strong ale and a little wine in the course of the afternoon; pulse somewhat raised, though still fluttering.

19th.—A restless night; the patient evidently sinking. A circumscribed swelling has appeared on the forehead, just over the nose; some redness. Both the feet and legs blue, though warm; pulsation in femoral arteries very weak and irregular. Died at one, p.m.

Examination 24 hours after death.—Body not emaciated. Both lower and right upper extremities of a dark purple colour; the right forearm more especially affected, the back part of a green colour, the cuticle separating. The superficial veins seen ramifying very distinctly over the surface of the body, and found to be filled with fluid blood.

The clavicle was first raised on the right side, parts beneath every here and there infiltrated with pus and serum. The scalenus anticus was then detached from the first rib; pus was found beneath it. The heart was then removed with the lungs; the latter gorged with blood, but not inflamed. There was considerable hypertrophy of the heart. The arch of the aorta, as well as the descending portion, much dilated, with steatomatous and osseous deposit beneath the lining membrane. The right subclavian artery much dilated. The ligature had been applied just where it emerges from behind the scalenus muscle, and a small coagulum occupied that part of the vessel nearest the heart.

The whole of the serous membrane of the artery, together with that of the arch and descending aorta, was of a peculiarly bright scarlet colour, similar to parts which are undergoing inflammation, but there was no deposition of lymph. The artery beyond the ligature was considerably dilated, forming the sac, which was about five inches long and two and a half in breadth. The whole of the sac was filled with coagula, some of which appeared recent and some of older date. The veins were healthy, but pus surrounded the subclavian externally where it passes towards the axilla. The axillary vein was full of firmly coagulated blood. The parts about the artery, vein, and sac, were much thickened.

On removing the sac, pus was perceived to issue from behind the tendon of the subscapularis muscle. The shoulder joint was sound, and no absorption of bone had taken place about the ribs.

The femoral vein and artery, as well as the popliteal, were examined in the right lower limb. The arteries were filled with fluid blood, and of a red colour. The veins were filled with firm coagulated blood, but healthy in their tunics.

Dec. 20, 1831.

METEOROLOGICAL JOURNAL, 1831.

December.	THERMOMETER.	BAROMETER.
15	from 33 to 48	from 29.73 to 29.64
16	29 46	29.76 29.79
17	30 45	29.65 29.59
18	28 46	29.30 29.30
19	31 44	29.49 29.60
20	29 43	29.58 29.63
21	32 45	29.56 29.76

Wind S.E. and S.W. the latter prevailing. Except the 17th, 18th, and 19th, generally cloudy, with frequent rain.

Rain fallen, .725 of an inch.

CHARLES HENRY ADAMS.

EDMONTON, Latitude 51° 37' 32" N.

Longitude 0° 3' 51" W. of Greenwich.

* * For Notices, &c. see opposite page of wrapper.

W. WILSON, Printer, 37, Skinner-Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF
Medicine and the Collateral Sciences.

SATURDAY, DECEMBER 31, 1831.

LECTURES
ON
THE THEORY AND PRACTICE OF
MEDICINE;

Delivered at the London University,

By DR. ELLIOTSON.

PART I.—LECTURE XIII.

Subject of Inflammation—continued.

Diagnosis of Internal Inflammation.

THE diagnosis, gentlemen, of Inflammation, when the inflamed part is visible during life, or when we make an examination after death, I have already spoken of sufficiently; but if it happen that we have to make a diagnosis during life respecting a part which we cannot see, then of course it must be founded upon something else than if the inflammation were visible.

When an invisible part is inflamed, there is almost always pain increased upon *sudden* pressure, frequently upon the most gradual pressure, and there is generally more or less pyrexia or feverishness, or as some call it, *symptomatic* fever. The pain is often throbbing, and frequently there is a sense of weight and tension. It is, however, to be remembered, that although a part be inflamed, *gradual* pressure may sometimes be borne; and it is to be remembered, on the other hand, that *very slight* pressure, a mere brush of the surface, may cause pain when there is no inflammation. In neuralgia,—when a person is labouring under *tic douloureux*, you may sometimes produce the most agonizing pain by brushing the surface in the slightest possible manner: but that is not the case where there is inflammation; such a slight touch does not produce such an intense effect, and, more than that, if a slight touch do produce so great an effect, it is only in inflammation of the skin, and that you can see.

Suppose you are in doubt, notwithstanding the pain is increased by pressure, and there is pyrexia, the best way always is, if the patient's powers will allow it, to treat it as

an inflammation; and you will frequently find that the blood which is drawn away fully justifies your presumption. You will frequently find that the blood is buffed or cupped, or both; but still, if it be not, and you have strong reason to treat the case as inflammation, you must not suppose that you have less reason to imagine yourself right *simply* because the blood is not buffed: generally, however, it will be found buffy, if there is inflammation.

Had there have been a hospital attached to this school, I should have had an opportunity, when speaking of the buffy state of the blood, of shewing you what is meant by that term. I suppose that every gentleman present knows what is buffy and cupped blood; but just before I came to lecture, I saw a specimen, which I put in my pocket, and which I now exhibit. It is an instance of blood both buffed and cupped.

Prognosis.

In regard to the prognosis of inflammation, you are to consider, on the one hand, the age of the patient, and the evident powers of his constitution; and on the other, the violence of the disease, the degree of disposition to it, and its seat. The danger of the affection depends, on the one hand, on the power which the patient has from the beginning of going through it; much must depend, in some measure, upon his age, and upon the general state of the constitution; and it depends, on the other hand, not only on the intensity of the disease, and its liability to increase, but on its seat. An inflammation, exceedingly violent, in an extremity, might not be half so dangerous as one less violent in certain viscera. You are also to calculate upon any idiosyncrasy, whether natural or acquired, in the individual; for example—I believe I have already mentioned the circumstance—I once had a patient who had merely a rheumatic affection of the external part of the head, but he had previously had his skull fractured, and the irritation without

the head produced sufficient irritation within for violent delirium to ensue, so that he was in a most violent state of phrenzy. There was no danger at all in this—it gave way to the ordinary treatment of rheumatism of the external part of the head; but had there not been this idiosyncrasy, one would have feared that an inflammation of the brain was set up of a highly dangerous character. If the man took a glass of spirits, the same effect, I understand, was always produced. You therefore have to consider whether there is any idiosyncrasy or not; for frequently when you are attending a patient for an inflammatory or any other complaint, if he be nervous, you have such a depression of the system, such a quickness of the pulse, such a disturbance of the constitution, as would frighten you if you had not known the patient before.

Treatment of Active Inflammation.

I now proceed to consider the treatment of Inflammation; and first of all, of that which is of an active character.

DIMINUTION OF STIMULI.

Now in the treatment of active inflammation, the first point is to lessen the ordinary stimuli to which the body is subjected, both externally and internally. Excessive action is going on, or there is excessive activity and excitement, and our business is to lessen all the stimuli which maintain that activity.

Exclusion of External Stimuli.

The external stimuli may be diminished by exclusion.

Temperature.—In the first place you exclude high temperature, you procure a moderate temperature, so that its stimulus may act as little as possible. In attending to this point, however, great care must be taken not to allow the temperature to fall too low, for if you chill a patient labouring under inflammation, you not only cause great discomfort, but you most likely increase the affection. By diminishing the circulation in healthy parts too much, it is possible that you may increase the activity of those which are inflamed, so that in lessening the temperature around a patient it is necessary to moderate it only so far as is comfortable to him.

Cold.—However, with respect to the part itself, the temperature there is of course too high, and to the part you may apply cold. Cold may be applied by means of plain water, which generally answers as well as any thing that can be used, if it be continually renewed; but we can procure, of course, a lower degree of cold, if we employ evaporating fluids—those which evaporate more quickly than water. It is often of great use to employ real ice; and for this purpose a bladder may be half-filled with it, and laid on the head after it has been shaved, or on the front of the chest, or on any other part of the

surface that requires it. If you fill the bladder it will not accommodate itself well to the part, but if it be only half-filled, it presses down in close apposition with it. If it be the surface itself which is inflamed, and not the interior of the head or chest (and, indeed, in inflammation of the chest cold is not applied externally; it is only in cases of hemorrhage that we have recourse to it); if it be the surface which is inflamed, it is not well to apply ice in this way, because the pressure would do harm. It is then best to apply plain water, or iced water, or evaporating lotions; but in the case of internal inflammation of the head, or hemorrhage from the lungs, a bladder half-filled with ice is one of the best things that can be resorted to. Certain inflammatory pains of the head have given way to a stream of cold water applied to it, when nothing else would succeed. By means of a tube, a stream of water has been applied to the head for many hours in the course of a day, and the complaint has been cured, when evaporating lotions did not answer the purpose.

Heat.—However, the very opposite treatment is often equally beneficial. If you apply warmth and moisture together, you often produce as good an effect as if you apply cold. It is only a speculation, but I imagine that the reason why two such opposite agents produce the same effect is this—if you apply cold you lessen the stimulus, you lessen the heat of the part, you lessen the quantity of blood in all the vessels, and you therefore lessen the tension; whereas, if you apply warmth and moisture, you cause a relaxation of the inflamed part, you soften the solids, and in that way the tension is taken off. I presume the cold lessens the stretching cause, whereas the moisture allows the part to be easily stretched—to give way to the distending fluids. Perhaps the warmth and the moisture applied to the surface may relax the ends of the vessels, cause a free perspiration, and in that way also ease the parts. Such, however, is the fact, one patient will be as much relieved by warm applications as another will by cold. I have myself frequently suffered violent external inflammations; and one part of the day I have found the greatest relief from iced water, or a stream constantly applied: and when that ceased to produce ease or caused pain, then I have experienced the greatest comfort from the application of warm water and flannels. You will find the feelings of the patient to be the best guide in the application of these means. I never apply cold when it is uncomfortable. If you once allow the patient to become uncomfortable, you may produce not only general chilliness, but make him ill, and frequently increase the inflammation.

Light and Noise.—The temperature of the part, however, is not the only thing to be attended to. If you know that the part af-

fected is one greatly influenced by other surrounding stimuli, as, for example, the head, you should likewise exclude light (this is an important point to be attended to in all affections of the head, and of the brain itself,) and also noise; and of course in all cases prevent the conversation both of the patient and of others, so that you may keep him in as great freedom from stimuli as possible.

Food.—Still pursuing the plan of exclusion, you should give the patient but little food, and even that should be of the most inert kind; in fact you should starve him without letting him know it—pursue starvation in disguise. Plain water would be the best thing in many inflammatory complaints; but you must allow toast and water, or barley-water, otherwise the patient would think you were going to kill him. The object, however, in inflammation is to give as little food as possible, and that of the least nutritious and least stimulating kind. In consequence of the thirst a great deal of drink must be taken, and this should be cold unless the patient wish to have it warm; and if the drinks be made acid, they are of course so much the pleasanter. A saline draught is a good thing, but a few ounces will be productive of little benefit; the patient should have a pint in the course of twenty-four hours, if he has it to do him good.

Removal of Internal Stimuli.

Blood-letting.—Although, however, these things are all highly important, the great remedy in the disease, I need not say, is the loss of blood; that is, you have not only to *exclude* the external, but *remove* the internal stimuli; and the great internal stimulus of the body is the blood. Blood-letting may be performed anywhere. The object is to withdraw blood; and not merely so, but to make as great an impression upon the system by a given loss of blood as possible. Hence it is generally good practice to make a large orifice in the vein, if you open a vein, and to make the patient sit up, so that he may faint quickly. If the blood be drawn with great rapidity, a far greater effect is produced than if it be drawn slowly; and a large orifice tends to accomplish that object; and if a patient be sitting up he is far more likely to faint than if he be lying down. When we are sitting up or standing the blood finds more difficulty in returning to the heart, and escapes more easily from it; and therefore, if there be causes tending to produce fainting, they are far more likely to occasion it in that posture than if we are recumbent. If it be an object to spare blood very much, and yet make as great an impression as possible, it is well to have two veins opened at once, one in each arm, and make the patient stand up; the loss of a very few ounces will then produce fainting.

Now the rule for taking away blood in general, is not to consider the quantity, but to consider the effect—to do every thing you can to occasion a great effect from a given quantity; but while you are doing so, to go on till the pulse sinks, till the patient says he is better, or an evident improvement takes place. Under any of these circumstances, it is well to stop the bleeding, especially if the patient grow faint, lest the depression become too violent. If, however, the faintness go off, and yet the pulse is no better—if the patient feels no better—or if, when he has not fainted, but there has been an improvement of the pulse, the pulse falls back into its former state—or if there have been no faintness, no improvement of the pulse, but a great improvement in the patient's feelings, a great diminution of the disease, and the pain all returns—then, under any of these circumstances, you should let the blood flow again. In general, one never orders a particular quantity of blood to be taken away in an acute disease, but rather a certain effect to be produced, if possible.

Now you find sometimes on bleeding that the pulse, so far from diminishing, will increase in volume, and so far from becoming slower will become quicker, and still retain its firmness. This circumstance may be a very great improvement; there are some diseases in which the pulse is oppressed—in which the pulse may be large and firm enough, but seems to have scarcely any activity in it; it is not a fluttering pulse, but it moves heavily, if I may so speak. In what is commonly called an *oppressed* pulse, there is plenty of volume—plenty of firmness, but very little impetus. This is often the case in affections of the head; the pulse is frequently at the same time very slow, but not necessarily so: a pulse may be oppressed and yet not slow, or there may be a morbid slowness, and yet the pulse may not be oppressed. The one or the other may be the case, and yet, on bleeding, the pulse will rise—it will be sharper or quicker, or both, and this is as much to be considered an improvement as diminished force and frequency in other instances; and I would arrest the flow of blood in both cases. I would then wait and see whether things fell back, and if they did, that is to say, if the pulse became slower, or heavier, but still having sufficient volume, I would let the blood flow again.

The repetition of bleeding after an interval of some hours or of a day, must depend upon the same circumstances. When the pulse becomes quicker or fuller again, or the symptoms become worse, then you should follow the same rules in repeating blood-letting as you did at the very moment you were bleeding.

In general, in violent inflammation there is more difficulty in making a patient faint,

than when the inflammation is less violent, or there is very little the matter with the patient. I believe it is true that very frequently in inflammation, a degree of bleeding will not occasion fainting, which in health, or in a state not far from it, would produce syncope.

Now, although these are the general rules to be observed, it may be necessary to act in the very opposite manner. If a patient be very plethoric, so that it appears necessary to take a large quantity of blood—if it appear that you will only knock down the disease for a minute, or an hour or so, without copious depletion, and that all the symptoms may return on account of the great fulness of the system—or, if it so happen that the patient is nervous and thus easily disposed to faint, in either of these cases it may be a great point to prevent syncope before you have withdrawn the quantity of blood you wish, and therefore it may be right to bleed from a moderate orifice instead of a large one; to make the patient lie down instead of sitting up; and if the patient be very nervous—as is often the case with women—and likely to faint as soon as a few ounces of blood come away, before you have taken what is sufficient to make an impression upon the disease, a smelling bottle may be used. If you expect nervous fainting, instead of syncope from the absolute loss of blood—if it is likely to arise more from emotion of mind than from the abstraction of blood, you must make a very small orifice, and more than that, you must make the patient lie down, so that you may be able to carry the depletion far enough to make an impression on the system. If fainting occur when you have only taken one or two ounces away, then you are disappointed; you are obliged to cup or apply leeches, and go fiddle-faddling on for a number of days, when you might have cured the patient in twenty-four hours: and therefore, in cases where it is requisite to prevent syncope, the use of the smelling bottle is proper, a small orifice, and the recumbent posture; whereas, in other cases, all this would be highly improper. When a patient does faint in ordinary bleeding, I need not say that you must not only stop the blood, but lay him down, lest the fainting should become excessive.

Now, though a quick, full, firm, wiry, or jerking pulse, may justify bleeding, with the symptoms of inflammation, yet the absence of any of these states of the pulse will not forbid bleeding, if the symptoms themselves demand it. If there be no extraordinary debility of the pulse, no feebleness of the constitution—if there be neither tender nor extreme old age—if none of these circumstances forbid free bleeding, you may have recourse to it, notwithstanding the pulse gives you no such indication. The pulse may not indicate bleeding, but it still may

justify bleeding. The pulse has been called by Celsus *res fallacissima*—the most fallacious of all symptoms, and, if you depend on the pulse alone, you will be led into error. The pulse will give you highly important information, but if there be a sufficiency of other symptoms to point out the nature of the case—to shew that the person is labouring under an inflammatory affection, though the pulse would not lead you to such a conclusion, you must treat the case in spite of the pulse, provided, however, the pulse does not indicate such debility as would evidently make bleeding improper, or the constitution and age of the patient forbid it. I have known in the most dangerous cases of peritonitis—when the patient was obliged to lie upon his back, with his body and even his thighs raised, so as to relax the peritoneum as much as possible, and where the abdomen could scarcely bear the pressure of the bed-clothes—I have known the pulse scarcely different in volume, or force, from what it was in a state of health. Notwithstanding the peritoneum is a serous membrane, I have seen the pulse without any great firmness, any wiriness, any jerking state—without any thing that would lead you to suppose there was disease of this investing membrane. You will continually see females in a state of pregnancy with a small pulse, such as would prevent you from thinking of bleeding, unless there were other symptoms; but from pressing the part affected, you find bleeding indicated, and from attending to the countenance, and looking at the patient, you see that bleeding will be borne. In the cases of peritonitis to which I have just referred, I have seen the blood, after it has been taken away, both bled and cupped, and the patient speedily recover after a loss, perhaps, of forty, fifty, or even sixty ounces. But it is the same with every other symptom; you are to take all the symptoms together, and seldom place your reliance on one alone; every one is important, but every one has also its relative value.

General bleeding is superior to local: it is speedy, and it is powerful. However local the inflammation—however remote from the heart, even in inflammation of the testicle, or of the finger in paronychia, or whitlow, you will find venesection more powerful, and far more quickly produce an impression upon the disease, than any local bleeding.

I believe that venesection in the arm is just as good as arteriotomy in cases of affections of the head. The great point is simply to get a large quantity of blood, in an acute disease, in as short a time as possible from the system, and venesection in the arm will generally answer every purpose; at least I have never seen any thing to make me pre-

fer opening an artery in the case of inflammation of the head, or inflammation of the eye.

I myself have never had occasion to take away more than thirty ounces of blood at a time, except in one instance, in which forty were required to induce syncope; nor do I recollect having occasion to take away more than eighty ounces altogether in an acute complaint. Of course, I have taken away many hundreds of ounces in chronic complaints, going on with small bleedings for many weeks, or months; but, in very acute complaints, I do not recollect taking away more than I have stated. It is right, however, for you to know that some practitioners have taken away an immense quantity of blood in acute diseases in a very short time, and with success. You will find that 150 ounces of blood are said to have been taken away in inflammation of the lungs in a few days. In the twelfth volume of the *Medico-Chirurgical Transactions*, Dr. Blundell says that Mr. Hensley, a gentleman on whom he could rely, declared that he had taken away, in two cases of inflammation of the lungs, in men, a gallon and a half of blood in five days, and they both recovered. Precisely such a case is mentioned in the *Medical Gazette* for January 1829. Dr. Badeley speaks of having taken away five quarts in five days in a case of peripneumonia, and with perfect success. If a case be very obstinate, and will not yield to proper treatment, you see what there is authority for doing: for these are undoubted facts. I do not condemn such practice; but, from the mode of treatment I have generally adopted, I have never had occasion to have recourse to such profuse evacuations.

I may mention among the effects of bleeding, in reference to the buffed and cupped state of the blood, that two Swiss physicians, who have made many experiments on the blood, Drs. Prevost and Dumas, of Geneva, say, that after bloodletting has been employed, the red particles are found, after a certain interval, to be fewer in number; there is not only a change by the disappearance of the buffiness, but venesection lessens the number of red particles in the blood. When blood is taken away from an individual not unhealthily or in perfect health, we know, indeed, that its effect is to impoverish the blood. It is, however, ascertained in a scientific manner, that the red particles are diminished, so that in bloodletting you not only diminish the mass of the blood, but you lessen its stimulating qualities; you lower its quality while you diminish its quantity.

If a vein cannot be opened, then I would in any case of dreadfully violent inflammation certainly open an artery. I suppose it is always safe to open the temporal artery. If you cannot obtain blood from a vein in a very dangerous complaint, instead of troubling the

patient with cupping or leeches, I would open the temporal artery. I know that some practitioners have opened the radial artery. But opening the temporal artery is safe, and generally affords as much blood as is required.

Local.—However, besides general, it is sometimes necessary to have recourse to local bleeding. Now local bleeding is usually employed in proportion to the less violence of the general symptoms, in proportion to the smaller powers of the patient, and in proportion to the existence of mere congestion of blood, rather than inflammation. If you find the general state of the patient's system not much disturbed, if there be no great excitement or fullness of the pulse, or if you find the patient very weak, and a great accumulation of blood rather than inflammation—if the inflammation is rather of a passive or atonic character—then, according to that proportion, local bleeding is usually preferred to general. But I believe that in many cases where local bleeding is had recourse to, general bleeding would be found to answer the same purpose—at any rate we are to take care never to allow local bleeding, which is performed by cupping or leeches, to stand in the way of general bleeding. It is frequently a better practice, even if the patient be weak, to bleed generally than locally, to set him upright in bed, or make him stand upright, and from a large orifice detract four or five ounces of blood, and produce a great effect, than to apply a number of leeches, and drain away perhaps a larger quantity of the vital fluid. I believe that this is often much the better practice of the two; and I am satisfied that general bleeding is continually omitted when it might be advantageously adopted, when it would produce a much more decided effect, and give far less trouble.

I believe that local is often of great use immediately after general bleeding; you lessen the load of blood in the part very materially by immediately applying a large number of leeches or cupping. I think that this is really a good practice. General bleeding frequently answers every purpose; but I think I have seen the symptoms disappear still more speedily if, after making a great impression upon the system, and lessening the force of blood sent to the part, I lessened the quantity contained in the part by adopting local bleeding.

Local bleeding may be employed to produce general effects; if it be carried very far you may see a patient become debilitated by it—become bloodless. You may see a child during the operation of leeches become exceedingly faint. You may produce these general effects by local bleeding, but it is usually a slow process, and the effects of local bleeding are often very local. In a case of pain of the head I have seen leeches

applied to only one temple, and that has been relieved while the other has not. Frequently, when a patient has had pain of the head, I have seen the occiput cupped, and relief obtained there, while the front of the head has remained as before; and when leeches have been applied to the front, I have known the occiput continue painful. This certainly is not invariably the case, but you will perpetually find the effects of local bleeding to be very local. I am not contending against local bleeding—I employ it extensively, but I am always anxious that it should never supersede general bleeding, if the latter be necessary. It is much better to take a decisive line of conduct, to make a strong impression upon the patient in an acute disease, and then local bleeding, if employed subsequently, has a far greater effect. It is to be remembered that I have been speaking all along of active inflammation.

Local bleeding will sometimes succeed best at a distance. I have known chronic inflammatory diseases of the head that had resisted general bleeding, and local bleeding at or in the neighbourhood of the head, yield to cupping of the hypochondria or leeches to the anus. Some think highly of bleeding at the foot; and the operation of distant local bleeding is termed *revulsion*.

Position.—You may frequently diminish the quantity of blood in a part by position, and this is always to be attended to. Position may increase the quantity of blood in a part, and if you can change the position, of course it would be very bad practice to omit doing so. If a patient have inflammation of the foot, it would be madness to allow it to hang down; or if a patient have inflammation of the hand, it would be equally absurd not to have it kept in a sling. But you may often carry these things farther. It is often of use in inflammation of the foot, not merely to have it on a level with the body, but to have it raised, and the same is the case with regard to the hand. In inflammation of the head it would be absurd to allow the patient to lie with his head low. By attending to these points, though you are not doing any thing which will cure the patient, yet you are doing that which will enable other things to cure him better, and you prevent other things from being counteracted.

Purging.—Next to bleeding, in the way of evacuation, comes purging. This, I need not say, although inferior to bleeding, is highly necessary; for in the first place you remove from the interior of the body a quantity of irritating matter, of feces which are almost always diseased, and will most likely become putrid if they be allowed to remain. You not only remove this, but you are sure to find the secretions themselves diseased; so that in removing the remains of food you also remove such diseased secretion

as would be more or less poisonous, or at least irritating. Besides this, however, you produce an evacuation of liquids from the vessels of the body, and likewise cause a counter-irritation in a part distant from the inflammation. I presume that you do more or less good in the latter way. In all inflammatory affections of the head, chest, and various parts of the body, you find the intestines disposed to become torpid; the excitement occasioned by the inflammation causes a depression of excitement in the intestines; and if you increase their excitement, so as to bring them into full action, you in a proportionate manner tend to lessen the distant inflammation. Still it is right not to give very stimulating purgatives, lest you should overdo the thing, and increase the general excitement. The point is, to give those which thoroughly empty the intestines of their contents, and subsequently to exhibit such as produce a considerable drain; but at the same time, you must select those which effect the object without great irritation.

I must here remark, that you are not to imagine, because there are very diseased stools produced, that you are to go on purging for the purpose of bringing such stuff away; for there can be no doubt whatever that purgatives, especially if acrid, will cause a diseased secretion. If a person in perfect health take an acrid purgative, you will find his feces not exhibit the healthy character they presented previously to the purgative being taken. There can be no doubt that many purgatives, especially if they possess any acrimonious quality, will disorder the secretions and produce a fetid discharge from the intestines, which would not otherwise take place. It is not necessary that a patient should have diseased secretions in inflammation and other diseases before you administer purgatives; it is, doubtless, continually not the case, and it is to be remembered that purgatives will have this effect; and after a time, when the feces would assume their natural appearance, they may be kept in an unnatural state from the administration of purgatives.

With respect to the repetition of purgatives, as also the repetition of bleeding, that must all be regulated by the violence of the disease on the one hand, and the strength of the patient on the other.

Sudorifics and Diaphoretics.—In regard to other evacuations, I do not think much of them; venesection and purging are by far the chief. Sudorifics are of very inferior use; and, for the most part, if you leave the skin alone, and well combat the complaint with other remedies, you will find that the skin will resume its healthy function, and the patient perspire. I seldom take any trouble whatever with the skin, by the administration of either sudori-

fics or diaphoretics. By merely bleeding the patient well, starving him, excluding stimuli, and applying such remedies as I shall presently speak of, you may generally neglect the skin as well as the urine. In inflammatory complaints the urine will come round without any direct means, when the disease is combated, and so will the skin; at any rate, if you give stimulating sudorifics you do great harm. In regard to the exhibition of antimony, as it is generally exhibited in inflammatory complaints, for the purpose of producing a moderate diaphoresis, you may just as well treat the patient without it. A few drops of liq. antim. tartarizati, in a saline draught, cannot do material good in a severe inflammation: so trifling is the power of small doses of this medicine, that I have now patients at St. Thomas's Hospital who have taken two or three drachms every three hours, to shew how it can be borne, without any sensible effect whatever being produced. The idea of a few drops of antimonial wine producing any serious effect in inflammation, is absurd. If you well combat the disease with local and general bleeding and purging, you will find that small doses of it are just as well omitted as not; and I never think of saline draughts, or a few drops of liq. antim. tartarizati, or any of these pretty things, for the purpose of curing inflammation.

CONTRA-STIMULATING.

There is, however, another plan to be followed in the treatment of this disease, and that is, to exhibit counter-irritants, or, as they are called, *contra-stimulants*, so as to produce a strong impression upon the system. The chief of these, I believe, are colchicum, digitalis, and antimony, in very full doses, and mercury. In violent inflammation, and indeed in many violent morbid states of the body, all remedial agents are opposed. You may give in fever, you may give in inflammation, you may give in insanity, you may give in spasm, far larger doses of medicine than you could exhibit in health. In inflammation some say they have given an ounce of tinct. digitalis in twenty-four hours. I never did; or a scruple, or half a drachm, of the powder, in the same time. It is certain, however, that in this disease you may give a far larger quantity than you could in health, and it may be laid down as a general rule, that all agents are resisted. Certainly that is the case with mercury, of which you may give a quantity which, in the same individual in a state of health, would induce the most violent ptyalism, and cause all the teeth to drop out. You see that, in violent inflammation, the effect of bleeding on the heart is resisted—the system is in a new state, and many things will not produce that effect which they would if it were in its natural condition.

Digitalis.

Of the first of the *contra-stimulants* which I have enumerated, I have not any great experience. Of the use of digitalis in inflammatory diseases, I have little or no knowledge. I know that some say they can cure inflammatory affections with it, unaided by venesection;—so far do some go; but I have really a horror of digitalis. I have seen so many people die suddenly who were taking it, that, whether they died from it or not, it is a medicine of which I am particularly shy; and, knowing the effect of bleeding, together with some other remedies, I have not had recourse to it in any quantity sufficient to control the circulation. I have exhibited it in the dose of a scruple or half a drachm of the tincture three or four times a-day; but I never saw it useful in inflammation, and as I never gave larger, I cannot say much of it from my own experience.

Colchicum.

As to colchicum, there can be no doubt of its extraordinary powers in active rheumatism and gout. There can be no question that so strong an anti-inflammatory remedy as I suppose colchicum is, will produce sweating, purging, and vomiting; and nausea has a great tendency to lessen inflammation; but I cannot say that I have been so satisfied with it as to make me trust much to it. With the exception of its employment in gout and rheumatism, its utility has not appeared to me so decisive as to induce me to exhibit it in preference to mercury.

Antimony.

With respect to antimony, it may be given, it is now well known, in far larger doses than could be imagined. I believe it is now ten years ago since I found pulvis antimonialis to be in general a very inert thing. I have seen others give two or three grains three or four times a-day, or five-grain doses at night, to make persons perspire, ordering them at the same time to take plenty of gruel, to bathe their feet in hot water, and put plenty of clothes on the bed; and if they have sweated profusely, it has been all attributed to the antimony. Seeing these things, I was in doubt as to its powers, and I went on till I gave people a drachm or a drachm and a half, and even two drachms, three times a-day. I was satisfied it could not arise from the remedy being resisted, and I therefore gave it to patients labouring under itch. It is well, when a patient is rubbing in sulphur, to give something internally, to gratify his wish to have his blood purified. In the cases to which I am referring, I gave the antimony with no effect. I forgot to bring it with me, but yesterday stumbled on an old hospital ticket, in which I prescribed 130 grains three times a-day, without its producing even nausea.

In fact, it is a very uncertain remedy; it contains a great deal of phosphate of lime and peroxide of antimony, and sometimes it contains nothing else; but if you give it with calomel, you may sometimes produce nausea. I will not say that pulv. antimony, as it ought to be, is an inert remedy: but such as it is commonly to be procured may be given in the quantity I have just stated in a large number of cases, with no effect at all.

With respect to the antimonium tartarizatum, if it be ever so good, it may be given in large quantities. There are many men now in St. Thomas's Hospital, taking half-ounce doses of antimonial wine, in chronic bronchitis, every four hours, without its producing nausea or any thing else. They said, when first taking it, that it made them sick, but they have not felt so since. Many persons give twenty grains of tartar emetic in the course of twenty-four hours; and I have given, in inflammatory complaints, two grains every two hours, which has induced nausea or vomiting at first, but none afterwards; and then, after going on for perhaps a fortnight, the patients will perhaps be sick again. I have used it in the way I have stated, but I cannot say that I have been satisfied with it; it has not produced the salutary effect I desired. I dare say you may sometimes save a patient's life with it, by the nausea, but you will not save so many as a good practitioner should try to do; nor do I think that colchicum or digitalis are to be depended upon like mercury—I think not. I have given colchicum and antimony fair trials, and perhaps I am not contented without great success in inflammation, because I think if we have fair play, we ought generally to cure it. It is an established truth, however, respecting tartarized antimony, that in inflammatory diseases you may give it in these large quantities, and, although a patient may be made sick at first, yet the sickness will go off and the remedy be borne. I have, however, generally seen the sickness come on at the end of about a fortnight, and then continue as long as the remedy is continued.

I will proceed to the consideration of mercury at the next lecture.

A HISTORY
OF A
REMARKABLE CASE OF ICTERUS,
WITH HYDATIDS IN THE LIVER.

By EDWARD BLACKMORE, M.D.

To the Editor of the London Medical Gazette.

Plymouth, Nov. 25, 1831.

SIR,

Mrs. G. 32 years of age, was treated five years ago for hepatitis, since which

she has habitually suffered from cramp in the stomach, flatulency, a fastidious appetite, and costiveness. The use of brandy, which had been prescribed for her, invariably aggravated these complaints. The menstrua were irregular and painful, and at times immoderate. She has been married four years without offspring. Since her residence in Devonshire, during the last eighteen months, her health has been improved. She had icterus, however, twelve months ago, which disappeared without medical treatment.

In September 1831, she consulted Mr. Lyon, of Stonehouse, who found her to be affected with the ordinary symptoms of inflammation in the liver. Twelve ounces of blood were then drawn, eight leeches applied on the side, saline purgatives given, and calomel with antimony, and subsequently the Pil. Hydrarg. to ptyalism. The blood was buffy. The bowels remained costive, and the stomach was affected with severe spasms, after which icterus again appeared. About this time a vesicular mole was discharged from the uterus, after an interruption of the menstrua for three months. Mr. L. persisted in the use of purgative injections, blisters, the warm bath, embrocations, antispasmodics, diaphoretics, the acetate of morphine, and occasional cordials, until my first visit on the 20th of October. The icterus was then intense, with pain at the left præcordial region, acute on pressure; costiveness, extreme debility, and oppression. The gums were ulcerated and white from the mercury, but with little salivation: there was daily severe vomiting and inappetency; the pulse quick; the tongue dry, glossy, adhesive, such as is seen in diabetes. Opiates, brandy, and a warm bath, had been used the day before.

Mist. Salin. c. Vino Colchici.

Enema bis die. Hirudines epigastrio.

Pil. ex Rhæo et Scammonio.

21st.—Much fever through the night; the hands now hot and dry; the pulse 120, tense; thirst; urgent vomiting of shreds of green mucus or albumen, resembling grape skins, one being a perfect cyst (hydatid); a similar matter discharged from the intestines; much prostration.

M. Sang. ad ʒviij. Cataplasma epigast.

Magnes. c. Vin. Colchici ter die (C.)

Calomel gr. ij. vespere.

The blood was very buffy and cupped; the urine like brandy; the stools green and bilious.

22d.—The fever gone; more membranes ejected by vomiting and in the stools, before which the stomach was much oppressed. The saline seemed to disorder the stomach whenever it was in the least acidulous. The countenance is less icteric.

(C.) cont. Enema; Sinapism on the legs.

In the evening, the coldness of the extremities and the disorder of the stomach were relieved by the mustard; pulse 86, soft.

23d, 9 A.M.—A severe return of vomiting in the night prevented sleep; more of the green mucus and membranes discharged; thirst; a dry tongue; the hands and feet hot and dry; much prostration; little pain. The solution of acetate of morphine had been given by Mr. L. for the vomiting to day.

M. Sang. \mathfrak{z} iv. Pediluvium; fatus; (C.) repeated.

Evening.—Profuse perspiration, with sensible relief; the fever and vomiting subsided; pulse 80, soft; five stools, containing some bilious mucus, were passed in the last twenty-four hours, from the pills (three), which have been unintentionally continued; the bowels are now quiet. Some gruel seemed to disorder the stomach.

24th.—At eleven last night, the vomiting of entire green hydatids returned, some being as large as Lisbon grapes; no sleep; some fever now present; the tongue clean; pulse 80.

Infus. Rhæi c. Solut. Alkalina.

Enema emol. of Carrageen Moss.

Diet of Sago and Tea.

Evening.—Much relief; the bowels have been well moved by the clyster; the belly is not tumid or tender; a sense of scalding in micturition; much less icterus.

Contin. D.

St. Tinct. Hyoseyam. \mathfrak{gtt} . xxx. c. Liqueur. Ammon. Acet. \mathfrak{z} ij. vespere.

27th.—There has been much sensible relief from the medicine; but on using some purgative medicine and fowl-broth, late last night, the vomiting of bitter green mucus returned, and four purg-

ing stools, with pain at the stomach and bowels, but no hydatids were discharged; good sleep was afterwards obtained. The fever is now abated; the stomach and bowels quiet.

Intermit. medicam.

Sumat. Solut. Alkalina. \mathfrak{m} xv. 4tâ quâq. horâ.

Evening.—Two doses of the medicine taken, which have rested well; some fever is present.

Sumat. Mist. c. Nitrat. Potass. c. Tinct. Digital. et Vin. Colchici.

28th.—A third dreadful paroxysm has ensued of vomiting of a green bitter fluid, with some membranes and many entire hydatids, attended by irritative fever, cramp, and oppression at the stomach, unlike the former tenderness; the pulse 150, tense; some greenish fluid stools passed.

Missio. Sanguinis ad \mathfrak{z} iss.

The pulse was instantly relaxed from the loss of blood, which was buffy, but not cupped.

Sumat. Tinct. Hyoseyami \mathfrak{m} xxx. Solut. Acet. Morphæ \mathfrak{m} ij. Carb. Ammon. gr. v. 4tâ quâque horâ.

Evening.—Much relief; tea only taken.

29th.—A good night; some perspiration, with sensible relief; no stool, no vomiting; the stomach bears pressure; no tumor or tension palpable; the tongue of a variegated red colour, clean, and moist; pulse 120; some Camphor mixture, with tincture of Hyoseyamus, seems to have excited fever. The skin is easily chilled; the urine less yellow, cloudy; much prostration in the countenance and manner.

Intermit. medic. St. Infus. Rhæi \mathfrak{z} j. ad duas vices; fatus.

Evening.—Profuse warm perspiration, which has relieved the stomach; no stool for twenty-four hours; extreme inappetency, except for tea.

Beef tea and Jelly.

Infus. Rhæi et Gentian, c. Ol. Terebinth. \mathfrak{m} xxx. 4tâ quâque horâ (E).

Pil. ex Opii gr. j. Camphoræ gr. iij. vesp.

An aphthous state of the tongue was first observed yesterday; after the cordial, it became red and dry; thirst severe; the extreme circulation low.

Sp. Camphoræ, c. Extr. Belladonnæ, epigastrio.

30th, 9 A.M.—After the use of one dose of (E), lbiiss. of a green fluid, with mucus, and numerous large hydatids, were vomited, some entire, others broken, before which, in the night, Mrs. G. had complained of heat in the stomach, and seemed to be dying from exhaustion, Mr. L. administered some port-wine negus. No sleep was obtained, although a grain of opium was taken. The bowels are torpid; the stomach is tranquil; the tongue apthous, red, and dry; pulse 140, small, soft; the aspect indicates extreme lowness; some biscuit and fowl broth have been taken; perspiration not profuse. The medicine (E) is said to occasion sickness. The belly below the right ribs is hard, but without tumor, and is a little painful.

St. Negus aliment. Infus. Gentian. c. Camphoræ.

Evening.—The countenance is less sunken; she desires to masticate meat; the medicine (E) has been taken well: no more vomiting.

Con. Med. and Beef-juice.

[From this time to the 16th of November, she continued sometimes rather better, sometimes worse; occasionally passing a few hydatids, and generally taking light nourishment, laxatives, compound infusion of Gentian, Liq. Acet. Ammoniacæ, &c. &c. and latterly Sulphate of Quina. At the above date the report is as follows.]

16th.—The pain had continued all day, and then was relieved by an embrocation of spirit. camphoræ; two stools; tongue very red; pulse 100; look calm and cheerful; mind sustained by religion.

Jelly and beef-juice.

Shortly afterwards Mrs. G. was taken into the drawing-room, on the sofa, and whilst masticating some mutton-chop she felt the death-stroke. On being placed in bed two stools were passed involuntarily, and she became insensible and breathless. After some wine she revived, complained of pain at the stomach, and again sank. When seen by me at 2 P.M., the pulse had ceased, the respiration was slow, the jaw had dropped; in two hours more death was complete.

Inspection, 20 hours after death.—The body exhibited a sallow colour, and

extreme emaciation; the belly was so much sunken that the lumbar vertebræ touched the abdominal muscles anteriorly; over the epigastre, and below the right ribs as far as the navel, a circumscribed hardness was palpable, of an uniform surface. The peritoneum abdominis was natural, and held ℥iij. of clear citron-coloured serum. The liver was vastly enlarged, extending below the right crista ili, and filling more than half the abdomen: it was of a variegated, mottled, raspberry colour, neither hardened nor softened. In its interior, at the margin of the right lobe, were some scattered serofulous abscesses, or softened tubercles, of which the largest was the size of a pea. The posterior part of this lobe was firmly adherent to the ribs, its concave surface to the duodenum; in the whole of its thickness, from the convex surface to the gall-bladder, was a cavity (the exterior lateral boundary of which seemed to consist only of the thickened peritoneal tunica), holding lbj. of bilious ichor, with some coagula, none of which were organized: *no hydatids were seen*; a thick membrane formed of morbid albumen lined the cavity, being in most places of a darkish rugged aspect, as if from incipient ulceration, and in one place *so organized as to resemble the diseased coats of a large artery*, three inches long and two broad. The cavity communicated with the adherent duodenum by an ulcerous opening, six lines in diameter. The surrounding part of the liver was carnified, but not much indurated; its other portions were morbid only in colour. The vena portæ, &c. seemed healthy. The gall-bladder was small, and of an ochry colour. The stomach was small, very narrow, natural in colour, and texture; but its mucous membrane rugous from hypertrophy. The intestines were very small, from the contraction and tenuity of their coats. The ileum exhibited a diffuse vascularity; its mucous coat was intensely vascular, particularly at the prominent parts of the valvulæ conniventes, but without obvious ulceration. The spleen was very small, but healthy. The kidneys were healthy. The uterus was exteriorly, at its fundus, livid, very much indurated throughout; its mucous membrane dark, with some vestiges of an albuminous exudation.

COMMENTS.—This case seemed at first to be one of ordinary icterus, with in-

flammation in the digestive organs. The nature and extent of the organic affection could only be known from the progress of the symptoms. In its course no doubt was entertained of an hydatidical disorganization of the liver; but how the hydatids passed into the alimentary tube, seemed obscure.

The prognosis was, of course, that of ultimate death, as hydatids in the human subject are, I believe, always a token of a malignant disease, and as the powers of the constitution were here evidently much worn.

The icterus was partly from the pressure made on the gall-ducts by the distended sac in the liver, whereby the passage of the bile into the duodenum was impeded; and partly from the greater formation of this fluid in the more healthy parts of this enlarged viscus.

The nature of the paroxysms was that of *irritative, nervous*, and febrile action, which it is important to distinguish from a purely inflammatory state.

This case well exhibited the distinct nature of oppression from exhaustion of the vital functions. The disorder at the stomach was generally from an accumulation of hydatids in it, and the noxious influence of the attendant vitiated humours on its sensitive mucous membrane. The last exacerbation seemed to be induced by the over-stimulus of a more generous diet.

The state of the mouth and of the intestine was pathologically analogous; and the former was an important index of the latter. The colliquative diarrhœa which had begun was the proper effect of vascular ulcerative action in the mucous coat of the ileum—a truly asthenic cachectic disorder, the result of deficiency in vitality, as in the case of incipient decomposition of organized textures.

The condition of the uterus well explains the disordered state of its functions during life.

The blood was *not* inflammatory. That albuminous separation which is so often seen in chronic diseases, is characteristic of a specific cachectic action in the system, which is essentially different from the morbid actions of the blood-vessels in the genuine phlegmasiæ. May it not be regarded as a semi-vitalized state of this fluid, wherein a chemical decomposition of its constituent parts easily happens?

The remote cause of the malady in Mrs. G., was long indigestion. Bad pasture is remarked to be the constant cause of a similar disease (the rot) in sheep. I had expected that death would more gradually happen from apthous diarrhœa; this idea was erroneous. The sudden accession of rapid exhaustion is memorable. The vital powers often give way instantly, when the stimulus of disease is removed. This suggests an important caution regarding prognosis in chronic maladies.

The effect of the turpentine was salutary, as its use was followed by a more complete evacuation of the cavity; and when the discharge of the hydatids was determined through the intestines, the vomiting subsided. Acids, refrigerants, and sedatives, were ill suited to the peculiar form of vascular action which existed in this case. Cordials were equally inadmissible. In such a state it is important for the sick, and the physician, patiently to repose on the restorative powers of nature, and to reject the means of giving transient artificial strength at the hazard of serious consequences. Blood-letting was employed only as palliative; and by the abstraction of a small quantity, an instant change was induced in the nervous system. The effect was truly sedative.

The hydatids were entire, rotund, thick cysts, which became opaque on immersion in hot water, without visible blood-vessels, and separate; whereas, in the vesicular placenta I have seen them in clusters, connected by filaments. Can such substances be regarded as animalcules, capable of an independent existence? for such a capability is an essential property of animate bodies. They appear to be formed by a variety in that action of albuminous deposition, which, under another mode, generated the membrane lining the cavity.

The origin of such a disease appears to me to be this:—A morbid matter being secreted in the substance of an organ, becomes a specific stimulus to interstitial absorption; a cavity is thereby formed, which is lined by a membrane endowed with peculiar vital powers, whence an exudation takes place, which becomes variously organized according to the diverse modes of action in the containing part.

A singular mode of this degenerate organizing action was seen in the fibrous

part of the lining membrane, which in all respects resembled a blood-vessel. Inflammation is not the essence or proximate pathological cause of such organic vicia, but a frequent adjunct; whence a modification is requisite, both in the treatment which is proper for pure inflammation, and for the cachectic affection itself when not so complicated. A correspondent morbid action existed in the liver and in the uterus, as to the albuminous exudation; but a different action in respect of the induration in the latter, and the soft tuberculation and suppuration in the former. All sorts of disordered organic actions are found concurrent in cachectic diseases.

It is probable that there are two species of hydatids—the one malignant, invariably attended by a fatal disorganization of the parts in which they are situated, as in the present example; the other comparatively innocent, being simple vesicles embedded in an organ, the contiguous portions of which are little diseased. An instance of the latter I have seen in the kidneys.

TREATMENT OF CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

As a sequel to the observations of M. de Jonnès on the *pathology* of cholera, in a former number, I forward you the conclusions he has come to after an able and succinct sketch of its *treatment* in India, the western parts of Asia, and Russia; and I am, sir,

Your most obedient servant,
C. W. CROWDY.

Brixton-Hill, Dec. 6th.

1st. It is evident, from the foregoing details, that the treatment of cholera, during its disastrous career of fifteen years, has differed remarkably with place, seasons, the prejudices of people and casts, and the ideas either suggested to individual practitioners by the success or otherwise of their practice, or adopted *à priori*, and founded on erroneous systems.

2d. It has been every where acknowledged that the violence of the disease at its first invasion is often so great that

no medical aid can avail. However, in comparing the mortality in Bengal, of persons under medical treatment, with those who were deprived of it, we must admit that, if remedies are administered promptly and judiciously, death may frequently be arrested. But it is easy to conceive that such a circumstance is rare during the prevalence of a disorder which attacks a whole population, and deranges all the resources of a country; and it has been proved, too, that even this comparison, which is in favour of medical science, is limited to certain places, and that in others, those who have not had the benefit of our art, have not suffered more extensively than those for whom all its energies were employed. Spite of the able and courageous exertions of the English physicians, the disease had more victims in India than in Persia, where it was scarcely resisted; and in Russia, where a mode of treatment more rational than elsewhere, and having the advantages of experience, was pursued, the mortality amounted to one-half, or three-fifths, of the sick, being similar to what happened in those countries where they were abandoned to their fate.

3d. If the tables of mortality which I have given did not afford this melancholy result, a proof of it might be found in the number and diversity of the remedies which demonstrate their powerlessness. In a memoir, addressed to the Academy of Sciences, Dr. Janichen, who practised at Moscow during the prevalence of the disease in that city in 1830, does not scruple to avow that "one can place no reliance on the resources of art in this horrible malady."

4th. Without adopting an opinion so discouraging, we may observe that it may be doubted whether, hitherto, any rational treatment has been discovered or practised, when we hear remedies totally opposite eulogized and employed by physicians equally respectable.

5th. If I may be permitted to deduce from facts a conclusion which seems to me inevitable, but which in a matter of such importance should be expressed with hesitation and reserve, I should say that bleeding does not appear to be indicated by the nature of the disease. As is the case in yellow fever, if it lessens the violence of the symptoms, it does so by diminishing the resistance of the vital powers, and not by combating the principle of the malady. Its only

advantage is that of procuring for the unhappy patient a more speedy and less painful death.

6th. From the testimony of Dr. Walker, an English physician who was sent to Moscow by the British government, it appears that no benefit was derived from blood-letting in the cholera of Russia in 1830, but great influence was attributed to sudorifics, and for this purpose they applied immense cataplasms of heated hay-seeds to the chest and abdomen, or coverings of wool immersed in boiling water, and endeavoured to assist the operation of these remedies by administering brandy.

These remedies, which were subsequently recommended in Poland by the committee of health at Warsaw, had never been employed in Asia. They appear to be a new application of the popular theory of the countries of the north, which imagines that, as *cold* is the cause of a great part of their maladies, *heat* must be their cure, and that vapour-baths are a sort of universal panacea. However, experience is not in favour of these remedies, for in those countries where they have been tried, the deaths have amounted to three out of five of the sick, whilst in those where they are unknown the loss has not exceeded one-half.

7th. The prophylactic means employed during fifteen years as a preservative from cholera are purely empirical, since we are quite ignorant of its original causes. In the different countries of Asia there have been successively recommended baths, perfumes, the strongest aromatics, fires in public places, cleanliness, temperance, abstinence from certain articles of diet, amulets, prayers, talismans; in short, the usual resources in those calamities which excite fear or provoke credulity. But no kind of surveillance has been exercised as regards infected places, ships suspected to be so, merchandize, pilgrims, or troops coming from countries where the disease is raging. Throughout all the east, the clothes of the dead have been worn by those who inherited them; their houses left empty, have been soon inhabited by fresh families; their precious shawls and rich furs have been sold, and carried afar by the caravans. In short, everywhere has reigned, with the carelessness of an uncivilized people, that blind fatal-

ism which perpetuates the plague in the fair countries of the Levant.

8th. It appears, on the contrary, that in the towns of Russia, and especially in Moscow, prophylactic measures have been generally adopted; and although, frequently, those which were most highly estimated were found wanting, it cannot be said that they have been altogether unsuccessful; for it is certain that the propagation of the malady has been less extensive in Russia than in the other countries which it has traversed since 1817.

We must, however, confess that a hope, which we never ourselves entertained, but which, nevertheless, was very general, has been completely disappointed: we mean that which arose from the recent discovery of chlorine as a disinfectant. The Russian physician already cited, Dr. Janichen, informs us that the use of this preservative was common in all the towns attacked by cholera; but he adds, chlorine and the chlorurets have had no influence over the development of the disease, and it has sprung up amidst emanations of chlorine, which was in continual use with all classes of society.

9th. After the loss of this hope, we know of no other useful precautions than such as prevent persons exposed to the disease from being acted on by any causes which may interrupt the operations of the vital powers—such as fear, drunkenness, or any other excess. These precautions are the same as are adopted in places infected by any contagion, to prevent the absorption of its germ through the pulmonary or cutaneous channels. They ought necessarily to be as numerous as the chances which menace life, when any of its functions may become in a moment the cause of death.

10th. It is clear that a deplorable uncertainty must attach itself to these means of preservation; but there is one, of which the value is certain, and it is that pointed out by Franklin. "In all contagious diseases," said that wise man, "we must take it as a maxim, if we would avoid them, to go as soon, to go as far, and to be absent as long, as we can."

11th. Experience, purchased by the lives of many millions, has caused this maxim to be adopted by the inhabitants of Hindostan. As soon as the

cholera appears in a town, village, or single house, every one flies, abandoning his property and seeking refuge afar off, either in the fields and woods or on the mountains. This example was followed in the Russian provinces; and the emigration of the inhabitants of Moscow, Astrachan, and Teflis, so much diminished the population of those towns as to restrain the mortality of the disease. It is true, that in many instances it has favoured its propagation, the fugitives having carried with them, and disseminated, its germ.

12th. Another mode, more difficult, has nevertheless many times succeeded, viz. that of sequestration. The inhabitants of a house situated in the midst of an infected town, those of a capital in the centre of a country, ravaged by the cholera, have been saved from this pestilence by intercepting all communications from without. This plan was adopted with complete success in the isle of Bourbon in 1819; in 1820 in the Philippine islands; in 1823 at Aleppo and Antioch in Syria; in 1822 at Teheran in Persia; in 1823 at Astrachan on the Caspian; and in 1830 at Sarepta on the Volga. In all these instances, which we shall detail elsewhere, the cholera was arrested, as the plague has been, by cutting off, in time, all communication between the infected and those who had not yet received the germs of the disease.

13th. Facts have completely proved that, as the germ of the cholera does not float in the open air, as has been asserted, we may remain with safety in the midst of an infected town, and that there is no risk of being attacked by it so long as we rigidly isolate the house we may occupy.

The aggregate of these researches establishes, that it is with the cholera, as with the other severe contagions, as regards curative and prophylactic means: the remedies we can oppose to it to save the lives of those attacked, are inefficacious, or uncertain: our prophylactic resources afford but rare and doubtful chances of escape; but sanitary measures, whether their object be to arrest or prevent its invasion, to fly from it, or be protected by separation, are attended with the happiest results.

HOT AIR-BATH IN CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

THE approach of cholera has given to the hot air-bath a degree of interest and importance which it would not have otherwise possessed, since it seems calculated to become the most efficient agent in sustaining the heat of the body and restoring the circulation to the surface. It will be found, however, on trial, that the common hot air-bath is incapable of effecting these objects, for the ascending property of hot air causes the heat to be most intense at the upper part of the apparatus, while the under part and upper end are but slightly acted upon; and a difference of from thirty to fifty degrees will consequently be found to exist between the temperature near the feet and about the shoulders. Even if this defect be obviated, the whole under surface of the body can acquire no heat in the ordinary bath, as the patient must lie on his back under the frame work. From the valuable observations of Dr. Barry, at the Westminster Medical Society, on Saturday, the 10th inst., respecting the benefits which were derived from the application of the actual cautery on that part of the spine where ramollissement of the cord had been usually found, we may infer, that, if we do not actually have recourse to that remedy, it will at least be of the greatest importance that as much heat should be communicated to the spinal column as the patient can bear with impunity. Impressed with these opinions, I have caused to be constructed a hot air-bath, which, on trial, I have found to obviate these several objections, and which I flatter myself is not only calculated to be of more benefit in cholera than any other hitherto offered, but is capable of being made an efficient remedial agent in many other diseases. The hot air is communicated and diffused under the patient, and every part of the body is subjected to a uniform degree of heat of any required temperature. I beg to subjoin a description of my apparatus. It consists of three square frames, each larger by three inches at one end than the other, and which are made of three sizes, so that one fits into the other. When placed together they form a strong

frame-work five feet long, twenty-six inches wide at the larger, and seventeen inches wide at the smaller extremity. Each frame consists of two cross-pieces of deal eight inches deep, with two openings in each, and of two strong bars of beech, over which pieces of strong or double webbing are securely nailed, four inches apart. Three pieces of thin deal board, six inches wide, are fitted to lay under each frame, passing under the opening of the cross pieces, to preserve the bed-clothes from being injured by the tube, and to prevent the rising of the feathers. The other part of the apparatus consists of the spirit furnace, which is made of sheet iron, about ten inches high, and turns off at a right angle, to communicate with a flattened tube, four feet long and four inches wide, at the larger extremity, and which is perforated its whole length by two rows of holes about the size of a quill. The tube is made of three pieces, which fit into each other. The end of the smallest frame is made with an iron plate over the lower opening, seen in the sketch; from which a short tube projects, and to which the narrow end of the long tube is fixed on the inner side, and which receives the end of the furnace on the outer side. Four iron rods (covered with list) sloping inwards, are made to fit one into each side of the larger and smaller frames, with an eye at the top of each. Through these a cord is passed, which, when drawn tightly, forms a secure support for the blankets which are used to retain the heat. If used in a French bedstead, an additional tube, with an elbow-joint, is required. When this apparatus is to be used, the frames are to be placed together on a bed, or sofa, and if required longer are to be drawn a little apart. The other parts of the apparatus are then to be put together, which will not take more than a few minutes, and the patient, wrapped in a blanket, is to be placed on the webbing. It will be found that the heat will be diffused gradually and equally under the patient, as the thermometer will indicate nearly an equal degree of heat in every part. The heat may be diminished at any required part, by applying a small piece of deal over the perforations as a damper, and the end of the tube may be closed by a cloth, should the heat round that part of the body be too in-

tense. The sides under the bars being open, will admit of a bed-pan being placed under the patient when required.

The whole apparatus is very portable, being enclosed within the space of the larger frame.

I have left my apparatus in the hands of the manufacturer of the iron work, Mr. Crook, 5, Carnaby-Street, Golden-Square, for the inspection of any member of the profession who is desirous of examining it*.

JAMES RUSSELL, Surgeon.

8, Broad-Street, Golden-Square,
December 14, 1831.

CONTAGIOUSNESS OF CHOLERA.

To the Editor of the London Medical Gazette.

North Shields, Dec. 21st, 1831.

SIR,

It being still a matter of dispute as to the disease called spasmodic cholera being of a contagious nature, I think it is the duty of every medical man to collect such evidence as comes within the sphere of his own observation, so that by an accumulation of facts the question may be finally set at rest. Acting upon this principle, I now send you an account of all the cases which have hitherto occurred at North Shields, and the connexion that any of them may have had with suspicious sources.

Number 1, was a man residing in the eastern part of the town, a mendicant, also a dealer in old rags: he had been over to Sunderland for a few days, where he stayed at a common lodging-house; returned to this place on the 8th of December, bringing with him a considerable store of filthy rags. On the 10th he was attacked with spasmodic cholera, from the effects of which he is now recovering.

Number 2, a woman residing in a central part of the town, more than half a mile from number 1, was attacked also on the same day; could trace no communication with an infected source:

* As the description renders the construction of the apparatus sufficiently intelligible, we have not thought it necessary to make use of the drawing which accompanied it. We ought perhaps to add, that the above letter was received before the publication of Mr. Green's in our last. We mention this because there is some resemblance in the baths.—ED. GAZ.

this woman died in fifteen hours after the attack.

Number 3.—This was a woman, the wife of number 1: she was taken ill on the 13th, and died on the 14th, after an illness of twenty-three hours. No other case presented itself until the 18th.

Number 4.—This was also a woman who resided near the western extremity of the town, about three quarters of a mile distant from number 2. She had been at Newcastle, returned home in a common passage-boat, took ill early the next morning, and died in about thirty-four hours.

Number 5, also a woman residing in the same district as number 4: she had visited a sister labouring under spasmodic cholera, at a village a few miles distant on the 18th. She took ill of the disease on the 19th, and died on the 20th.

Number 6, also a woman residing in the vicinity of the two last-mentioned, took ill on the 19th, and died on the 20th: could trace no communication with an infected source.

Number 7, also a woman who was employed as a nurse to attend upon number 1, and his wife number 3. She enjoyed good health up to the evening of the 20th, when, having occasion to go out for a short time, on her return she found her charge, number 1, had crept out of bed during her absence, and was busily employed in sorting a quantity of rags which he had contrived to secrete somewhere from the vigilance of the overseers; she complained of the offensive smell arising from the rags; she sickened in half an hour, and died of the disease within nine hours.

Number 8, a seaman on board of ship. He left his family on the 19th, who reside in a part of Newcastle where the disease is prevailing; took ill on the 21st, and died in a few hours. Some other cases have also been reported to-day, but I am not yet in possession of the particulars. I make no comment upon these cases, but leave facts to speak for themselves. Should you think this sort of journal of sufficient interest to occupy a place in your Gazette, I shall have much pleasure in sending you a continuation of it.

I remain, sir,

Your obedient servant,

EDWARD GREENHOW,

Vice-President to the Board of Health at North Shields and Tynemouth.

CURIOUS CASE SIMULATING CHOLERA.

BY DR. ALDERSON.

[THE following is an extract of a letter from Dr. Alderson, the official correspondent of the Central Board of Health, addressed to Dr. Barry. We think great credit is due to Dr. Alderson, and the gentleman who assisted him, as well for the zeal they displayed as for their acuteness in detecting so unexpected a cause of the symptoms.]

“About three weeks ago, a case of suspected cholera was reported to me by a magistrate, about sixteen miles from Hull, on the road to the coast, and without loss of time, before daylight even, and in company with two surgeons, I repaired to the village (Gonhill, near Hornsea,) and minutely inspected the case. A farmer, Peter Cross, aged 64, had been seized with symptoms of cholera—vomiting and purging, with spasms in the legs, at half-past nine in the evening, and died at seven the following morning, (nine hours and a half only.) Every part of the body was minutely inspected, and a rusted needle was found in the pancreas, projecting half an inch into the duodenum! The stomach was like that of a person having taken arsenic, and the inflammatory appearance of the mucous membrane ceased about two inches below, beyond the situation of the needle. The needle was about an inch and a quarter long, and had the appearance of having been in the body some time, for that part of it which projected into the intestine was thinner than the rest, so much so as to shew quite a shoulder at the membrane: by some accidental cause, its position may have been altered. We had no hesitation, of course, with such a local cause of inflammation, in pronouncing this as the real cause of the death of the man. No other case has occurred in this neighbourhood; nor have we had any cases of the cholera of this country since the time Dr. Dunn was here, when bilious cholera was rife. Measles are very prevalent and very fatal, as well as inflammatory fever, which, in the timid bleeder, who sees in fever nothing but *typhus*, soon takes on a typhoid form. I need scarcely add, that you may confidently rely upon the very first intelligence of any

real case of cholera that may occur in the town or neighbourhood. I should say with respect to the health of the town, that the deaths, in the last three weeks, have been very much above the average number."

EXTRACTS FROM REPORTS ON THE CHOLERA AT DANZIG,

By JOHN HAMETT, M.D.

On the 30th of May cholera appeared not only in the town, but also in the suburb Schlapke belonging to Schidlitz: thus, at the same time, in two quite different parts of Danzig. It attacked several individuals in Eimermacherhoff, Rambau, and Seigen, three streets adjacent to each other, not far from the principal ramparts of the fortress, and Mottlau guard-house, which is situated on the confluence of the Mottlau and Radaune streams, into which all the dirt and filth of the low and old town in particular are conveyed. The Mottlau, which is the larger stream, is lost in the Vistula at about 2000 paces from its place of junction with the Radaune. The above-mentioned streets are in the lowest part of the city, and were in March*, 1829, quite inundated, the water rising, in several houses, from five to six feet, and in some even to the ceilings on the first floor. The ground there is rather marshy, and intersected by drains for carrying off the dirt and filth. When the Vistula is higher than ordinary, parts of these streets are generally more or less inundated.

The suburb Schidlitz, distant about a quarter of a German mile from the city, is in a dry and more healthy situation; the houses are more airy, and less contiguous than those of the above named streets, and the manner of subsistence and occupations of the inhabitants of a totally different nature:—those of the above-mentioned part of the city gaining their livelihood chiefly by such occupations as relate to shipping, while those of the suburb of Schidlitz and Schlapke are chiefly agricultural or mercantile labourers, and subsist on the produce of their gardens. Here only one woman fell sick at an early period, and subsequently but few cases appeared.

It not only appeared, as above stated, in the old town, but continued to spread, without any marked order from personal contact or proximity, in low, damp, and

dirty, or close situations, all over the city, among the destitute and poor, who are here, in general, ill-clothed, ill-fed, uncleanly in their persons and dwellings; never wearing flannel next the skin; subsisting chiefly on indigestible and unwholesome food, and in the habit of using pernicious drinks:—habitual drunkards of whatever class have been invariably the victims of this malady. Besides these, it occasionally selected for its objects, in comparatively healthy spots, persons of particular constitutions and habits, in easy circumstances of life, who happened to suffer in their health, in the various ordinary ways; from exposure to cold damp air, especially at night; from profuse perspiration suddenly suppressed, the bad effects of conjoined wet and cold, &c. &c., and, above all, from recent derangement of the stomach and bowels. The higher and middle classes escaped the disease, a few excepted, who brought it on by a want of common care of themselves, or who became incidentally predisposed to it, as shewn in the Police Reports, and the interesting communications to me from Mr. Gibsone, the British Consul in Danzig; they not having been near, or at least ascertained to have been near, infected persons.

In proof of these statements, between the 28th of May and the 23d of July inclusive, 835 persons, consisting almost entirely of the wretched and unhealthy poor, were attacked with cholera, of whom only 195 recovered, according to the general list, making the amount of deaths during that period 640. During this period of eight weeks, notwithstanding that 2000 inhabitants of the dwellings of these sick were shut up for twenty-one days, subject to the bad effects of fear, want of exercise, and fresh air—though indeed having the essential advantage of being well fed—only 188 persons fell sick in 108 houses, in each of which, at the same, or at different periods, there was more than one patient*; and in these 183 patients, a probable or predisposing cause to the disease has been officially reported in the separate general list.

In a population of between 70,000 and 72,000 in Danzig and its immediate district, the whole mortality from cholera was officially reported to have been 1028 of both sexes and all ages, coinciding in all particulars of the disease with the above-mentioned

* As in the low parts, particularly of the old town, of Edinburgh, so in the low and the old town of Danzig, the houses are composed of stories, or flats, in each of which one or more families reside. But, unlike the old town of Edinburgh, there is in general here a large privy in each house, which is seldom emptied until it is full, or nearly full. The sewers are, for the most part, made of wood, not close, and prove bad conductors of the dirty fluids thrown into them. Hence the offensive effluvia occasionally so common here at those changes of the weather when the mercury rises in the barometer.

* An inundation unexampled in the memory of man took place in this month and year, and laid twenty German square miles of the immense plain, or valley of the Vistula, as it is called, under water.

640, between the 30th of May and 8th of September. The numbers actually taken ill of the disease I have been unable to ascertain. The physicians in Danzig, as I repeatedly stated in my reports home, did not report all affected with cholera.

I now revert to the first cases that appeared in the old town, as before stated, which are, according to the official reports:—

1. The recruit of militia, Andreas Hermann, was taken ill in the night of the 30th of May, at his lodgings in the house of the nail-maker Wruck, in Eimermacherhoff, No. 1723. He had been the day before with his comrade, Koeker, to fetch his military effects from the store of his battalion; he had taken for supper potatoes and ham, and drank ale. On going to bed, he complained of coldness in his limbs, and requested a feather-bed for a covering, which was given him. At midnight he woke his comrade, complaining of pain in the bowels; when he was covered with blankets, and took chamomile tea, and peppermint; but vomiting, diarrhœa, and cramps, came on; and when Koeker left his quarters at five in the morning, the man was speechless, and only by slight convulsive motions gave signs of life. The commander of the battalion, Major Schwikowski, and the police officer of the district, were made acquainted with Hermann's illness; and at ten o'clock he was conveyed to the Military Hospital, where he died a few moments after. He had only been enlisted three days; had served some months with the constable, Hein, at Stublau in the Danzig Werder; had had continual intercourse with the other servants and inhabitants of the village, but none had fallen sick, neither had his comrade or any of his landlord's family.

2. Friedrich Kuntz, private of the 5th regiment of infantry, lodging in the Seigen, No. 1186, fell sick on the morning of the 30th of May, while at drill. On the day previous he had eaten grey peas; was in the night affected with a slight diarrhœa, but in the morning felt sufficiently well to attend drill; he, however, could not keep to his post from illness, and was immediately taken to the hospital, where he died of cholera four hours after his admittance. It appears he was addicted to drinking; but this was not ascertained to have been the case the day previous to his illness. No immediate cause for his illness was then assigned.

3. John Muller, labourer, living in Eimermacherhoff, Gelbe Reihe (Yellow Row), No. 1751, at two o'clock in the morning of the 30th of May, was taken ill with headache, and coldness of the body. His wife, thinking his indisposition slight, left him to fetch milk from Neuendorf, a village in the Werder district, three-quarters of a German mile from the town. On her return at half-past seven, she found him dangerously ill, and speechless, affected also with cramps

and diarrhœa. Dr. Klinnsmann visited him at half-past eight o'clock, and he died in an hour afterwards. The preceding day the deceased had eaten pork and potatoes. He had been a day-labourer, but had then no other occupation than that of fetching milk every morning from Neuendorf. Contagion could not be discovered.

4. Jacob Koss, lighterman and day-labourer, lodging Eimermacherhoff, No. 1749, was indisposed since the 27th of May, complaining of pains in his breast, but continued to work till the 29th. On the 30th he had repeated vomiting, diarrhœa, and continual faintings; Dr. Klinnsmann visited him, and prescribed medicines at eleven o'clock in the forenoon; but he died at seven in the evening, under, as Dr. Mathy says in his report of the 31st of May, but slight symptoms of cholera. The day previous to his death, he had eaten roasted bacon and potatoes. He had latterly no employment. During the last year he suffered from inflammation of the lungs. No contagion could be discovered.

5. On the 30th of May, Aron Hirsch Simonsolm, a Jew, formerly clerk to commercial establishments, living in Rambau, No. 832, went abroad as usual at nine o'clock in the morning, and at half-past six in the evening was conveyed to his house sick, by two unknown Jews; he had diarrhœa, cramps, and was speechless, and died at half-past three o'clock the next morning. He had no medical assistance. His landlord stated that he was inclined to drinking; that he maintained himself by assisting Jewish tradesmen; and consequently he could not tell with what people he had intercourse.

6. On the 30th of May, Louisa Zielke, residing in Seigen, No. 839, unmarried, returned to her home at ten o'clock in the evening, from her customary employment of shovelling grain, on the banks of the Vistula, in good health; at eleven she complained of cramps, sickness, and diarrhœa. Dr. Klinnsmann visited her at nine o'clock the next morning, and prescribed accordingly; she died an hour and a half after. No cause for her illness could be found out, as the patient was stated to have lived a regular life.

7. On the 30th of May, Adelgunda Shroeder, a married woman, residing at Schlapke, No. 965, was taken ill at ten in the morning, with pain in the bowels, while washing linen at the saddler Mischke's, at Schilditz; she returned home, went to bed, and had recourse to warm applications. Vomiting and diarrhœa soon came on, which continued all that day and the subsequent night. Dr. Lenz, and the surgeon of the district, Leuc, visited her on the 31st, and declared her disease to be cholera. The prescribed regulations were put in force, and the patient recovered. No positive cause could be conjectured for her falling sick. She

had little intercourse with other people; and during the days previous to her illness, had almost always been at home. Her chief diet was potatoes, boiled in water, with salt, and her drink succory water. On the morning of her falling sick she had taken no other nourishment than some dishes of coffee with milk at Mischke's. Her husband, and four children, of eleven, nine, seven, and five years of age, respectively, continued during her illness in their wretched abode; and, although they all slept in the same room during the time they were isolated, which was forty-three days, yet no one fell sick.

8. Martin Kluth, sawyer in a timber-yard, in Seigen, No. 1175, at seven in the morning of the 28th of May, went to his work already indisposed. Sickness of stomach, with headache, and cramps in the calves, came on at three o'clock p.m. He took some brandy, and returned home, when vomiting and diarrhoea succeeded. In the evening he became speechless, and the police commissary of the district caused him to be conveyed to the Town Hospital. He recovered on the 15th of June, and was discharged from the Contumace Establishment on the 21st of July. The physician of the hospital, Dr. Baum, affirms, that the sickness of Kluth was at that time only looked on as sporadic cholera, and it was not until a later period that he was convinced of his having really suffered from Asiatic cholera, consequently he put him in his list of the 3d of June. Associating only with his fellow sawyers, and, with the exception of his hours of labour, not having been from home, contagion cannot in his case be well traced.

Several other cases, similar to cholera, occurred previous to the above dates, but they were not officially reported, and were only spoken of after the continued appearance of the disease. The following are some of these cases:—

A. It is mentioned in the hospital report of the city, dated 30th of June, that Gottlieb Friedrich Kluge, journeyman to an amber-turner, lodging in the Kunst Gasse, was brought into the hospital on the 21st of May, affected with cholera, and that he died on the 2d of June. It is stated that he had extravasation in the brain after the symptoms of cholera had ceased, which led to the supposition that he had died of this disease; but Dr. Baum, on being desired to give the requisite report, declared it to be only a case of sporadic, and not of Asiatic cholera. This patient had one side affected with palsy.

B. In the same information given by the nail-maker Wruck, on the 30th of May, of the suspicious sickness of Hermann, the recruit of militia, he also mentions that a woman had previously died under the like symptoms, and that one of his neighbours was in like manner ill. On inquiry, it was found that Florentine Muller, a married wo-

man, residing in Eimermacherhoff, No. 1720, had been attacked at eight o'clock on the morning of the 27th of May with cramps in her feet and other bad symptoms, and died at half-past ten the following day. She was attended by Dr. Klinmann. The husband of the deceased deposed, that she had suffered at times from convulsions, was careless of her health, and went lightly clothed. Half an hour before her death she received the sacrament with great devotion and good understanding from the hands of the Dominican monk, Joseph. Dr. Klinmann and Dr. Mathy inspected the corpse, and gave permission for ordinary burial, not then supposing the disease which caused her death of a suspicious nature.

C. Solomon Muller, glazier, lodging in Hacker Gasse, No. 1474, was taken ill on the 28th of May in the evening. Dr. Jaeger visited him at ten o'clock, found him suffering from diarrhoea and repeated vomiting, and prescribed the necessary remedies; but he died at four the next morning. Dr. Jaeger communicated with Dr. Mathy on the 29th respecting this critical case of sickness and death: the corpse was examined; and although it was not admitted at the time that the man died of cholera, yet Dr. Mathy ordered the necessary fumigations and purifications, and caused the body to be interred with the usual precautions. On further inquiries, however, afterwards, it appeared that the man's symptoms were truly choleraic. The deceased was much addicted to drinking ardent spirits. On the morning of the day he was attacked he had been exceedingly vexed at the unexpected loss of some money. He had satiated himself on a sturgeon, then drank brandy, and afterwards, to cool himself, buttermilk; consequently vomiting and diarrhoea, as might be expected, first took place.

D. Johann David Hammermeister, labourer, formerly mariner (the neighbour mentioned by the nail-maker Wruck), lodging in Eimermacherhoff, or rather Grosse Gasse, No. 1733, felt himself indisposed on the 29th of May; on the subsequent night had repeated vomiting and spasmodic movements, and died on the 30th, in the forenoon. The corpse was examined by Dr. Mathy, but no suspicion was then entertained that the man died of cholera.

Besides these, there are other isolated cases of cholera furnished me by Dr. Baum, Physician-General of the Town Hospital in Danzig, which had occurred at an earlier period than the first appearance of the epidemic, as stated, and which are detailed in my medical report, A. q. v.

The foregoing contains most accurately the events relating to the first appearance and subsequent spread of cholera in Danzig; and with the general and separate lists and other authentic documents, which I trans-

mitted home, will be found to contain, I imagine, all the information possible to be obtained on the subject of the epidemic in this quarter.

JOHN HANETT, M.D.

ANALYSES & NOTICES OF BOOKS.

L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

The Morbid Anatomy of the Human Uterus and its Appendages; with Illustrations of the most frequent and important Organic Diseases to which these Viscera are subject. By ROBERT HOOPER, M.D. &c. &c. Price 3l. 3s.

So long a period had elapsed since the appearance of Dr. Hooper's splendid work on the Brain, that we began to fear it was destined to be the first and last of his promised series of plates; it is therefore with the satisfaction which results from the fulfilment of hope long deferred that we at length find the present able contribution to science on our table.

The plates are lithographed from drawings chiefly by Mr. Howship, and many of them are in the best style of the art; but they are unequal in point of execution, and some are decidedly very inferior, to the others. The first plate, containing two figures, represents, first, the uterus and its appendages in a healthy state, taken from a virgin in her twenty-fifth year; and, secondly, the same parts in a female killed by an accident during menstruation. The other plates are devoted to the organic diseases of the parts, and they are thus arranged:—A, Inflammation and its consequences; B, Tumors; C, Diseased structures, and unnatural appearances, without tumefaction; D, Ulcers; E, Polypus.

To the morbid anatomist generally, but to the practitioner and teacher of midwifery more particularly, this volume will prove a most valuable acquisition.

Reports of Medical Cases. By DR. BRIGHT. Price 9l. 9s.

[Continued from page 313.]

BEFORE we proceed with the regular prosecution of our analysis, it will

be necessary to set right a typographical mistake, which occurred just about where we left off (see p. 312, ad fin.): the postmortem examination, in the very important case of A. Z., was printed incompletely; we now beg leave to give it in its more perfect form:—

" *Sectio Cadaveris.*—On removing the skull, the vessels of the dura mater were rather turgid; and raising the membrane it was found to adhere to the parts below on the exterior portion of the right hemisphere of the cerebrum, more particularly about an inch from the front near the longitudinal sinus, where the adhesion was perfect, with an appearance of vessels radiating over the hemisphere. The surface of what appeared to be brain at that part, was obviously changed, looking opaque; and on being felt gave great resistance, and was perfectly firm and hard: it proved on further examination that this was the surface of a circumscribed tumor, which extended backwards nearly four inches, and sunk into the hemisphere, so as to reach nearly to the ventricle, pressing the anterior part of the corpus striatum before it. The tumor was lobulated on its lower or internal surface, and was covered with vessels; and there was a small space containing a serous fluid beneath it, forming an insulating bed, by which it was separated from the thin layer of brain interposed between it and the ventricle. The tumor being cut into was one uniform substance, like a newly-made cheese or a mass of curd, but affording much resistance, and shewing some marks of a lobulated structure; it weighed nine ounces. The other viscera of the body were perfectly healthy."

This renders the history of the case complete, and we think it as interesting a detail as we have ever met with, of the effects of chronic disease producing gradual pressure and frequent irritation on the brain. For many months, it appears that the symptoms were regarded as rather depending upon functional than upon organic disease. The growth of the tumor was so gradual that it produced no sudden shock, no apoplectic stroke, no decided paralytic seizure: it scarcely affected the substance of the brain, except as an external source of pressure, till towards the conclusion of the disease, when possibly the surrounding portions of the brain became slightly disorganized, and more urgent symptoms, amongst which were epileptic seizures, occurred. The tumor, it may be added, was a growth from the dura mater, or rather perhaps from the

arachnoid lining the dura mater—of a fungoid character, slow in its progress, and lobulated in its structure; and it would appear that the great aggravation of symptoms leading to the fatal termination, was connected with a softening and a watery infiltration of the brain, in the neighbourhood of the tumor.

PRESSURE OF THE BRAIN resumed.

We were obliged to break off, in the last portion of our analysis, with the death of Amelia Humphreys, who sunk under symptoms of paralysis and great irritation. The following account of the examination of her body seems too important to be omitted:—

“On removing the calvaria, nothing remarkable could be seen in the dura mater; but when this was raised, on the left side just beneath the arachnoid were observed several irregular-shaped yellow spots, generally following in some degree the sulci of the convolutions, and not elevated above the surface; on touching them it was evident that they were hard and unyielding; some were glued and fastened to the thin membranes, and others were not only fixed to them, but were so firmly attached to the dura mater that they partly tore away, if that membrane was forcibly raised; incisions made into them proved that they frequently extended to the depth of about half an inch, varying however in this respect; they were of a light yellow colour throughout, and of a curdy or cheesy consistence, softening towards their centres. On raising the right side of the dura mater, precisely the same appearance was seen. There was no unusual effusion of fluid under the arachnoid. A section being made just on a level with the top of the ventricles, the chief seat of these tubercles, besides the superior part of the hemispheres, was seen to be the posterior and internal part of the brain, where the hemispheres join, and in no case did they seem to be completely imbedded in the brain so as to be detached from the cineritious matter; in one instance only, at the posterior part of the ventricle was a small tubercle observed, which apparently was imbedded in the medullary matter; but on more strict examination this was found to arise from the posterior part of the corpus striatum. The ventricles, so far from being distended with fluid, contained less than natural. Small tubercles of a more distinctly circular form, and of the size of small beans, occurred in several parts of the base of the lobes of the cerebrum, always, however, situated in the cineritious substance. The

cerebellum on its superior surface also presented these tubercles; and when the left lobe was divided, a considerable body of the same kind was found in the corpus rhomboideum; one small tubercle was also found in the cineritious portion of the medulla oblongata; a considerable portion of the medullary structure in the middle lobe of the left side was softer than the rest, but not obviously disorganized.

“The most minute examination of the spinal cord led to the discovery of no tubercular disease, though the whole was rather soft; and from the way in which it projected wherever an accidental wound was made through the arachnoid, we suspected that membrane to bind it rather more closely than natural; the membrane was not, however, opaque or thickened.

“The left lung was free from adhesions, but internally it was almost filled with minute miliary tubercles, and the intervening portions of the lung were red. The right lung was less filled with tubercles, but contained many, and it was unusually adherent to the pleura costalis. Heart healthy, except in having a white patch upon its surface. The liver of a light drab colour. The omentum adhered firmly to the parietes of the abdomen, and was studded with tubercles; the whole peritoneum of the intestines was in a similar state, studded with small miliary tubercles sprinkled in all parts, either single or in clusters; these were most numerous about the mesentery. The spleen contained some small tubercles, as did the kidneys. The uterus was small and healthy, but the fallopian tubes were tortuous and hard, of the size of a small quill, and filled with a white cheesy matter; their orifices were open, but at a very short distance down, the tube was quite blocked up by its morbid contents; their fibrinated extremities were widely expanded, stiff, and thickened, owing to which the structure was rendered very obvious.”

The tendency to the formation of tubercles which exists in scrofulous constitutions is strikingly exemplified in this case. The same tendency which, in some instances, produces tubercles in the lungs, mesentery, and peritoneum, giving rise to phthisis, marasmus, and peritoneal disorders, here destroyed life by the formation of tumors in the brain. “This disease (Dr. Bright remarks) appears to affect in a peculiar way the serous membranes, and, not unfrequently, is attended with a disposition to an increased accumulation of fluid in the cavities, shewing itself by effusion into the abdomen, or producing hydrocephalus; and it would appear that it is not necessary for tubercles to

exist in the brain in order to excite the hydrocephalic action, nor is the effusion of fluid in the brain at all a necessary result of the existence of such tubercles. Thus in the present case, though the tubercles had proceeded to a great extent, and induced the most distressing effects, no effusion had taken place; and in a case which I shall immediately relate, the effusion had taken place in a constitution exactly analogous, as proved by the other morbid appearances, without any tubercular formation in the brain; while in a third, to which I shall also refer, both the tubercles existed to a small extent in the membranes of the brain, and hydrocephalus had taken place; from which different facts, it would seem that the hydrocephalus is rather the result of the constitutional tendency than of the tubercles, though these morbid deposits are probably often the exciting cause. In the family of Amelia Humphreys, two children had already died of hydrocephalus. "The case now before us is that to which I have referred in a former page, as tending to shew the probable connexion between injury of the cortical portion of the brain and convulsions. "The peculiar appearance of the fallopian tubes corresponds precisely with that described by Dr. Abercrombie, at page 170 of his valuable work on the Brain, where, indeed, several cases analogous to the present will be found. And the state of constitution on which these diseases appear to depend, is precisely that to which the eminent pathologist, Dr. Farre, has referred in his "Apology for British Anatomy."

From the consideration of pressure, purely from tumors, we are led to the subject of pressure resulting from changes in the general structure of the brain; and the transition is well managed by the detail of a few cases in which the existence of fluid, along with tubercular disease, was detected.

The first of these is that of a boy of seven, whose head was large and full towards the occiput; his chest ill expanded; his frame, formerly stout, now fallen off; and his temper become unnaturally irritable. Five or six weeks before his death, his medical attendant (Dr. Hodgkin) thought it advisable to order him to the sea-side, and cautioned his parents against over-exercising his

mind; though there were then no decided symptoms of head affection. In the course of a fortnight the boy improved vastly, but two days before he left the sea he was not so well; he was attacked with sickness at the stomach, and retched severely. Coming to town in the steam boat, he was observed to be remarkably nervous; upon entering the hustle of the streets, he trembled and shook with alarm, and when he got into the coach, held firmly, as if he feared some injury. His good looks now left him; he complained of constant pain in the fore-part of his head; he disliked the light; got drowsy; had strabismus of the right eye; was sometimes incoherent, though occasionally sensible, as he was within an hour or two of his death, when he roused up and made some coherent observations.

Upon examination after death, the skull was found to be thin, and easily separable from the dura mater; but the sutures gave way a little while the calvarium was being raised. The dura mater was full and tense, and the arachnoid slightly unctuous to the touch. The large veins going to the longitudinal sinus were distended. The convolutions of the brain were all flattened; and between them, in the sulci, was seen a small collection of serum, of a dull yellowish colour, slightly opaque, or turbid; apparently the result of inflammation. When a cut was made, of the usual depth, to remove the top of the hemisphere, the lateral ventricle was cut into, and a quantity of clear fluid escaped. But the ventricles were greatly distended with this fluid, which could not have been less than six or eight ounces. Spots of ecchymosis were discovered on the roof of the left ventricle, and presented a very curious appearance. The brain, at the posterior part of the same ventricle, was completely softened down, but not discoloured in its substance, though pervaded with several of the ecchymosed spots. At the base of the brain there was also some fluid, and near the optic nerves a little of the yellow lymph-like serum was seen. The bulbs of the olfactory nerves tore away, so as to be left in their places on the ethmoid bones. The pleura of the left lung was studded with miliary tubercles; and the whole omentum was thickly covered with small semi-transparent tubercles, not larger than small seeds, and it was glued down

in the right iliac fossa. The same appearance was seen over all the peritoneum covering the parietes, and the intestines, and the mesentery, on which part it was still more advanced, and some of the mesenteric glands were enlarged.

Tubercles were found in the brain, as well as in various parts of the body of a girl of twelve years of age, who died of hydrocephalus. "Beneath the arachnoid, apparently adhering to it, and pressing upon the brain, were found in different situations three or four round tubercles, of the size of small marbles, of a yellow colour, and soft, like cheese: one of these had softened down in the centre, so as to form a cavity. The arachnoid of the hemispheres appeared thickened and opaque; there was serous effusion beneath it, and in the ventricles about six ounces of fluid. The membranes of the chest and abdomen were covered by innumerable hard tubercles, varying in size, from that of a millet-seed to that of a split horse bean. Both pleuræ—the pericardium, the diaphragm, the liver, and, in short, the whole of the peritoneum, were covered with thin tubercles."

Pressure from changes in the general substance of the Brain.

The changes here referred to may be comprehended under—1. An increase of volume, without any remarkable alteration in the texture and consistence of the brain—a state of disease which is sometimes attended with acute symptoms, resembling those of hydrocephalus. 2. An unusual degree of firmness, which yet has not been attended with decided paralytic symptoms. Dr. Bright recollects a particular instance of this in a woman who had long been an incurable lunatic. 3. A flaccid condition of the brain, when it has lost its firmness, but retains a certain tenacity, and this seems chiefly to occur in broken constitutions after long illness. 4. A soft and watery condition, attendant upon diseases of emaciation, with general loss of strength, but without paralysis. 5. A decrease of volume, attended with hardness and an appearance of contraction. 6. Softness of the cineritious substance, with contraction and hardness of the medullary. But, as Dr. Bright observes, opposite as most of these conditions undoubtedly are to each other, it is highly probable

they all produce their effects on one common principle—that of interrupted circulation. Cases illustrative of the two extreme conditions of alteration are then laid before the reader; but we must hasten on to the no less interesting head of—

Paralysis from Inflammation and Morbid Action in the Membranes of the Brain and Nerves.

Cases of this kind are often preceded by rheumatic pains, and sometimes by distinct attacks of rheumatic gout; sometimes by well-marked neuralgic affections; and they are frequently to be traced to exposure to cold and wet. In some of them, examination after death gives satisfactory evidence of the existence of such results of inflammation as are well calculated to produce pressure, and at the same time proves the nature of the action which has been going on; but in other cases the appearances are scarcely sufficient to account for the symptoms. Still it is right to take into consideration, as Dr. Bright well observes, the peculiar structure of the mass of nervous matter which composes the brain, and the numerous and complicated bundles of fibres of which the spinal cord and nerves are formed, and the extent of the fine cellular membrane connecting the nervous fibres. It is by taking this view alone that we can easily understand how, in many cases, all the effects of pressure, or at least of interrupted circulation, should be induced, without the brain or nerves presenting any very great or obvious appearance of disorganization. From among a number of cases which then follow, illustrative of these views—and which, having for the most part terminated fatally, and been traced up in the post-mortem examinations with singular minuteness of observation, afford the fullest evidence of their correctness—we select the following short history of an example of general paralysis with vertigo. The remark appended to it by Dr. Bright is highly creditable to his sagacity and judgment:—

"Joseph Lasky, aged 45, a German, of middle stature, with a round bald head, was admitted May 15th, 1829. As much as five or six years ago, he occasionally suffered from giddiness, but he never experienced any thing like a fit. About a year subsequently he had a sensation, which he describes as cramp in his legs, extending

MEDICAL GAZETTE.

Saturday, December 31, 1831.

“*Licet omnibus, licet etiam mihi, dignitatem Artis Medicæ tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso.*”—CICERO.

MEDICAL EVIDENCE REGARDING THE DEATH OF THE ITALIAN BOY.

So long as the excitement of the public mind caused by the murder of the Italian boy remained in full force, we were unwilling to discuss any of the questions connected with it; but now that these fearful events have become matter of history, we are desirous, ere the particulars be yet forgotten, to make a few remarks on the medical evidence. We do this with no desire to derogate from the general attainments of those who were consulted on this occasion, but because we think their testimony displayed an extraordinary ignorance on the particular points regarding which their opinions were received by the court, while those opinions were given with a degree of confidence which, if it shewed them to be sincere, shewed also that their knowledge of the subject was not even sufficient to make them aware of its difficulty. And this leads us to inquire, why, in all cases of such painful interest to the public, and involving the lives of individuals whose guilt no one had a right to assume till it were proved—the legal authorities do not direct competent persons to be called?—not merely gentlemen who may have been accidentally present, or who may have contrived to be summoned, in the puerile hope that the appearance of their names in the newspapers may obtain for them a little temporary notoriety; but persons who, from necessity or choice, are known to have directed their attention to the difficult and still imperfect subject of medical jurisprudence? Why, occurring as

afterwards to his thighs and to his body generally; and soon after, while he was one day eating his dinner, he became suddenly very hot, and covered with a rash, which subsided in an hour or two, but left him numb all over; and thus he has remained ever since, the face being the only part which at all retains its natural sensibility, and there it is not complete: he says that he does not feel the crutches within his hands, nor pinches upon them, nor even the incisions made in cupping upon the neck: he has also gradually become weak in his limbs, and now drags his legs as he moves forwards by crutches. He has a constant noise in the head like the rushing of water, and is frequently giddy, becoming dizzy and nearly blind two or three times a-day; he can scarcely ever see in a bright light. His hearing is rather affected, particularly on the right side, Pulse 96; bowels generally open. From his admission, on the 13th of May, to the 1st of August, no improvement had taken place, though great attention had been paid to his bowels; he had lost ten ounces of blood, on five different occasions, from his neck, and had had blisters applied both there and to the loins.

August 4th.—He began to take a grain of the nux vomica three times a-day, and from that time his giddiness almost immediately left him. The nux vomica was afterwards further increased to three, four, and five grains; the giddiness never returned, but he left the hospital very little relieved in his general paralytic symptoms.”

“When I first saw this man (says Dr. Bright) which was in July, I was led, from the peculiarity of his symptoms, to inquire whether he had been at all employed in the use of quicksilver, and he said that it was now many years since he was, for a year, occupied in silvering looking-glasses: that he found the business did not agree with him, and left it, but at that time experienced no symptoms of paralysis. How far it is possible that his former occupation might have had an influence on his general health, and paved the way for the peculiar paralytic affection, must be matter of conjecture, but it is not probable. I consider the morbid condition to be of the nature of congestion, possibly going on to effusion, and the nux vomica probably produced its good effect by stimulating the cerebral circulation; nor is this by any means the only instance in which I have seen the good effects of nux vomica, or of strychnia, in relieving vertigo.”

this event did, at an institution, having a teacher whose province it professedly is to investigate such points of forensic medicine, why was *he* not required to assist at the examination, and to aid the cause of justice with the result of his investigations? In Edinburgh, when Burke was arraigned, he whose name, by being given to the newly-invented crime, has been rendered infamous in all perpetuity, the legal authorities entrusted the examination of the subject to persons known to have given their minds to such inquiries, and who were thus qualified to judge correctly, as, from their situations and characters, their statements were calculated to have weight with the public. How different the system pursued here! Our readers can scarcely have forgotten the absurd and revolting display made on the occasion of St. John Long's trial for the slaughter of Miss Cashin—how the Coroner's court, and afterwards the witness box, were filled with ignorant and forward youngsters, thirsting after the distinction of delivering their crude, and in several instances, absurd and erroneous opinions. To such was the examination of the unfortunate young lady's body intrusted, and to such these momentous inquiries are but too often confided in this country, because persons who are not over-burdened either with business or information, are always ready to thrust themselves forward, while those capable of giving satisfactory evidence are, for the most part, indisposed to volunteer their services, where possibly they may not be appreciated, and where certainly their duties must be always anxious and often perplexing.

We must again protest against being supposed to intend our remarks as applying especially to those gentlemen who gave evidence in this particular case—we mean them to be

entirely general; and farther, we will say that Mr. Partridge and Mr. Beam, who were not volunteers on the occasion, and who could not, under the circumstances, avoid giving their evidence, delivered precisely such sentiments as intelligent surgeons might be expected to do *who had not made themselves acquainted with the subject on which they spoke*. With regard to Mr. Douchez, who, with amusing circumstantiality, described death as having been produced by dislocation of the neck “in the same manner as it was usual to wring the neck of a duck,” we are utterly at a loss to conjecture (as the deceased did not belong to the Drury-Lane establishment) by what process of accident or of contrivance he was enabled to reach the witness-box. But, be this as it may, there were other surgeons present, and why, we ask again, were not they called? Why was the demonstrator of anatomy alone consulted? Why was the court not benefited by the opinion of the Professor? In fulfilling the ends of justice, it is always proper to have recourse to the highest authorities within reach, and as Mr. Mayo was present at the examination of the body, so he ought to have been called upon for his evidence. Had it been so, we have no doubt that the professional opinions, and the strong probabilities of the case, would not have been so much at variance as to the cause of death; for putting other considerations aside, we regard the learned professor as much too sensible a person not to make himself acquainted with the contents of this journal, and he must thus have been familiar with the appearances found on examining the body of the unhappy woman murdered by Burke, followed by the observations of Dr. Christison, in which all the wonted acumen of that intelligent medical jurist is displayed*. Our learned

* Vol. iii. p. 638.

brethren in the north will doubtless smile at the confidence with which a circumstance, probably quite unconnected with the event, was assigned as the cause of death, while that which was probably the real mode of destroying life does not seem to have been admitted as even possible; but they will be mistaken if they suppose that the medical men of London generally concurred in these inferences; as they must also be aware, that no one of any degree of eminence was called, with the exception of Mr. Tyrrell, and who, it must be admitted, presents an unfortunate example of the erroneous opinions which intelligent men may form on such subjects—unless they take the trouble to make themselves acquainted with what others have done towards their elucidation. Indeed, it appears quite extraordinary that not one of those examined in this last case should have been aware of what had transpired in the former instance, when the trial of Burke excited so much attention; and it is even more wonderful, that they should not have taken advantage of the interval between the Coroner's inquest and the proceedings at the Old Bailey, to inform themselves upon the subject. We allude particularly to the striking similarity in the post-mortem appearances, displayed in the body of Margery Campbell, the first person who was ascertained to have been *burked*, and that of the boy who was brought to King's College.

The mistakes under which, according to our view, the gentlemen who conducted the latter examination laboured, were—first, that the appearances in the neck could not have been produced except by injury inflicted during life; and, secondly, that those appearances were sufficient to prove that the individual had not been suffocated. On both these points we have the misfortune to differ from them. Mr. Partridge's statement is as follows:—

“The brain and its continuation, the spinal cord or marrow, were likewise examined, and were found to be perfectly healthy. In cutting down through the skin and muscles at the back of the neck, in order to come at the bony canal in which the spinal cord is contained, a quantity of coagulated blood was found in the interstices of the muscles; and on removing the back part of the bony canal, some blood was found upon the membrane which envelops the spinal cord. There was coagulated blood opposite the muscles, where a blow might have been struck on the back part of the neck. There was uncoagulated blood found within the rest of the bony canal which contains the spinal marrow; the spinal marrow itself appeared to be perfectly healthy, and there was no other remarkable appearance about it. It was his opinion that the marks of internal violence which he had stated, were sufficient to produce death. On external examination of the body, he could not discover any thing that would have been sufficient to account for death.”

Again: Mr. Beaman distinctly stated that there was no appearance whatever of any injury on the external surface of the neck; but, speaking of the parts beneath, said that the blood “*must have been effused while the subject was alive*,” while, in alluding to a bruise on the scalp, he observed—“I detected some blood—about the size of a crown. Such an appearance as that must have been produced by a blow *given during life*.”

Now, in the first place, we have to observe, that the total absence of any degree of tumefaction, abrasion, or discoloration of the skin over the site of the supposed blow—a blow assumed to have been so heavy as to cause death—is exceedingly improbable; and, in the second place, there is nothing in the appearances presented on the person of this boy, which was not found in Margery Campbell, or which is not occasionally met with where the body, soon after death, is packed up as *subjects* usually are, in the smallest possible

space, with the head forcibly bent upon the chest.

Hear the postmortem appearances in Margery Campbell, in whom no external mark directed attention to the neck.

“An extensive effusion of semifluid blood under the trapezius muscle, near the inferior angle of the right scapula; a small effusion of the same nature in the left loin; neither of them indicated by any outward mark. Some black fluid blood extravasated into the cellular tissue, and among the muscular fibres in various parts of each side of the cervical and dorsal spine, but especially of the upper cervical spine. No displacement or fracture of the vertebræ. A little blood under the anterior ligament of the spine, covering the fore part of the bodies of the third and fourth cervical vertebræ, and this blood evidently extended into the intervertebral space. On careful examination we found nearly the whole posterior ligamentous connexions between the two vertebræ ruptured—namely, the posterior ligament of the spine, the posterior half of the intervertebral substance, the posterior halves of the capsules of the articulations of the oblique processes, and the whole of the yellow ligament of the spine, except what connects the tips of the spinous processes. In the region of the rupture, blood was minutely injected among the fibres of the spinal muscles into the cellular tissue between them, and into the lacerated part of the intervertebral space. On the sheath of the spinal cord, opposite the rupture, there was a mass of thick semifluid black blood, about the diameter of a half-penny, and twice its thickness, from which also a thin layer of the same kind of blood extended along the posterior surface of the sheath as far down as the dorsal vertebræ. The spinal cord was not injured, and no blood could be found under the sheath*.”

The resemblance between the appearances in the neck of the Italian boy, and the woman murdered by Burke, amounts to a complete parallelism. But it may, perhaps, be said, that Burke destroyed his victim, not by suffocation, but by a blow on precisely the same part

of the neck as was supposed to have occurred in the Italian boy—rather an improbable occurrence, by the way, independently of the positive proofs to the contrary. Let us next, then, see what the result is, when a person, dying a natural death, has the head bent forcibly forward. This was done by Dr. Christison, three hours and a quarter after the decease of a man who had died of fever. The appearances are thus described:—

“Among the spinal muscles in the neck and upper part of the back, black fluid blood was here and there effused between their fibres. Between the third and fourth, as well as between the sixth and seventh cervical vertebræ, the whole yellow ligament of the spine was lacerated, except at the mere tips of the spines; a considerable quantity of fluid blood was effused into the loose cellular tissue between the dura mater and ligamentous covering of the spinal canal posteriorly, and likewise between that covering and the bone itself. There was not any effusion within the sheath. The posterior ligament of the spine was uninjured.”

The same experiment was tried in several instances, and with the same general results; nor can we require more convincing evidence, that we are not warranted in concluding, because we find effusion about the spinal canal, that that must necessarily have taken place during life.

Another circumstance is deserving of attention. “I found (said one witness) no external appearance of violence at the back of the neck;” but “it would have produced such a mark if the boy had lived some time after the blow had been given.” This idea is wholly founded in error: not only is it unnecessary that an individual should live some time after he has received a blow, in order that it may produce a bruised mark, but such appearance is readily produced, though the individual has been dead “some time” before the

* Edinburgh Medical and Surgical Journal, April 1829.

blow be inflicted;—a very important fact, with reference to medical evidence, but too well ascertained to require that we should offer any proofs of its correctness.

The only circumstance, indeed, which presents a shadow of difference between the cases is, that in the Italian boy the blood was in part coagulated, whereas, in the case of Campbell, and in the experiments, it was mostly fluid. But although this phenomenon was mentioned by the witnesses, it is one to which they evidently attached no importance, because they did not assign the *coagulation* of the blood, but its *effusion*, as the ground of their opinion that the injury had been inflicted during life. The blood being in the condition described, appears to us to afford a strong presumptive proof that the injury had been inflicted on the spine soon after death, and probably by forcibly coiling up the body into a box or hamper; but it goes no farther than this. Indeed Dr. Christison, while he admits that in his experiments (wherein some time was necessarily suffered to intervene after death) he never found the blood coagulated, observes, “but it may be doubted whether clots might not be found if the injury was inflicted soon after death;” and he expressly mentions a case, in which “blood drawn from the jugular and femoral veins, *eight hours* after death, flowed out quite fluid, and in a few minutes *formed a firm coagulum*, with separation of serum”—an illustration which appears to us decisive of the question, and positively to prove that the opinion was not correct, that, in the case of the Italian boy, the blood “must have been effused while the patient was alive.”

With regard to the opinion that “unquestionably the deceased did *not* die from suffocation or strangulation,” we can only explain it on the supposition that the witnesses believed such death

to leave signs on the corpse, so obvious as at once to attract attention, and so unequivocal as to remove all doubt. On this point it may be sufficient to observe, that the idea is one derived rather from popular impression than from actual observation; that nothing is necessarily present in the bodies of those who have been suffocated, which might not entirely escape the notice of persons who, however skilful as anatomists, had not been accustomed to contemplate the dead body with the eye of medical jurists; and that the indications usually left by suffocation—viz. tumefaction, and redness about the face and eyes, were actually present in the Italian boy.

The heart was unusually empty; but on this no stress is laid, nor can any inference be drawn from it in favour of either of the supposed modes of death; for if not usual in suffocation, so neither is it in sudden death from external violence; nor are we aware of its having been described as characteristic of any morbid condition, except the “*idiopathic asphyxia*” of the late Mr. Chevalier.

Here, then, for the present, we close our observations, leaving it open for us to resume them, should occasion require. We have thought it incumbent on us to record our dissent from the opinions and reasoning of those who gave evidence in this case; and though all are probably satisfied that justice had no more than her due in the execution of the accused, yet, had proof of the particular mode in which the death was effected been essentially necessary towards establishing the guilt of the parties, and had they been condemned, in consequence of the medical evidence being regarded by the court as conclusive on this point, the witnesses, we suspect, would by this time have given no trifling consideration to be able to revise their opinions. We earnestly hope that the circumstances we have detailed will lead prac-

tioners to pay more attention, than they are wont to do, to the important and interesting, but difficult subject, of forensic medicine.

WAS THE DUKE OF BOURBON MURDERED ?

It may be interesting at the present moment, in a medico-legal point of view, to notice some of the circumstances which attended the death of this prince. It was an event which, had it occurred at any other time than so soon as it did after the late revolution, would have excited considerable inquiry, not only in France, but throughout Europe. Even as it was, the subject was not unmooted by the medical jurists of the French Académie. Three memoirs were, in consequence, given to the profession; one by M. Marc*, in the *Annales d'Hygiène et de Médecine Légale*; another by M. Dubois, of Amiens, in the *Revue Médicale*; and another by M. Gendrin, in the *Transactions Médicales*: all professing to elucidate the causes of his Royal Highness's death.

M. Marc maintained that the prince had committed suicide; MM. Dubois and Gendrin, that he *might have been* murdered. Some of the reasons assigned were the following. There were no marks of external violence upon the body, except where the noose was tied. The print of this, round the neck, was *oblique*, from each side proceeding upwards and backwards, without any appearance of pressure behind; from which circumstance, M. Marc inferred that the strangulation was not effected by the hands of others, as then the mark would have been parallel, or nearly so, to the lower jaw. The same physician also observed, that if assassins had done the work, they would have used a rope, or a cord, and not the old man's neckcloth. To which M. Dubois replied, that the absence of marks of violence proved nothing against the supposition of murder; there would have been no need of violence with a feeble person of 74, lying in his bed. Assassins might just

as readily have employed a handkerchief as a cord; and the obliquity of the print merely shewed the direction of the violence, and by no means excluded the supposition that it was inflicted by other hands. M. Gendrin fully coincided in this view of the case.

It should further be observed that M. Marc, though he denied the existence of external injury, except from the noose, admitted that there were *excoriations*,—one particularly, on the outer and fore part of the right leg, bloody, uneven, about six inches in length, by two in breadth; and on the left leg were other similar marks, but not quite so large. These M. Marc pretended to consider as *so far from* corroborating the hypothesis of assassination, that they *perfectly* explained how the suicide was effected: in precipitating himself off the chair, and, during his convulsive struggles, he rubbed his legs against the projecting edge of the seat*. M. Dubois thinks this a gratuitous hypothesis, for the excoriation might have been produced by the friction of any other body as well as of the edge of the chair; and even supposing that it was the chair, there was nothing to preclude the additional supposition that the injury was inflicted while the Prince was being hung by assassins. The reasonings of M. Gendrin lead him also to a conclusion quite the reverse of M. Marc's: the excoriations, in his opinion, *so far from* indicating the commission of suicide, are inexplicable on that hypothesis: they were most probably occasioned while the victim was dragged to the place of suspension, by the legs rubbing against some furniture in the apartment; or after he was suspended, by violently withdrawing the chair.

After discussing the *material* circumstances of the Prince's death, the disputants took up the *moral* considerations attending that act. Into these we must decline entering, for obvious reasons; the more so, as the medical jurists in question seem to be swayed by an undue political bias, and to view the facts of the case with over-much of the spirit of party. With us, some

* In the sitting of the Ac. de Med. 30 Nov. 1830, this gentleman read a paper on the subject, from which we derive some of the particulars in the present article.

* It may be worth noticing that the Duke's legs were not off the floor, when he was found hanging; but legal medicine abounds with instances of this sort. Persons have been found strangled on their knees or in a sitting posture. The slightest cerebral compression seems to suffice for the paralyzing of the lower extremities, and to render any motion for relief impossible.

of the particulars recently disclosed, we must confess, have considerable weight: we allude to the old man's winding up all his watches as usual—the knot on his handkerchief to remind him of something to be done next day—his well-known love of life, and often expressed abhorrence of suicide. The "*melancholy*" upon which M. Marc lays the chief stress, in his moral survey, will scarcely bear him through his hypothesis.

We ought probably to have mentioned sooner, that M. Marc was, and we believe is, physician in ordinary to his Majesty, Louis-Philippe.

NEW STYPTIC.

MM. TALRICH and Halma-Grand, deposited at the Academy of Sciences, in Paris, on the 26th September, a sealed packet, containing the composition of their liquid for arresting hæmorrhage. It is to be opened as soon as a series of experiments now in progress have been completed. The following is an account of some of these trials. Fifteen sheep have publicly had the carotid artery opened: four lengthways, nine across, and two with an oval portion cut out. In all of these the bleeding was arrested in four or five minutes, and the cicatrization completed in a few days. The same result was obtained on the horse whose carotid was opened a few days ago in the abattoir at Montfaucon. In order to stop the bleeding, dossils of lint, steeped in the fluid, are applied with some degree of pressure, and afterwards suffered to remain there. The writer in the French journal from which we take these particulars, says that he was very lately called to a young gentleman, who had had copious bleeding for twenty-four hours, in consequence of having had a tooth extracted from the lower jaw. Various approved methods had been tried without avail, and the actual cautery was just about to be had recourse to, when it was determined to give the styptic of MM. Talrich and Halma-Grand a trial. In seven minutes the bleeding was completely stopped.

A correspondent informs us, that the styptic above alluded to has been tried at St. George's Hospital. The carotid artery of a sheep was opened by means of a longitudinal incision, about two lines in length; dossils of lint were applied, and covered with compresses,

which were retained in their place with some firmness for about a quarter of an hour; they were then removed, the lint being suffered to remain. The animal was set at liberty, and no bleeding occurred. At the end of some days it was slaughtered. On examining the parts, great extravasation of blood was found in the cellular texture round the vessel, which itself contained no clot.

On Tuesday last, in consequence of a circular letter of invitation, a considerable number of persons assembled to witness some farther experiments. The first consisted in laying bare the carotid artery of a sheep, and cutting out a portion of it with a curved scissors. Portions of lint, about the size of a small walnut, steeped in the styptic, were applied to the wounded vessel, the first being kept steadily in its place for ten minutes, and two or three others successively laid over it, the whole process occupying a quarter of an hour. The animal was then unbound, and suffered to rise, when the lint was instantly thrown out, and the animal bled to death. In another the carotid artery was cut across to the extent of about half its calibre, and then treated as above, with the addition of a stitch, to retain the lint in its place. No hæmorrhage followed; the stitch and lint were removed next day, and the animal appears to suffer little inconvenience.

ANALYSIS OF THE BLOOD IN CHOLERA.

To the Editors of the London Medical Gazette.

GENTLEMEN,

HAVING been enabled to complete the experimental inquiries on which I have for some time past been engaged in Newcastle upon Tyne, I beg you will have the kindness to give insertion to the annexed outline of the results I have obtained:—

1. The blood drawn in the worst cases of the cholera, is unchanged in its anatomical or globular structure.

2. It has lost a large proportion of its water, 1000 parts of cholera serum having but the average of 850 parts of water.

3. It has lost also a great proportion of its NEUTRAL saline ingredients.

4. *Of the free alkali contained in healthy serum, not a particle is present in some cholera cases, and barely a trace in others*.*

5. Urea exists in the cases where suppression of urine has been a marked symptom.

6. *All the salts deficient in the blood, especially the alkali or carbonate of soda, are present in large quantities in the peculiar white dejected matters*.*

There are other results of minor consequence to which I will not at present allude, neither shall I on this occasion offer any observation on the practical inference to which my experiments may lead. In a few days a detailed report shall be published, in which the mode of analysis, &c. will be minutely described. It will be found, I regret to state, to contradict in every important particular that recently given by Hermann, as translated in the *Journal de Chimie Medicale*. All my experiments, however, have been publicly performed, and can be authenticated by numerous witnesses—a precaution I thought it necessary to adopt, lest it might be supposed that I impugned, without sufficient foundation, the accuracy of the Moscow professor.

May I add, that, until the publication of my report, I shall deem the suspension of discussion on the results now announced, as a matter of personal courtesy and obligation.

I have the honour to be, gentlemen,
Your obedient servant,
W. B. O'SHAUGHNESSY, M.D.

London, Dec. 29, 1831.

ANATOMY.

THE following excellent letter has been addressed by the Council of the College of Surgeons to the Secretary of the Home Department:—

Royal College of Surgeons in
London, Dec. 10, 1831.

My Lord,—The undersigned members of the Council of the College of Surgeons in London, have the honour of addressing your Lordship on a subject of painful interest to the whole community, but especially to the members of the medical profession.

The Royal College of Surgeons are em-

powered by their charter to examine certain individuals as to their knowledge of surgery, and they are especially required to institute such examination respecting those who are candidates for the situation of surgeon in the army or navy.

It is not possible that any one should be properly qualified to practice in this department of the healing art, who has not attained a due knowledge of human anatomy, and explored with his own hand the structure of the dead body. Proofs of their having done so have therefore been always required of candidates who have presented themselves for examination.

The Council believed that they could not properly perform their duty to their Sovereign, from whom the College received its charter, nor to the public, for whose benefit it was granted, without insisting on the study of anatomy by dissection, as the most important part of surgical education.

They have, however, been aware that some serious objections might be urged to the course which they thus ventured to take.

In the present state of the common law, as it is construed by the law authorities, the individual who dissects a human body, or even has it in his possession for any other purpose than that of burial, is guilty of a misdemeanor, unless it be the body of a malefactor hanged for murder.

Bodies used for dissection in the anatomical schools have necessarily been procured by illegal means: by the invasion of consecrated ground, and the disturbance of graves, in a way disgusting to society at large, and especially offensive to the friends and relatives of the deceased.

The regulations of the Council have therefore had a tendency to encourage both teachers and students to a direct violation of the law, and to establish, in the procurers of dead bodies, a set of men living by practices which are revolting to the feelings of society, exposed to the hatred and contempt of those around them, and likely, by the joint operation of these causes, to become trained and gradually habituated to the commission of still greater crimes.

The Council felt that they could only do what was, on the whole, for the best, in the dilemma in which they were placed. The circumstances, which have just been enumerated, did not escape their attention, and have continually excited their most deep regret. But, on the other hand, they were called upon to regard the obligations of their charter. They were aware that the want of properly educated surgeons would prove a serious evil to the public. However much they might be inclined to encourage the use of preserved parts and models as subsidiary means of teaching anatomy, they were convinced that these are of themselves quite inadequate to afford that minute, complete, and accurate

* The blood and dejected substance were obtained, in one analysis, from the same patient, and the blood was drawn half an hour after the evacuation occurred.

knowledge which is necessary in surgical practice, and which the student acquires by dissection.

The Council further submit that they have laboured under much embarrassment from the inconsistencies and contradictions of the law itself, which at the same time that it declares the student to be guilty of a misdemeanor if he attempt to attain anatomical knowledge, renders him, when afterwards engaged in practice, liable to a civil action on account of any mistake which his ignorance of anatomy may lead him to commit.

But whatever may have been the extent of the difficulties which have heretofore obstructed the Council in the execution of their duty, they may well be regarded as insignificant when compared with those which they have to encounter at the present moment.

The large prices which have of late been given for anatomical subjects have operated as a premium for murder. If the Council of the College continue to require that those who present themselves for examination shall have studied practical anatomy, who can venture to say that crimes similar to those which have just now filled the public with dismay will not be again committed? More criminals will undoubtedly arise; new victims will be added to the list; and the medical profession will be necessarily degraded from the high station which it ought to hold as having in its relations to society no object but that of conferring benefit on others.

The Council have no expectation while the law remains as it is at present, and surgical students continue to cultivate the science of anatomy, that any means can be contrived which will prevent a repetition of the horrible offences to which they have just alluded.

Attention and constant suspicion on the part of the teachers may effect much, but not all that is requisite. It is vain to imagine it always possible to distinguish the body of a person who has been murdered from that of one who has died a natural death. The very individuals who have lately suffered on the scaffold would probably have escaped detection if they had been more circumspect and wary in their conduct.

Nor can all the precautions with which it is desirable the study of anatomy should be conducted, be adopted under the existing laws. In the other countries of Europe, anatomy is taught only under a license, and in certain places appointed by the government, and an exact register is preserved of all the bodies consigned for dissection. But it is a contradiction to suppose that any such license can be granted, or such register preserved in this country, where the study of anatomy is barely tolerated, and where not only the procurers of dead bodies, but the anatomical teachers and students, are alike engaged in illegal pursuits.

In offering this representation to his Ma-

jesty's Government, the Council are not without hopes that some plan may be devised by the Legislature calculated to remove the serious evil of which they now complain. At the same time they beg leave to declare on their own part, and on that of all the other members of their profession who are now in practice, with the exception of the very few who devote themselves to the laborious and often unprofitable task of teaching anatomy, that the question is one in which they have no direct or personal interest. Whether anatomy be taught legally or illegally, or not at all, does not concern the existing race of the practitioners in medicine or surgery, who have completed the period of their education: but it deeply concerns the public: and it is under a strong sense of the evils which society may ultimately experience, and from a desire conscientiously to perform their duties, that the Council of the College have ventured to make this demand on your Lordship's patience and attention, at a moment which, on an occasion of less importance, they should have deemed unreasonable.

(Signed by the President, Vice-Presidents, and all the other Members of the Council.)

DR. WILSON PHILIP'S PAPER ON CHOLERA.

WE omitted to mention in the account of this paper which we gave last week, that it was communicated by the learned author to the Westminster Medical Society, in consequence of a request expressly made to him through one of the Presidents.

METEOROLOGICAL JOURNAL,

*Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.*

Dec. 1831.	THERMOMETER.	BAROMETER.
22	from 26 to 46	from 29.75 to 29.53
23	26 39	29.55 29.71
24	24 37	29.81 29.94
25	21 31	30.07 30.24
26	24 39	30.39 30.35
27	34 40	30.33 30.27
28	30 41	30.20 30.27

Wind variable, N.W. prevailing.

Except the 23d, 24th, and morning of the 26th, generally cloudy; a little rain in the evening of the 22d; a very dense fog during the 25th.

Rain fallen, .025 of an inch.

CHARLES HENRY ADAMS.

W. WILSON, Printer, 57, Skinner-Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JANUARY 7, 1832.

LECTURES

ON

THE THEORY AND PRACTICE OF
MEDICINE;

Delivered at the London University,

BY DR. ELLIOTSON.

PART I.—LECTURE XIV.

Treatment of Inflammation—continued.

I BEGAN, gentlemen, at the last lecture, the consideration of the treatment of acute inflammation. I stated that the object in the first instance was to lessen the ordinary stimuli, both external and internal.

With respect to external stimuli, we have to moderate the temperature, and even to apply cold to the inflamed parts if they be within our reach; and some parts of the body, although situated within, may be considerably benefitted, when inflamed, by the application of cold to the corresponding external portion. This is the case in regard to the head, and some inflammatory affections of the chest. In hæmoptysis, of a decidedly inflammatory character, I have never seen the application of ice attended with harm. Some have said that the application of cold in the case of inflammation within the abdomen, has been of use, but I am not aware that that is the case. I stated that ice may be frequently employed for this purpose, but that the feelings of the patient should always be attended to. If you apply cold, and it is unpleasant to the patient, you are almost sure to do harm; whereas, if you apply warmth, combined with moisture, it is frequently attended with a good effect. I stated that very possibly we might explain the similar operation of these two opposite agents in this way—that the cold

diminished the sensibility of the part, and the quantity of blood in the inflamed vessels, and consequently the cause of tension; whereas the application of warmth and moisture did not lessen the cause of tension, but allowed the parts to yield more easily to the distention caused by the disease, and that the latter mode might do good also by exciting a free secretion of the inflamed part, or of a portion of the surface corresponding with the inflamed part.

I believe I omitted to mention that when the surface is abraded, or when it is a mucous membrane that is inflamed, such as the interior of the month, or the conjunctiva of the eyes, which is very similar to a mucous membrane, that it is generally found of great service to combine a small portion of the superacetate of lead with the cold, but not sufficient to irritate the part. When the surface, however, is not abraded, I doubt whether the employment of lead is more efficacious than plain cold water, or an evaporating lotion.

In regard to venesection, I stated that I believe it will answer every purpose, whatever the nature of the inflammation may be, and that in the case of the head it would do just as well as arteriotomy. I was particularly anxious to impress upon you the necessity of making a sudden and rapid impression on the patient—that in active inflammation there is frequently a great indisposition to faint, and a far larger quantity of blood may be lost without syncope being induced than could be sustained in health. It is usually found that the more speedily blood is taken away, the greater the effect, the greater the benefit; and unless you attend to this, you may sometimes drain a person's system, and perhaps fail after all, or at any rate you take away much more blood than is necessary. Indeed, there are some inflammations which proceed with such extraordinary rapidity, lasting only a few hours, destroying life before the day is out, that unless you knock down the complaint at once, all your

attempts to do it the day afterwards are useless. I have seen cases of this description.

I stated that I believe local bleeding is employed very frequently in acute cases, where general bleeding will answer every purpose. If inflammation occur in persons who are exceedingly weak, it is often much the best plan to make them stand up, if possible, or support them in that posture, to make a large orifice in the arm, or, if you choose, open both arms at the same time, so as to produce fainting, which you may frequently do by the loss of five or six ounces of blood, and this is far superior to applying twenty or thirty leeches during the day, taking away more blood, without making the same impression on the system. In regard to local bleeding, I stated, that although in general we are in the habit of withdrawing blood from the neighbourhood of the inflamed part—as, for instance, the hypochondrium in inflammation of the liver, the epigastrium in inflammation of the stomach, and from the region of the left breast in inflammation of the heart—yet it is certainly a fact that the detraction of blood from a distant part is frequently of great utility, and many think of even greater utility than from the immediate seat of the disease. The ancients were very fond of this, or at least the older writers, and supposed that a revulsion was produced—that the blood would immediately rush from the inflamed part to that from which the depletion was made. I certainly think that in this country the circumstance has been by far too much overlooked. I know that I have had cases of inflammation of the head, as I will mention hereafter, where repeated local bleedings, even till the patient became pale, proved of no avail; but on taking away blood from the sides of the trunk—far enough from the head—the relief was perfect, and the patients got well. I have had several cases of this description. There can be no doubt that the application of leeches to the anus will frequently produce the most decided relief in affections of the head, and indeed even of the heart. Whether it is more efficacious than local bleeding from those parts I can scarcely say: I can only assert that I know that very great benefit, and even perfect relief, has been derived from the practice. Of course, you know that on the continent this mode is commonly resorted to, but from delicacy it is not usual here, nor do I know that it is absolutely necessary, but I have seen people who have been treated this way abroad anxious to have it put in practice, and they have certainly found the most decided relief.

When two or three parts are affected—as, for instance, the head and stomach, it is frequently a good practice to take blood from the anus. One can easily imagine why great relief should be obtained from blood

being taken away in that part. The veins there go to form the vena portæ, and therefore a great load is taken from the liver, a great load is taken from the whole venous system; less blood goes to the heart. As the hæmorrhoidal veins run to assist in forming the vena portæ, and this branches through the hepatic veins to the vena cava inferior, far greater relief is obtained in diseases of the heart, and affections situated within the chest, as well as in great congestion within the head, by this means sometimes than by any other. Still, however, it is easy to make assertions in medicine and in surgery, and one ought to have a large number of cases registered for comparison, shewing where relief is obtained, and where not, before we draw any conclusions. General assertions are frequently not to be depended upon; extensive and accurate observations are necessary to enable us to say that one mode of treatment is superior to another. I do not know that the removal of blood from the anus is decidedly preferable; whether the same quantity taken from other parts would be more or less beneficial, I cannot say; but certainly I have been surprised frequently in affections of the head on seeing the benefit that is derived from taking away blood around the abdomen, or from the anus.

Counter-Irritants.

I was entering at the conclusion of the last lecture on the consideration of the second division of the treatment of active inflammation, namely, the administration of counter-irritants,—the remedies which produce a particular operation on the body, some a depressive and others merely a peculiar operation, but which operation is found to be very beneficial in controlling inflammation. I mentioned that the chief of these were, I believe, colchicum, digitalis, antimony, and mercury.

Digitalis.—With respect to digitalis, I mentioned that I had not sufficient experience of my own to say what degree of reliance may be placed upon it. Where I have seen it used in doses of twenty or thirty drops, every six or eight hours, I have not noticed such benefit as would lead me to place dependence upon it; but I know that others have employed it in a larger quantity, and they say with great advantage. Not being able to speak from my own experience, I only mention the assertions of others; it is a remedy of which I have a little dread, from having seen, I fancy, several people suddenly die who were taking it. Probably from the operation of the medicine, they have suddenly fallen back in bed, and died in a very unexpected manner.

Colchicum.—With respect to colchicum, I have tried it very extensively, and I think that, except in rheumatism and gout, you

cannot place any thing like the same reliance upon it that you may on mercury.

Antimony.—With regard to antimony, that also is very inefficient compared to mercury. It must be given, in order to produce any decided effect, in very large quantities; exactly as digitalis, I presume, must be exhibited. That form of antimony which should be employed I think is the antim. tartarizatum. If you administer pulvis antimonialis, and find that its operation is inert, you are not to conceive that it arises from the strong action going on in the system, and which certainly does continually counteract all agents. In the violent pain of tetanus you may give an ounce of laudanum, and in various other diseases besides inflammation large doses of medicine are borne, and indeed required. In inflammation, many agents are resisted, and among the rest antimony; but if you employ antimonial powders, the inertness you meet with is not to be attributed to the resistance to the remedy under particular circumstances, because, as I stated, you may give large quantities of it to a man in perfect health without any effect being produced. I mentioned that I had found an old hospital ticket of a patient in St. Thomas's, who had taken it to see how far we might go. There was little the matter with the man, and as I found that he took one drachm, and then a drachm and a half, three times a day, of pulv. antimonialis with no more effect than would have resulted from powder of post, I gradually increased the quantity till he took 130 grains, three times a day. I have frequently given a drachm and a half without any effect whatever, though now and then a person has been made sick with it. It is the pulvis antimonialis which is so inert, and I therefore recommend you, if antimony be employed at all, to give antimonium tartarizatum. It is of no use to give a few drops of the wine; you should give a quantity requisite for producing nausea, and you then depress the system. You may give a grain every two or three hours, I know; I have myself given 24 grains in 24 hours. There is a man in St. Thomas's at this moment who has incipient phthisis, and there is inflammation of the bronchiæ, consequently he must be treated by antiphlogistic measures. I fear there are tubercles at the bottom of his complaint, and I am therefore not willing to give him mercury, and he has been taking a grain of tartar emetic every 4 hours without nausea. He has taken it for several days, and yesterday it produced sickness for the first time, so that the frequency of the dose is now reduced to every six hours. Where it is exhibited, it should be in considerable doses. You will find that it only produces sickness at first; that the sickness then goes off, and perhaps does not return for some time; but at last I have found it return and continue.

Mercury.—I have made comparative experiments with antimony and mercury, and I am quite certain that the success of those who employ antimony in addition to bleeding is very inferior to what those are accustomed to see who employ mercury. In violent inflammation, I really make it a matter of conscience to employ mercury. If there be great danger, as in violent inflammation of the larynx, where the patient, if neglected, would be dead in a few hours, I do not hesitate to give ten grains of calomel every two hours. In this case, if you do not bring your means to operate very speedily, the patient will in a moment be suffocated, fall back in bed from œdema in the glottis, and the case be over. In other instances, however, there is no occasion to be so active; five grains every four hours will do very well, or perhaps rather less. In a case where every moment life appeared to be in danger, I have given as much as a scruple every two hours; these, however, are extreme cases, and in which you are very sure that if you do not instantly save the patient's life, he will slip through your fingers. If the mercury begin to run off by the bowels, it is better to unite it with opium. It very soon does this; and you see the necessity of resorting to opium, or, if there be an objection to this remedy, you might give an infusion of catechu, which is one of the most powerful astringents. Of course, astringents in tinctures must be injurious in inflammation, and it is better to give an infusion. Kino, for what I know, is as good as catechu. If the calomel run off in spite of all you can do, you may exhibit pil. hydrargi; but what is still less likely to do so, is hydrarg. c. creta, with which you may salivate a person very readily. I know you will find it stated in some books that salivation cannot be produced by it, but I have salivated hundreds with it. You may give a dose of ten grains every two or three hours; but I think it has a greater tendency to produce sickness than any of the other three forms of mercury. You will find more patients become sick who are taking hydrarg. c. creta, than those who take either calomel or blue pill. It is frequently necessary to unite it with opium.

It may be right, also, to rub in mercury externally on the extremities and abdomen as quickly as it can be done.

The plan is, as far as I know, to get the mouth sore as quickly as possible. It is right, at every visit, to press upon each gum, and likewise to press under the lower jaw; to look at the gums, and smell the breath. The moment you find any symptoms of an affection of the mouth, the remedy should be suspended,—you certainly should not go on. In some cases the affection of the mouth will run beyond what you wish; but it is better now and then to have a violent pyalism, and save nearly all your patients, than

to be over particular,—to take too much precaution, and lose a patient now and then. I can state as a positive fact, that if bleeding be properly had recourse to, and you can produce a certain degree of affection of the mouth, it is very, very rare, indeed, that you will lose a patient with acute inflammation, unless there be some organic disease that keeps up the irritation in spite of all you can do, or unless your efforts be counteracted by something wrong in regard to diet, or some violent emotion of mind, which must be more than a match for all the best remedies, or by something unusual.

I would strongly advise you, nay I am anxious, that you should read Dr. Duncan's Medical Commentaries, published in 1788. All we know now relative to the use of mercury was known then, though it has been considered that the facts known relative to the use of mercury in many local inflammations were first generalized by Dr. Armstrong. The general fact is fully stated by Dr. Hamilton. It is singular how often many of us, who know excellent practical things, do not practise them when we come into practice. It is a sort of inertness that is apt to come upon us. The paper to which I refer, is by Dr. Hamilton, of Lynn-Regis, who says that he learned the use of mercury from a navy surgeon; that having been informed by a navy surgeon of the great use of mercury in inflammation, he gave it in pleuritis, in hepatitis, and all the itises, and they all yielded in the most extraordinary manner, in a way that he never found before by any practice he adopted.

But, with respect to the use of mercury, I beg particularly to state, that it is not to make you employ less bleeding before hand, or at the time you are exhibiting it. So far as there is occasion for bleeding, it is to be put into practice whether you employ mercury, or any other remedy, or no remedy; but you will find as you go on with bleeding, and as you go on exhibiting mercury, that accordingly as the mercury produces its effect upon the mouth, you have occasion for less bleeding. You should bleed certainly according to the necessity of the case, without considering whether you are giving mercury or not. You will find the necessity of practising bloodletting to be far less than if you gave no mercury at all; but, if you omit bleeding, mercury will not answer your purpose. Bleeding is the great remedy,—it is the sheet anchor, but the addition of mercury renders far less bleeding necessary; it causes the bleeding which you do put in practice to be ten times more efficacious. It is necessary to have a clear view upon the subject,—not to imagine for a moment that it will become a substitute for bleeding, but that it will lessen the necessity of depletion. Dr. Armstrong used to say, that bleeding was the right

arm and mercury the left arm of medicine in the treatment of inflammation. That was an excellent expression of the fact.

Ptyalism.—If it so happen that the mouth become too much affected, I do not think you can employ a better application than the chloride of lime or of soda. If one ounce of the solution be mixed with six or eight of water, and the patient's mouth washed with this every hour, you will find the ptyalism readily go away. If ulceration have taken place, time must be required for it to heal, and the sore mouth may remain for a long time; but if you be before-hand—if you employ the chloride before ulceration has occurred—you will find the soreness from the inflammation of the mouth in general rapidly give way. It should be employed just in sufficient strength to be felt by the patient, but not to produce any pain. It is also to be remembered, that if you maintain a free state of the bowels there is much less danger of a violent affection of the mouth; and if the mouth become affected, and you open the bowels freely, you generally diminish it to a certain extent.

Opium.—You will find some cases in which it would be very wrong to employ opium—for instance, in phrenitis; but it is a practice with some to give a full dose of opium after they have instituted a copious venesection; and, as far as I know, it is a very good practice. Two or three grains of opium, after venesection to syncope, will frequently prevent the necessity of another bleeding, or at any rate will send the patient into a quiet sleep, by which he will be very much refreshed. I never myself saw any harm done by it. I know it is a practice void of danger for the most part, unless there be inflammation of the head. I know that it frequently produces a beneficial soothing effect; and, if the bleeding be likely to cause great irritation in the patient, if the patient be weak, it may be a highly proper practice; it may be wrong to omit it, because it may prevent that morbid irritability, that restlessness, which sometimes takes place after bleeding; but still it ought not to take the place of mercury, and I would exhibit the latter just as though I had not administered the former. The chief point should be to endeavour to knock down the inflammation by free bleeding: if you choose you may follow that up by the local detraction of blood, and you may give a full dose of opium as soon as the blood-letting is over; but then you should be as anxious to give mercury as though that were itself the remedy. Still, however, you will cure many, many cases, without mercury; but if it be an object to save as many patients as you possibly can—which it undoubtedly is—you will save more if you employ mercury than if you do not, and with less loss of blood. Mere bleeding will cure many cases—mere starvation will cure

many cases—but still, if you give mercury, the exceptions will be reduced to a very small number indeed. It is better, with respect to opium, to give one full dose than repeated small doses; for it is found that opium has certainly a stimulating effect over the body; hence the pulse becomes fuller, and there is thirst. If you give small and repeated doses of opium, you know that both thirst and heat will continue. Dr. W. Philip made experiments illustrative of this subject. He applied, to a denuded brain, a small quantity of opium, and likewise of tobacco; and, on looking at the distant capillary vessels in the extremities, he found them very much excited; but if he applied a large portion of either of these ingredients he found the action diminished—just the opposite effect. Now that demonstration of the operation of large and of small doses is perfectly in harmony with what we all observe when we give large and small doses of this remedy. A small quantity of tobacco, for example, will excite feverishness—will make a person, perhaps, hot the whole of the night, and induce thirst; whereas a considerable quantity will depress the pulse, produce a cold chilly sweat, and pull down all the powers of life. So with respect to opium; if you want its sedative effects, it should be given in one full dose. This would be dangerous if you did not bleed; but it is perfectly safe if you have depleted the patient in the first instance copiously.

Supposing, however, that you have overdone the thing—and this is likely to happen—that either yourself or some person before you has bled too much—you then find opium of the greatest use. If too much blood have been lost, and the patient has headache, throbbing of the temples, frequent faintings, deadly faintness, great rapidity of pulse, vertigo—and this is a state one continually sees in women who have lost a great deal of blood during or after labour—then the chief remedy is opium. You will find that all these symptoms are much diminished by it. Stimulants are highly proper; ammonia is one of the best, and good nourishment; but the extreme restlessness of the body particularly, is very much alleviated by the addition of opium. I need not say that, in such a case as this, if you cannot prevail upon the patient to eat, it is necessary to use the stomach-pump, and strong broth should be injected both into the stomach and the rectum.

But notwithstanding this state of the system, you occasionally find that a local congestion of blood takes place, so as to render it necessary to apply leeches. When the body has lost so much blood that you are obliged to give ammonia, and nourishment, and opium, you find sometimes such a tightness of the head, such a degree of heaviness from local congestion, that the application of leeches is indispensably required, or ice.

These matters, however, cannot be learned minutely by precept; the knowledge can only be acquired by seeing cases—by practising yourselves—by acquiring a sort of knack in detecting them. You will see cases where, if you were to take away four ounces more blood, you would certainly precipitate the patient into the grave.

Blisters, &c.—Now with respect to all these things—mercury, colchicum, and so forth—they are general counter-irritants; but we have also frequently to put in practice, in the treatment of inflammation, a local counter-irritant—that is to say, to set up a new action in the neighbourhood, or at a distance. I stated that purgatives chiefly act in this way—not only in removing a quantity of stimulating faces, but by exciting the action of the intestines, and so far diminishing action elsewhere; but we also employ certain downright stimulants to the surface, and of these, perhaps, blisters and sinapisms are the chief.

It is, I believe, best to apply a blister on the surface over the internal part inflamed, as over the right hypochondrium, in inflammation of the liver, and over the thorax, in inflammation of the lungs; but with respect to the head, this is a dangerous practice when there is any active inflammation going on. The application of cold to the head does great good—infinite good in inflammation of the brain itself, or of its membranes; but early stimulating the vertex by a blister is equally injurious. Nothing can be a worse practice than to apply blisters to the top of the head in phrenitis, in the early stages of the disease; when, however, you have evacuated the patient sufficiently, then such a measure is very proper. But with respect to other parts of the body, it is best to apply blisters over the internal part itself which is inflamed. If you want to produce an effect rapidly, certainly a mustard poultice is one of the best things. Some persons have excited a blister by causing the steam of boiling water to play upon the part, and others by putting boiling water in a jug, with a napkin in it, and suddenly inverting it; but I think a strong mustard poultice, beat up with hot vinegar, will produce an effect as speedily as in general is to be desired. In young children, ordinary blisters are often a very dangerous thing; and a mustard poultice applied to the abdomen, or the back of the head, or chest, will produce as great an irritation as you desire, and one which you can control, by removing the sinapism, when the child expresses great distress. For a speedy effect, certainly a large mustard poultice, to cover the whole abdomen or chest, is one of the best things; and it may be taken off at pleasure. It may be borne for ten minutes, or some will bear it an hour: there is no rule for the length of the application.

I may mention with regard to blisters,

that some persons have introduced a solution of cantharides as a substitute for blister plaister. I have made some pretty extensive trials with it at St. Thomas's, because it appeared to me a desirable thing to be able to produce a blister by merely varnishing a part with a brush. It is much neater than the application of a stinking plaister, which a patient has to keep in bed with him for twenty-four hours; but it does not well answer the purpose. A blister was produced, but not immediately; frequently not till the next day; and then, when it was going to heal, a fresh blister would arise. I believe that the young gentlemen who applied it for me, did so with a brush very carefully, but I know that in many instances a far more violent blister was induced than I desired; and sometimes it would not rise at all, and in others blister after blister would arise. Upon the whole I found it very unsatisfactory, and it appeared to me preferable to continue the old practice of applying a common plaister of cantharides.

Atonic Inflammation.

Now in that form of inflammation in which there is little power—in inflammation which is called *atonic*, where there is not only inflammation but the body is in a state of debility, you must employ less evacuants, and trust more to mercury, and indeed to opium. Where patients are debilitated altogether, I should certainly place great reliance upon mercury, uniting it with opium, for the purpose of lessening the morbid irritability of the system; and make the evacuant means chiefly local—employ local bleeding, and trust, perhaps, frequently much more to blisters, or applications of cold, than to local bleeding. Still, as I mentioned before, it sometimes may be a better practice, even when there is debility, to produce a momentary effect upon the system by causing fainting—by taking away a few ounces of blood suddenly in an upright posture. But, as a general rule, when patients are of very weak constitution, one would trust more to the local detraction of blood than to general. It often may be necessary in these cases, while you take away blood locally, or while you are producing syncope by the detraction of a few ounces, to give good nourishment. It is not at all inconsistent, when there is violent inflammation in a part, and little power in the system at large, to lessen the inflammation of the part by local evacuants, and by cold, and support the whole powers of the system by good nourishment; not perhaps by wine or porter, but by strong beef-tea, by milk, and other nourishing matters, although you do not give stimulants. Even in some cases it is necessary to give stimulants; but these are exceptions to a general rule.

Passive Inflammation.

These same remarks apply to what is called *passive inflammation*—that is to say, where a part is not actively inflamed, but rather in a state of congestion, and where the affection is not so much in the constitution as in the part itself. In inflammation of the throat, where the throat is very much swollen, and of a dingy colour, without active symptoms, and frequently in inflammation of the eyes, where there is more congestion than real active inflammation,—in these cases local means are by far the best. You do not want to produce an impression on the system, you want to unload the part—to remove the congestion of blood, and therefore local means are exceedingly proper. Even in such cases stimulating applications to the parts are useful; they are in a state of atony, and you may by stimulating applications, and by astringents, frequently remove the unhealthy condition even better than by mere local evacuations.

It is of the highest importance to make a correct diagnosis between an active inflammation, and one of an atonic kind, and a state which is hardly perhaps sometimes to be called inflammation. You will see after an inflammation has been very active that the symptoms will continue; but very much modified, with great debility, with great morbid irritability, and that the remedies of inflammation will make bad worse. It is necessary to remember, that, in almost every case of inflammation, this stage may arrive; and it is also necessary to remember, that this set of symptoms may happen in the first instance, bearing so great a resemblance to inflammation, that one not practised in it might fall into error. Now this particularly occurs in inflammation of the brain. You will find children displaying many of the symptoms of arachnitis, acute inflammation of the membranes of the brain, hydrocephalus acutus, as it is called, but without that disease; you will see patients labouring under sudden violent delirium, without any inflammation, or at least so little, that debility inducing morbid irritability is the prominent feature of the attack; and if in these cases you were to take away blood, you would most likely destroy life. The mode of distinguishing these cases is, by observing that there is no great pain, or if there be any pain it is slight and transient; in the next place the pulse, although it may be quick, is feeble; it is not a pulse which, upon careful observation, you would think justified you in resorting to depletory measures. Then, further, the surface is frequently by no means hot, or, if it be hot, it is only in a transient manner, and the expression of the face altogether is one of weakness. I shall have to speak upon this subject at greater length, when, under the consideration of par-

ticular diseases, I treat of hydrocephalus and delirium tremens. But in every case of inflammation, it is possible that after you have treated it actively, and subdued its active inflammatory nature, the patient may fall into a state of morbid irritability, and the local symptoms may still exist, though not of an active kind. You can only judge of it by finding that, whatever local symptoms there are, still the pain is not sharp; it is, if not gone entirely, much reduced; the pulse is one which will not justify you in bleeding; there is a degree of feverishness, so that the pulse may be very rapid, but still it is a pulse of irritability, one that may be almost extinguished by the finger, and you find an expression of feverishness altogether.

Now in these cases it is necessary no longer to go on with evacuant means; they are highly dangerous; but it is necessary to administer good nourishment—to administer stimulants, and sometimes to give opium. You will find such a case occur not only after excessive bleeding, but after inflammation has spontaneously subsided, the local symptoms may still continue in this form; and you will sometimes see this state of things at the very commencement. It is particularly seen in children, in spurious hydrocephalus, if I may so call it; and likewise in delirium tremens; in a spurious sort of phrenitis; not invariably, but in many instances. In such cases you will sometimes have even to give wine, and also brandy; but the utmost care is required; and it is right that you should know, for your own satisfaction in practice, that you will continually meet with cases where you cannot satisfy your mind how far active inflammation still exists—how far it is a mere case of irritation, where you may sit anxiously pondering over every circumstance, and be unable after all to determine what course to pursue. Now, in these instances, I should advise combination of both plans—that is to say, support the strength, give moderate stimuli with the greatest caution, and anxiously watch their effects; and at the same time employ evacuates locally, very moderately, lest you should be doing harm on the other side: adopting, in fact, a mixed plan, which is by no means inconsistent. After the use of any one thing, you should carefully observe the effects, and draw your conclusion from its action. For example, if you think it right still to apply leeches while you are giving nourishment, whichever appears the more beneficial you may be satisfied is the more proper, and that must be steadily pursued.

Chronic Inflammation.

With regard to chronic inflammation, its treatment must depend entirely upon the circumstance of its being active or passive; you have little to do with the duration of the complaint; you are not to consider whether it is acute or chronic, but whether it is

active or passive, and what are the powers of the patient. You will frequently have occasion to bleed in rheumatism for example, which has existed for a year or two. I have met with cases where, after rheumatism has existed two or three years, the parts have been so hot that it would have been in vain to attempt to cure them without local bleeding. Of course, when the disease has continued so long, the powers of the patient will seldom be very great, but you are not to consider what is *likely* to be the case, but ascertain what the patient's powers *really* are; what is the degree of activity of the disease, and what proportion it bears to the strength of the patient. In these chronic cases we should make use of chronic remedies. You can apply, not only leeches and cupping-glasses from time to time, but also means which are not serviceable in active inflammation—that is to say, issues, setons, and the application of caustic, so as to produce external ulceration. In acute inflammation, whatever is done must be done rapidly; but in the chronic form, we can apply setons or moxæ; we can burn the part, or apply caustic and repeated blisters, or keep blisters open—I do not know which is best; and we can also produce great irritation by the application of tartar emetic, in the form of ointment, or spread upon plaister. Any of these things may be in the highest degree serviceable.

Lunar Caustic.—Speaking of caustic, I should have mentioned that there is one form of it which is frequently of great use even in certain acute inflammations, and that is nitrate of silver. I believe in certain inflammations of the skin—of which, of course, I shall speak hereafter, the application of lunar caustic, wetted and rubbed on the part, or a strong solution applied with a brush, frequently is of great use. There is sufficient testimony in its favour, but I have not employed it extensively myself. In one form, however, of violent inflammation of the skin, I have made a circle all round the inflamed part—I have not touched the inflammation itself, but I have touched a certain portion all around, so as to blacken it; and I have seen it put a stop to inflammation which threatened to spread to a great extent. I think I have seen lives saved in this way—by inflammation being arrested, which in all probability would have run on to such an extent that it must have destroyed life. But of this hereafter.

You will, therefore, remember that in chronic inflammation you are to consider whether it is active or passive; whether the powers of the patient are great or small, without any regard to the period of its duration. At the next lecture I shall consider the treatment of the terminations of inflammation.

CLINICAL LECTURES,

Delivered in the Dispensary of the London University,

By DR. ANTHONY TODD THOMSON.

LECTURE VIII.—Dec. 16, 1831.

Asthma—Porrigo Larvalis—Epilepsy.

GENTLEMEN,—We have lately had several severe cases of Asthma before us, and the result of the plan of treatment has been so satisfactory that I have selected one of these cases, to afford me an opportunity of offering to you a few comments upon the nature of this distressing disease; and to place before you my reasons for adopting the peculiar line of practice, which you have seen usually followed in this institution, in the management of asthma.

The patient, Emily Cross, a stout woman, forty-eight years of age, was in good health until about a month since, when her stomach became disordered, and she felt languid and unequal to her ordinary occupations. On the evening of the 19th November, she was suddenly attacked with chilliness on the back, and, very soon afterwards, with wheezing, great difficulty of breathing, and a hard dry cough, which was relieved by expectoration. At four o'clock on the following morning, she was awakened with a severe sensation of suffocation, which forced her to sit up in bed; and this continued, accompanied with loud wheezing, laborious breathing, and dry cough, for nearly three hours, when the paroxysm gradually subsided, and terminated with a copious expectoration of a frothy mucus. The cough has, since then, continued all day, more or less, to tease her; she has been obliged to be propped up in bed; and the paroxysm of difficult breathing has returned every morning, nearly at the same hour. Her bowels have been regular, nevertheless she has taken, of her own accord, occasionally a dose of the Compound Colocynth pill, and has also applied a blister on the chest, but without obtaining any relief. Her pulse is now 104, and regular; the temperature of the skin is natural. She says that the urine is high-coloured, and deposits a pink sediment.

She was ordered to be cupped between the shoulders to the amount of eighteen ounces, and to take, immediately afterwards, eight grains of Blue Pill, with four grains of Extract of Cinium, to be followed by a brisk Cathartic. The following pills were also ordered:—

R Pilulæ Hydrargyri, gr. x.

— Scillæ, ʒjss.

Pulveris Opii, gr. xxx.

Simul contradantur et molis dividitur in pilulas xxx. æquales, quarum ij. 4ta quæque horâ, et iij. horâ somni quotidie sumantur.

On the 24th she stated that the cupping had greatly relieved her—that her breathing

had continued easier, and the morning paroxysms had been comparatively slight, but still they recurred at the same hour in the morning; the urine was as before; the tongue moist, but furred, and fiery red at the edges.

She was ordered a brisk Cathartic, and to continue the use of her pills.

As the cough was still troublesome, on the 1st of December a blister was applied between the shoulders, and her medicines were ordered to be continued.

She did not return to the Dispensary till the 8th, when she reported that the blister had greatly relieved the cough and difficulty of breathing. She could lie down in bed, and sleep more comfortably; and although the paroxysm of coughing still roused her in the morning, yet its violence was much abated, and she expectorates with ease. Her bowels were open, the urine was abundant and natural, and the pulse only 70, and a little tense. The tongue was clean, but still red.

Perstet in usum pilularum.

R Infusi Sennæ foliorum, ʒij.

— Cascarillæ corticis, ʒr.

Sulphurici Acidi diluti, ʒjss. M.

Sumantur Cochli. iij. majora bis in die.

To-day she is sufficiently well to be discharged.

This case, merely modified by degree, is the picture of all the others. I have named the disease *Asthma*. Let us first inquire how this is ascertained. Upon what diagnostic symptoms do we rest our opinion? The breathing is rendered difficult by many causes;—when these affect the structure of the lungs, or are organic, however, there are no intermissions; the spasmodic action of the respiratory muscles does not occur in paroxysms. Thus, if the difficulty depend on water accumulated in the serous membrane—if on emphysema of the lungs, on aneurism, or on any circumstance connected with diseased structure—we do not find periods of natural, or at least nearly natural, respiration, as in the cases you have witnessed; we do not perceive the hollow sound on percussion of the thorax which is heard in *Asthma*, nor do we find that the stethoscope affords no indication whatsoever of disease, either in the lungs or in the heart. When the paroxysm is absent, this instrument detects nothing like disease; the air enters freely into the bronchial tubes, and the movements of the heart are natural and regular. These circumstances sufficiently distinguish *Asthma* from cases of diseased lungs, and authorize us to regard the instances which we have lately seen as cases of that disease. It is true that the intermissions are not always well-marked; the spasms occur on the least movement, and the patient cannot lie down for weeks, and is forced to sleep in the sitting posture; but afterwards, the breathing again becomes natural and he lies down with facility. The premonitory symptoms in *Asthma* differ also from those

which characterise other diseases which produce difficult respiration: the attack is ushered in by languor, heaviness over the eyes, headache, drowsiness, and heat in the ears, neck, and thorax; or, if it be excited, as is often the case, by indigestion, the dyspeptic symptoms are always accompanied by an unusual sensation of stricture at the præcordia, and great irritability of temper. The paroxysms generally occur in the night, or rather early in the morning. The patient awakes with a feeling of suffocation, and, if it be a first attack, he throws open the window of his apartment for relief; or he sits erect, resting the hands firmly upon the bed, throwing back the head, in order to enable him to breathe by opening the mouth, elevating the shoulders, and depressing the diaphragm; which he feels to require a powerful effort, and to be absolutely necessary for his existence. He cannot speak, or at least he articulates with great difficulty, gasping between each syllable, whilst the attempts to speak are farther interrupted by an incessant cough. The face is swollen and purple: the eyes are prominent and sparkling; the veins of the neck turgid; and the whole countenance is expressive of anxiety and suffering. The feet, hands, nose, and ears, are cold; the pulse is quick and weak, but less irregular, although the paroxysm is sometimes attended with an intermitting pulse and palpitation of heart. After this disturbance and spasmodic state of the respiratory function has continued for an indefinite time, from half an hour to two or three hours, the spasms gradually abate, the breathing becomes more easy, inspirations require less effort, the cough assumes a loose or moist character, and the paroxysm terminates with a copious expectoration of thick mucus.

I have been thus particular in detailing the symptoms of the asthmatic paroxysm, both to enable you to compare them with those which the cases before us have displayed, and the descriptions of their sufferings given by the patients; and also to direct your attention to the affinity of this disease with those nervous affections which are connected with irregular action of the muscular system. What is the explanation of these symptoms?

You are fully aware that the stethoscope detects no structural disease; and nothing has been discovered by the postmortem examination of the few instances which admitted of this mode of inquiry; for as Asthma is not a mortal affection, it seldom affords the opportunity for such investigations; and when death occurs, it is generally the result of some concomitant disease. In two instances only, as far as my information extends, the brain and spinal marrow have been examined; and it is not improbable that the softening of the cerebral substance, and the effusion of blood into it, noticed in one of these, ought to be

regarded as the consequences rather than the causes of the disease. Indeed, it is easy to conceive that the violence of the cough, and the repeated disturbances in the respiratory function, are most likely to produce both cerebral and thoracic diseases; thence we find asthma occasionally terminating in apoplexy, in chronic pulmonary catarrh, tubercles, vomica, emphysema, aneurism of the aorta, and hydro-thorax; and as these diseases supervene, the convulsive or spasmodic character of the malady abates. If asthma, therefore, be a functional disease, it must depend on some peculiar state of the nervous system; and as the treatment of the disease which you have witnessed here is founded upon that opinion, I will endeavour to explain to you my ideas upon this subject.

On looking into the anatomy of the respiratory organs, we find that not only the nerves which associate the muscles of the chest in the act of respiration, but those also which are distributed in the bronchial tubes, and most probably terminate in the cells themselves, arise from that medullary tract which descends from within the cranium downwards on the spinal marrow," betwixt the sulci that give rise to the anterior and posterior roots of the spinal nerves. The two most important of the nervous cords, arising from this tract, concerned in the action of the lungs themselves, in that of the heart and that of the diaphragm, are the *par vagum*, or *pneumogastric*, and the *phrenic nerves*. The former supplies the pulmonary plexus, carrying extreme fibrils along the bronchial tubes into the cells, at least we have reason to think so from the microscopic observations of Reisseisen; and from the same source, also, we may conjecture that the cells themselves possess a muscular character. You know that the heart is likewise supplied with nerves from the *par vagum*, which also, embracing the œsophagus, furnishes the gastric plexus. The latter, or phrenic nerve, is intended solely for regulating the action of the diaphragm, and, in conjunction with the other respiratory nerves, which associate the actions of the muscles of the chest, in elevating the ribs and expanding the thorax, and contracting the diaphragm, may be considered as the antagonists of those which are supplied to the bronchial cells. This becomes obvious, if we reflect, that the effect of a stimulus upon these is to produce such a contraction of the air-tubes and cells, as will aid the expulsion of the air which is necessary to be expired; and it is not until the relaxation consequent to this contraction takes place, that the phrenic and other nerves act, to expand the chest, and permit the air again to rush in and fill the bronchial cells. The act of inspiration, when circumstances occur to disturb the breathing, is a powerful voluntary effort; whilst that of expiration is nearly passive, or only under the influence of volition within a certain limit. Now, what are the inferences to be drawn from

these premises in explaining the paroxysm of Asthma? Before entering upon the consideration of this question, it will be proper to settle our opinions respecting the exciting causes of the paroxysm of Asthma.

The exciting causes of Asthma are of two kinds, physical and mental: to the former we refer sudden alternations of temperature; fogs; indigestions, especially such as induce flatulence, or which much increase the bulk of the stomach; irritations of smoke; and some odours in individuals of peculiar idiosyncracies: to the latter, we refer certain affections of the mind. The most frequent, and perhaps the most powerful of these exciting causes, is indigestion: but before this can produce asthma, there must exist a predisposition to the disease,—a susceptibility of impression on the nerves supplying the bronchi, which we frequently observe to be connate or hereditary, but of which we have no correct idea. Whichever of these exciting causes operate, it is undoubted that the impression is made on the nervous system, and that Asthma is as much a nervous affection as Chorea, Hysteria, or Hypochondriasis. Let us now recur to the query;—in referring to the functions of the respiratory nerves,—in what manner does the influence of these exciting causes produce a paroxysm of Asthma? I can only reply by an hypothesis. If we suppose the paroxysm to follow a fit of indigestion, the following is, in my opinion, the most probable manner in which it is produced. The morbid impression made by the contents of the stomach on the gastric plexus, is communicated to the bronchial nerves, and excites spasmodic action, of a clonic description, in the muscular fibres of the bronchial cells and minute branches of the tubes, so that the whole of the air contained in them is expelled. The natural consequence of this must be a sensation of suffocation, to relieve which, a violent voluntary effort is made to expand the chest; this, however, is opposed by the spasm of the air cells of the lungs, preventing their expansion, and the formation of a vacuum between their external surface and the ribs, which would be the necessary consequence of the dilatation of the cavity of the thorax, could it be effected whilst the lungs remain contracted. Thence the laborious efforts at inspiration; and, whilst the spasm continues, the whole phenomena of the paroxysm. The continuation of the effort of inspiration gradually exhausts the state of morbid excitement, which is consequently followed by a state of collapse; and, in this relaxation, the mucous membrane of the bronchial tubes pours out a large quantity of mucus, which is ejected by coughing,—the usual method adopted by nature to relieve the chest of every excess of its proper secretion. Mental causes operate nearly in the same manner in those predisposed to Asthma, primarily affecting the stomach, and secondarily the moving fibres

of the bronchial cells, producing spasmodic action in them; which, in others of a different predisposition, would have been set up in other parts, and been productive of Chorea, Hysteria, or other diseases of irregular action.

The nerves affected in Asthma are connected most intimately with the state of the circulation; and, thence, the irregularity of pulse which sometimes accompanies the paroxysm. The spasm upon the bronchial tubes and cells impedes the passage of the blood through the lungs, and the sensation of suffocation which results, is a natural consequence of the blood retaining a great part of the carbon from which it is freed by the respiratory function. The repeated and fruitless forcible efforts to effect a full inspiration, in truth, constitutes the disease. With respect to the influence of the stomach, we are well aware of the powerful influence of direct irritations of the nerves referred to on the whole respiratory apparatus; and, it is not less true, that, from the close connexion of the stomach with these nerves, irritation on that viscus produces indirect effects upon them nearly equivalent to direct impressions resulting from injury to the lungs; and, therefore, the great importance of attending to the state of the stomach, both in explaining the attacks of Asthma and the resistance of the disease to remedies, when that is overlooked.

If I have made myself understood in these remarks, you will have little difficulty in comprehending the various steps of the treatment which you have witnessed; and which has succeeded in relieving the persons who have presented themselves, labouring under Asthma, at this institution. I do not say cure, for the old remark—that it is impossible to cure Asthma, still remains in full force. The difficulty seems to depend on the predisposition being connected with the original conformation of the individual, and consequently unchangeable. In all the cases which you have seen here, cupping between the shoulders was prescribed; and you have heard from the patients how greatly their breathing has been relieved by it. From your knowledge of the origin of the affected nerves, you will have little difficulty in referring this relief to the cupping, acting as an immediate and powerful counter-irritant, rather than to the quantity of the blood abstracted. It is, nevertheless, true, that bloodletting, by removing congestion of the lungs, generally affords temporary relief in Asthma; but it is, certainly, far short of that obtained from cupping over the cervical and dorsal portions of the spine. On the same principle, and to avoid lowering the habit by a repetition of bloodletting, blisters have been applied; and purging has also been resorted to as a counter-irritant with much advantage. In some of the cases, which have been accompanied with a dry skin and slight febrile symptoms, the

compound powder of Ipecacuanha, in nauseating doses, has proved beneficial, not only in restoring the balance of the circulation, but in aiding expectorants in unloading the bronchial tubes of their overabundant mucus. With regard to the kind of expectorants to be employed, you have seen these varied in almost every case: where any thing like fever has existed, Ipecacuanha has been selected; and where the habit was languid and the pulmonary exhalants required to be stimulated, every thing has been derived from combinations of Squills and Ammoniacum. Dr. Cullen has extolled the *fœtid Gum Resins* in such cases; but I have scarcely ever found it necessary to order them, the Squill and Ammoniacum having answered every intention; or, where more stimulus has been demanded, I have generally relied on the Carbonate of Ammonia. Where the languor was not so great, and a tonic rather than a stimulant was indicated, whilst at the same time the expectation required to be attended to, a combination of Nitric Acid, Tincture of Squills, and Henbane, has answered well; and where expectorants have not been indicated, an equally good effect has resulted from Sulphuric Acid, with Senega, or some other bitter. Tonics in the intervals of the paroxysms, in conjunction with light diet and regular exercise, are the best means of allaying the morbid mobility of the system; and if any thing can effect such a change, they present the best means of overcoming the predisposition to the disease. Too little attention is usually paid in regulating the exercise of Asthmatic patients; this should be strictly forbidden after a full meal, as it invariably produces an anxious, agitated, and difficult respiration; and therefore, in the class of patients that present themselves to us, and who must return to labour immediately after dining, the importance of eating sparingly should be strenuously urged. "I must eat to enable me to work," is the reply in these cases; but you must point out to the patient, that the same disadvantage arises from overloading the stomach as from starvation; in the one case no strength follows, because the oppression destroys the powers of digestion; in the other there is nothing to digest; but in both the effect is the same. With respect to exercise at other times, the Asthmatic who is wholly regulated by his feelings is too apt to shrink from it, on account of the uneasiness which it occasions; but this feeling he must be taught to disregard; and by encountering it with firmness, he will gradually familiarize himself with the feeling, till he at length gains the ascendancy over it, and profits by the effort. In the middling and higher ranks of life, nothing tends more effectually to lessen the tendency to the returns of the Asthmatic paroxysms, than the use of the shower-bath.

Porrigo Larvalis.

The great number of cases of cutaneous eruptions which present themselves at the Dispensary, furnishes you, gentlemen, with excellent opportunities of studying this class of diseases. It is my intention, occasionally, to bring them particularly before you. The case of Porrigo which you have just seen, and which has so rapidly yielded to the remedies prescribed, is a striking demonstration of the facility of managing that formidable-looking eruption. The patient, a boy of fourteen years of age, presented himself at the Institution only six days ago, and is now fit to be discharged as cured; but I retain him to prevent too immediate a return to the diet and habits to which I ascribe the disease. Owing to the poverty of his parents, he has been scantily nourished; his food also has been of the most indigestible kind; he has, besides, been daily purged with salts, on the supposition of humour being present in the blood; so that the poor boy was in a very reduced state. When he presented himself at the Dispensary, the thick, honey-comb crusts, which follow the psydriacous pustules, surrounded the mouth, covering nearly the whole of the lower part of the face; and the pain of opening the mouth was so great, as to prevent him from putting out his tongue. A few crusts were also scattered over the forehead and the scalp, and two or three blotches on each arm. The countenance was pale and sallow; the expression anxious. He complained of gnawing pain at the stomach; and was much distressed with sour eructations.

As this eruption is altogether dependent on the state of the stomach, the indications of practice are chiefly directed to correct the deranged condition of that viscus, and gently to stimulate the diseased surface. With this view, ten grain doses of the Hydrargyrum cum Creta, with a quarter of a grain of Opium, were directed to be taken at bed-time every night: and a powder, consisting of a scruple of Carbonate of Soda, five grains of Rhubarb, and a scruple of powdered Columba root, twice a-day, with a generous, but light animal diet. The crusts were ordered to be smeared with the diluted ointment of Nitrated Mercury, night and morning. In three days, the crusts fell off, leaving a red, rough surface, which is now assuming a more healthy aspect, and the dyspeptic symptoms are rapidly disappearing. The object of the alkali, in this case, is not to neutralize the acid, but to allay the irritability of the stomach, and thence to enable it to prepare a more healthy gastric juice. The result of this practice, both in the case under consideration, and in similar cases, which you have witnessed, is well calculated to inspire you with confidence in this mode of treating this disgusting eruption.

Epilepsy.

The man who had been discharged as

cured of this disease, has again presented himself to the Institution, and states that he had three severe fits two days ago: his face is cut from the fall caused by the fit, which came on in the street. On making inquiry into the cause of this recurrence of the disease, I am persuaded that it is to be attributed solely to intemperance, notwithstanding the positive assertions of the man to the contrary. Whilst he was affirming that he had not tasted spirits, for many weeks, his breath smelt like a gin-bottle; and consequently every confidence in his account of himself was at an end. Nothing is more difficult than to get at truth, in such cases; and you must exercise a sound discretion in ascertaining the nature of exciting causes amongst this class of the community. Since this discovery, I have little expectation of being able to cure the disease in this individual.

IDENTITY OF SMALL-POX AND COW-POX.

To the Editor of the London Medical Gazette.

SIR,

THE excitement produced by the irruption of cholera having in some degree subsided, you will perhaps permit me to refer to a paper which appeared in your journal of the 5th November, purporting to be an account of the experiments of Dr. Sonderland, of Bremen, to determine the identity of small-pox and cow-pox. With this I would beg to couple a letter from Dr. Baron, of Gloucester, published in the Medical Gazette of December 3, in which the same question is discussed. From the perusal of these communications, a superficial reader could hardly rise without the conviction, that the identity of small-pox and cow-pox was hereafter to be viewed as an undoubted truth—a perfect *axiom* in pathology. My object in this letter will be to sift the facts and arguments of these gentlemen somewhat in detail, and to shew that much still requires to be done before this question can be laid at rest.

Dr. Sonderland's observations are given "*almost without abridgment*;" yet we are left in the dark as to *where* and *when* those experiments were conducted, which are said to have thrown so much light on the theory of small-pox and cow-pox. We are left to guess whether the experiment of infecting cattle with variola has been performed once, twice, or oftener—whether the disease thus produced is fatal, or otherwise, to the animal—whether, when

thus artificially produced, it spreads by contagion. Our imagination is taxed to discover whether the vaccination of children from this singular source has been performed once only, or so often as to preclude all reasonable doubt—whether the vesicles thus produced present any, and if any, what modifications of their normal character; and lastly, whether the lymph thus generated, has, or has not, been kept up by successive inoculations. It is surely, sir, not too much to expect, in a matter of so much importance, that these questions should be satisfactorily answered before we are called upon to *give in our adhesion*. The whole affair seems to me to savour very strongly of *romance*. We are told, for instance, "that a bed-cover impregnated with variolous contagion, if firmly rolled up, then wrapped in linen, afterwards in paper, then duly packed in a bucket, and carefully put by in a cool, shady place, with the thermometer between 32 and 52 degrees, will retain the contagion in an active state for at least two years." Now, sir, I would venture to ask, how many bed-covers must have been rolled, wrapped, packed, and stowed away, before this dogma in epizootic pathology could have been concocted? Dr. Sonderland, in this paragraph, seems to me to prove a great deal too much, and by so doing throws discredit upon the whole of his statements. The deductions of Dr. Sonderland from these alleged facts are miserably lame and impotent; in truth, it is only the first and third of them which in any way invite our attention. To these Dr. Baron has given a degree of importance, by putting in a *counter-claim* for Dr. Jenner. How far it is prudent to mix up the revered name of Jenner with a theoretical question, involved in so much doubt as the identity of small-pox and cow-pox, it is not for me to decide; but as Dr. Baron has put forward certain statements in reference to this question, it is proper to examine them, and, as far as possible, estimate their true value.

Dr. Baron states confidently (Medical Gazette, vol. ix. p. 301), "*that cows in many parts of Europe have long been known to be liable to small-pox.*" That cows are liable to a disorder which by some persons is *believed* to be identical with small-pox, is most true; but that this disease has been ever *proved* by *unquestionable* experiments to be genuine variola, I must continue to doubt. Analogously, the ascertained peculiarities of the

ford, indeed, presumptive evidence; but such evidence may be satisfactory to some persons, and very unsatisfactory to others. Nothing short of the *direct* transmission of the disease from man to animals, and from animals back to man, can (in my mind) justify the position, that *cows are liable to small-pox*; and where, may I ask, are such experiments recorded? Dr. Sonderland states (and here I believe him to be quite correct), that, "although many had suspected the identity of small-pox and cow-pox, no one had previously decided the question experimentally beyond doubt." The *inoculation* of cows has never been satisfactorily proved. Whatever may have happened to this or that observer, it is perfectly well known, that no person can at pleasure communicate the cow-pox to a cow by inoculation. I am free to admit, that Dr. Sonderland's experiments (if correct) have settled the question; but I cannot consent to build a superstructure of such vast extent and importance, upon so insecure a foundation as the experiments of a single individual, miserably deficient in details, and overloaded with theoretical deductions.

The second, third, and fourth positions of Dr. Baron (*Medical Gazette*, *loc. cit.*), in their bearing upon the question at issue, are contingent upon the first. Unless the disease of cattle therein alluded to be genuine variola, it matters not (so far as this question is concerned) whether it be, as Dr. Baron contends it is, a contagious, innocuable, and still prevalent disorder. A correspondent, under the signature of Non Nemo (whose style of writing savours very strongly of my old acquaintance, M. D. Oxon), corroborates all that Dr. Baron has affirmed; and adds, that the evidence in favour of the identity of small-pox and cow-pox, to be found in the fifth chapter of Dr. Baron's *Life of Jenner*, appears to him to be as conclusive as any thing of the kind can be. I regret that I cannot join in this opinion, though I have studied that chapter most diligently. For Dr. Baron's talents and professional attainments, and zeal in one of the best of causes, I entertain the highest respect; but he appears to me to attach an importance to the determination of this question which is scarcely warranted. The *practice* of vaccination would neither be promoted by the general adoption of Dr. Baron's views, nor retarded by

disease, and epizootic history (the three points upon which Dr. Baron relies), after their abandonment. The hypothesis is ingenious, but the importance of *demonstrating* it may be estimated from this single consideration—that vaccination has been practised for thirty-two years, and has established itself in every region of the earth—*et adhuc sub judice lis est*.

I have the honour to be, sir,
Your very obedient servant,
GEORGE GREGORY.

31, Weymouth-Street,
Dec. 22, 1831.

CASES OF STRICTURE PERFORATED BY THE LANCETTED STILETTE.

*To the Editor of the London Medical
Gazette.*

SIR,

IF you consider a few of such cases as the two following, where permanent strictures of the urethra have been perforated by the lancetted stilette, worthy of your notice, I shall occasionally send you some for publication, and you will oblige me by their insertion in your valuable journal.—I am, sir,

Your obedient servant,
R. A. STAFFORD.

28, Old Burlington Street,
Dec. 21st, 1831.

CASE 1st, August 1830.—L. SNOWDON, æt. 40, a printer*, applied to me with a diseased urethra, which began to be contracted at one inch from the meatus urinarius, and extended nearly two inches along the canal. Externally the part was extremely indurated; and when pressed between the finger and thumb, it felt as if a piece of small cord had been lodged in that portion of the urethra. From the patient's account, the disease had originated in consequence of a very virulent gonorrhœa, which he had contracted three years ago, and from which time the passage at that part had gradually narrowed, until it became so closed as only to allow of the urine passing through it guttatum. In the early stage of the disease bougies had been passed; but for the last two years no instrument whatever could be got through the stricture. The poor fellow was much emaciated, and appeared to have suffered considerably.

As my friend, Dr. Eckström, professor

* One of the compositors on the Medical Gazette.

of surgery at Stockholm, was very desirous to see the treatment, I requested him to examine the case. He was of opinion, from the great extent of disease, its situation in the urethra, its indurated structure, and its impermeability, that no other treatment could, with success, be adopted than its perforation by the lancetted stilette. I accordingly at once cut through in his presence about half of the strictured portion, leaving a bougie in the divided part for one night, to keep it open. On the following day it was withdrawn, and introduced daily. In a week from the first operation, I completed the division of the part, and introduced a No. 9 catheter into the bladder. Steel dilators were now passed on the alternate days until they arrived at the size of No. 11. The patient, finding himself nearly well, discontinued his attendance on me. I have since heard he is quite recovered, and that he makes his water in a natural sized stream. The treatment occupied from three weeks to a month, and during the whole time no unfavourable symptom whatever occurred.

REMARKS.—From the preceding history of this case, it may be observed that two inches of the urethra was irregularly thickened, and contracted to such an extent that the urine could only infiltrate itself through it guttatim. The part, of course, could not be dilated, for it was impermeable to a bougie. The extent of the disease also prevented the application of caustic, for it would have been almost impossible to have burnt through so long a contraction with this substance. The division of the part externally, likewise, (if ever to be recommended,) would in this instance, from the situation of the disease, have been of no service, and most probably attended by serious consequences. What treatment, then, was left for the poor sufferer? None of the above could have given him any relief. In course of time the urethra would have entirely closed up at that part, and then ulceration behind the stricture would have taken place, giving rise to extravasation of urine, with fistulous passages; or he would have lingered on, as many others have done before him, and at length would have been worn out by the disease, and have died. It will be seen the simple way by which the employment of the lancetted stilette

permeated an obstruction which could not by any other means have been overcome.

CASE II.—A naval officer, æt. 60, consulted me in Aug. 1830, under the following circumstances. He had been the sufferer for many years, of an impermeable stricture, situated immediately behind the bulb, at the entrance of the membranous portion of the urethra. He had placed himself under the care of various surgeons of eminence, who had tried all the known means, excepting external division, without success. As a last resource, and as otherwise he must have refused a valuable naval appointment, he was desirous of having the stricture divided by the lancetted stilette. The case was by no means a favourable one for the operation; for after each introduction of the bougie, hæmorrhage, to the amount of from half a pint to a pint, always occurred. Having had, however, another case very similar to it, where the bleeding ceased immediately after the incision, I consented to operate, more particularly as his nephew, who was a surgeon, was to be present. On the first day about half an inch of the obstruction was divided, and the hæmorrhage certainly was less than when a simple bougie was passed. On the sixth day from the first incision I introduced the instrument, and made another, when it slipped on with the greatest ease into the bladder; but little blood followed. A No. 9 catheter was now passed on the alternate days. Six days after the operation he was attacked with a continued fever, which he had been liable to before, in consequence of living on the banks of the Thames. He was removed from that situation, but the fever ran on for a month. During this period I did not attempt to pass instruments, but the stream of urine was all the time nearly as large as natural. When he was convalescent I introduced a bougie, but I could not, in consequence of spasm, get it into the bladder. He was recommended, therefore, to go into the country to recover his health, and he paid a visit to his brother-in-law, who was a surgeon. After a little time he improved so greatly, that this gentleman twice passed a No. 9 catheter. Having accepted the naval appointment, and his time being expired, he sailed for Jamaica. Since his arrival I have heard

from him. He makes water freely, and can pass a No. 9 catheter with ease.

This case is also one of great interest, for, independently of every remedy being employed without effect, excepting external division, it shows, even where the stricture is disposed to be hæmorrhagic, that an incision into it rather diminishes than increases this tendency. The same happened in a case which came under my care before. When a bougie touched the stricture, the hæmorrhage was terrific, but immediately after the part was divided, it ceased altogether. I shall not attempt to offer an explanation of this phenomenon. It is possible that the strictured part is of that vascular nature, that the least abrasion of its surface, as in some hæmorrhagic ulcers, makes it bleed. A clean incision, however, through it, allows the mouths of the vessels to contract, and thus hæmorrhage is prevented.

IS PUERPERAL FEVER INFECTIOUS?

To the Editor of the London Medical Gazette.

SIR,

THE letter of Dr. Campbell on puerperal fever, which appeared in the Medical Gazette of December 10th, has recalled my attention to certain facts in my possession, calculated to throw some light on the query which heads this communication. At the present moment, when the laws of infection are the subject of eager and dubious controversy, the facts I shall adduce may possibly interest some of your readers.

"On the question of infection (Dr. Campbell observes) I am as much as ever impressed with the belief, that unless the practitioner has been engaged in the dissection of the bodies of those who have fallen victims, the disease cannot be conveyed by him from females labouring under it to others recently delivered; but if he have been so engaged, I have strong reasons for believing that he may be the means of propagating it." My experience is not in unison with this conclusion. On the 4th of January last, (1831) a meeting of the medical officers of the Manchester Lying-in Charity was summoned in consequence of a great mortality having occurred, during the four preceding

weeks, among the patients of one of the midwives. The circumstances we found to be these: Mrs. A. B., a midwife in great practice among the patients of the Charity, had on the 4th of the preceding month (Dec. 1830) delivered a poor woman, who soon died with symptoms of puerperal fever. From this date to the 4th of Jan. inclusive—exactly one month, this midwife delivered thirty women residing in different parts of an extensive suburb, of which number sixteen caught the disease, and all of them ultimately died. These were the only cases of puerperal fever which had for a considerable time occurred in Manchester. The midwives, commonly twenty-five in number, deliver, on an average, ninety women per week, which is about three hundred and eighty in a month. Now of this number delivered during the month in question, none had puerperal fever except the patients of Mrs. A. B. Yet all this time this woman was crossing the other midwives in every direction, scores of the patients of the Charity being delivered by them in the very same quarters where her cases of fever were happening.

The following statement, transcribed from Mrs. A. B.'s day-book, specifies the dates of the deliveries of the thirty women, discriminating those who took the fever:—

	Deliveries.	Had Puerperal Fever.
1830, Dec.	4 one	one
	5 one	—
	6 two	one
	7*four	one
	18 three	two
	22 one	—
	23 one	one
	24 one	one
	25 two	two
	26 two	—
	28 one	one
	30 two	one
	31 one	—
	1831, Jan. 1 four	two
	2 two	one
	3 two	two
	thirty	sixteen

* Here an interval of ten days elapses without any deliveries having taken place—a circumstance which I can only account for by supposing, what is probable, that the midwife, in alarm, voluntarily suspended her operations.

The decision of the medical officers of the Charity was to the effect that Mrs. A. B. should abandon her practice for a short period, and go the country. In a short time after this meeting, cases of puerperal fever among the patients of other midwives, as well as in private practice, began to appear in various parts of the town. In the course of the spring months a great number of women died of this fever. It never prevailed more generally, nor perhaps ever more fatally, in Manchester. By about the beginning of June it had disappeared.

The fact that sixteen cases of puerperal fever occurred in one month in the practice of a single midwife, while the patients of the other midwives were exempted from the disease, leads naturally to the conclusion that this midwife was the *medium* of communicating (I take not upon myself to say *in what manner*) the malady from one woman to another—from one affected with the fever to another in health; for the reader will observe that the midwives always visit their patients during the first three days after delivery, if doing well; and for a longer period, if sick. Again, little more than half of the thirty women delivered by this midwife during the month before mentioned took the fever. On some days, all the women she delivered escaped; on other days, out of three or four, one or more of them were seized. This is no way opposed to what is observable in the career of other infectious maladies, and may be explained by assuming that there is a difference in different women, and perhaps in the same women at different times, in regard to predisposition; that the fever was occasionally conveyed *directly* from the diseased to the whole, I possess other evidence than I have stated. In one instance, within my knowledge, a practitioner introduced the catheter, in the case of a poor woman labouring under puerperal fever, late in the evening; and in the course of the said night he had to attend a lady in her confinement a little way in the country. On the morning of the second day after delivery, this lady had a violent rigor, and the other early symptoms of the malady. In another instance, a surgeon was called while in the act of inspecting the body of a woman who died of this fever, to attend a labour: within forty-eight hours after being put to bed the woman was seized with the fever.

That, besides being infectious—that is, capable of being conveyed, in some tangible medium, from one woman to another—this disease is propagated by some cause of a more general kind, probably existing in the atmosphere, *after the fever has prevailed for some time in a locality*, cannot be doubted. Numerous cases occurred during the late epidemic in Manchester; the origin of which could not, I apprehend, have been traced to infection properly so called.

The morbid appearances in our epidemic, resembled, in most respects, those mentioned by Dr. Campbell, as noticed by him in that of Edinburgh. Phlebitis, although diligently sought for, was not, that I am aware of, detected in a single case. Traces of inflammation of the peritoneum, generally slight, sometimes severe; inflammation of the pleura, with effusion of serum into the chest, and softening and disorganization of the ovaria, were the usual appearances.—In one case, which I inspected, there was great enlargement, thickening, and remarkable softening of the uterus, with other appearances indicative of putrefaction, such as greenness over the lower abdomen, about the pudendum, on the inside of the thighs and arms, and separation of the cuticle, as if it had been detached by a blister. This was the state of the body, exactly 25 hours after decease. The ovaria, I ought to have mentioned, resembled masses of venous blood. The patient, a woman of great vigour, was in perfect health on Saturday; fell in labour on Sunday; in the course of the night, while in labour, had a violent rigor, and began to complain of tenderness in the abdomen; was delivered early on Monday, and died on Tuesday at noon. I am not aware that there were any other cases, during the epidemic, with symptoms and appearances similar to those of this woman.

The cases of puerperal fever, according to my observation, were resolvable into three classes: first, those, the most numerous certainly, in which no medical treatment was of avail where the pulse was 140 and upwards, resembling, in the most striking manner, the pulse when rupture of the uterus has taken place in labour; and where the heat of the surface never rose to the natural standard. Second, those where leeching, calomel and opium, blisters, and

other counter-irritants were indicated, and occasionally proved successful.—Third, those cases in which bleeding by the lancet, owing to the complete development of the heat of the body, the acuteness of local pain, and the distinctness and comparative strength and hardness of the pulse, was clearly indicated; and which, when employed early in this variety of the disease, was almost uniformly successful. The latter class of cases became more prevalent towards the decline of the epidemic. During the first two months of its prevalence, on the contrary, very few cases recovered. In consequence of the *kind* of persons attacked by the puerperal fever—mothers of families—the ravages of the disease are in the highest degree astounding and deplorable. Those practitioners who have encountered the progress of this epidemic need not fear that they will find in the aspect and mortality of any other disease, although it may be of more famous name, features more formidable and success more disheartening.

Your's respectfully,

JOHN ROBERTON.

Manchester, Dec. 15th, 1831.

MODUS OPERANDI OF OIL OF TURPENTINE

IN INFLAMMATION OF THE EYE.

To the Editor of the London Medical Gazette.

SIR,

THE following question—What is the *modus operandi* of oil of turpentine in deep-seated inflammation of the eye? arises from a case which I had lately under my care, and in which I had the assistance of an eminent surgeon; it is briefly this:—

A gentleman, 30 years of age, rather dissipated, who had suffered severely for many years from strictures in the urethra, consulted me for a hemorrhage from the bladder, the result of what he called violent but ineffectual efforts to pass urine. Under the treatment usual in such cases the hemorrhage gradually subsided, the urine flowing freely as the irritation diminished. He now complained of pain and uneasiness in the right eye, which increased so rapidly as

to require the most active measures: bled to $\mathfrak{z}\text{xvi}$. On the following day cupped to viij ., and ordered to take two grains of calomel, and a quarter of a grain of opium, every four hours. Third day inflammation still increasing, with intolerance of light, cupped to $\mathfrak{z}\text{viij}$., and directed to take the calomel and opium every two hours. Fourth day inflammation less; pain easier; medicine continued; applied the unguentum hydrargyri to the temples, and belladonna to the superior palpebra. Fifth day mouth very sore, eye quieter, inflammation considerably diminished, but vision gone. The mercurial action being now well established, the pills were ordered to be taken at longer intervals. Vision had begun gradually to return, when he imprudently exposed himself to cold, which brought on a relapse more severe than the original attack. Prompt and vigorous treatment were now necessary to prevent the total disorganization of the eye; and which happily was effected, but at the expense of extreme debility. During all this time he passed his urine freely, though previous to this attack he was obliged to have daily recourse to strong opiates; half an ounce of tincture of opium was his ordinary dose in severe spasms. Vision of the right eye was now completely restored, when, unfortunately, the left eye became affected, the inflammation being, as near as possible, similar to that in the right. The previous treatment had exhausted him so much, that general blood-letting was considered inadmissible; and it was even a question how far he could bear the loss of a few ounces by cupping. Eight ounces were taken from him, which reduced him to such a state as to prevent a repetition of it. The state of his mouth, and the irritative fever under which he laboured, forbade the further employment of mercury. Inflammation and effusion increased so rapidly as to cause complete loss of sight. Under these circumstances we had recourse to the oil of turpentine, in $\mathfrak{z}\text{ij}$. doses twice a day in some mucilage; increased the dose on the second day to $\mathfrak{z}\text{ij}$. Third day urethra a little irritable; inflammation less; eye quieter, with returning vision; medicine continued. Fourth day bowels sharply affected; ordered to take only $\mathfrak{z}\text{ij}$. twice a day, and drink freely of mucilaginous liquids. Continued under this plan of treatment for

about a week, when vision was so far restored as to render my attendance no longer necessary. The oil, which he bore very well, was continued for a few days longer, when the eye seemed perfectly restored, there remaining only a little irregularity in the pupil. I have made no remark on the state of his bowels; it is sufficient to say that they were properly attended to.

The question then is—Did the oil of turpentine effect in the left, what the calomel and opium had in the right eye?

I remain, sir,

Your obedient servant,

RICH. BURKE, M.D.

4, Bolton-Row, Mayfair,
Dec. 6, 1831.

BOTANY OF NEW ZEALAND;

BEING A

Description of Trees, Plants, &c. indigenous to that country.

By GEORGE BENNETT, Esq. M. R. C. S. &c.

[With Engravings.]

Myrtus Bullata, BANKS and SOLANDER'S MSS.

Era-mára-ma of the natives of NEW ZEALAND.

Natural Family, *Myrtaceæ*.

THIS tree, curious from its bullated leaves, is of a slender growth, attaining the height of from ten to fourteen feet: it is named *Era-mára-ma* by the natives of New Zealand. It is as yet undescribed, but is correctly figured, both in flower and fruit, in Parkinson's Botanical Drawings, made during Cook's First Voyage, among the collections of Sir Joseph Banks, now deposited in the British Museum. The leaves are ovate, alternate, with a reddish tinge, and bullated. The flowers are of a white colour, axillary, and opposite; the fruit is a small red berry. It is found growing abundantly in New Zealand on the declivities of hills, in good soil, and shady situations. The accompanying sketch of a branch in flower and fruit will show its general appearance, drawn from a recently gathered specimen at New Zealand.—(See opposite page.)

Kai-kea tea of the natives of NEW ZEALAND.

Probably a *Dacrydium* Species.

Natural Family, *Coniferæ*.

From the specimens collected, this coniferous tree is supposed to be a species of *Dacrydium*: it is named *Kai-kea tea* by the natives of New Zealand, and attains the height of from 120 to 130 feet, being the loftiest timber tree in New Zealand; it is in circumference from twelve to sixteen feet; the quality of the timber is not good for spars, on account of its softness and liability of splintering, nor for plank, on account of its warping and deficiency of durability. A gum-resin, of a reddish colour, exudes from this tree, which is used as a masticatory by the natives, similar to that of the *Kowri*. This tree yields a small red berry, named *Koroe* by the natives, having one hard seed; the berries have a sweet taste, and are eaten by them. The common canoes are made from this timber, its great length enabling the canoes to be constructed of a large size, for carrying provisions, &c. &c.

Káwaka of the natives of NEW ZEALAND.

A tree of the Natural Family *Coniferæ*, collected without either flower or fruit: it is named *Káwaka* by the natives of New Zealand, attaining the height of from 60 to 70 feet, and from 8 to 10 feet in circumference. The timber is of a red colour, and of an excellent quality for either plank or spar. No gum-resin is produced by this tree. The natives informed me that it derived the name *Káwaka* from the branches growing out regularly on each side of the tree.

Merista lavigata, BANKS and SOLANDER MSS.

Tipau of the natives of NEW ZEALAND.

THIS tree is as yet undescribed, but is correctly figured in Parkinson's Botanical Drawings, made during Cook's First Voyage, among the collections of Sir Joseph Banks, now deposited in the British Museum. It is the *Merista lævigata* of Banks and Solander, and named *Tipau* by the natives of New Zealand; attaining the height of from

16 to 20 feet, usually growing straight, but small in circumference: the wood has a reddish grain, is hard and heavy, but is seldom used by the natives, its small circumference not rendering it of

any utility, except as poles. The trees grow on both high and low land, in good soil. The leaves are of a light green colour above, whitish underneath.



Myrtus Bullata.

Metrosideros excelsa, SOLANDER MSS.
Pohu-tukawa of the natives of NEW
 ZEALAND.

Natural Family, *Myrtaceæ*.

Class, *Icosantria*. Order, *Monogynia*.

This is an unpublished species of *Metrosideros*, the *M. excelsa* of Solander's MSS. and is figured in Parkinson's Botanical Drawings, made during Cook's First Voyage, deposited in the British Museum: it is named *Pohu-tukawa* by the natives of New Zealand, and is found growing on both elevated and low land, but more commonly in the vicinity of the sea. It is the New Zealand oak of Europeans, and is very irregular in its growth, never growing straight. It attains the height of 60 or 70 feet, and in circumference from 10 to 14 feet. The timber is hard and durable, and, from its crooked growth, is used, and is valuable for the knees of ships. The timber is not used for any purpose by the natives, but the Europeans at 'Hokianga use it in the construction of their vessels, and consider it as a valuable and durable timber for that purpose. The leaves of this tree, previous to their fall, change to a fine scarlet colour, causing the tree at that time to display a high degree of beauty; particularly when growing on elevated land, and viewed at a distance, (as when off the coast in a ship,) the appearance of it at that time is strikingly beautiful.

Pittosporum tenuifolium of SOLANDER'S MSS.

Tafiri of the natives of NEW ZEALAND.

Natural Family, *Pittosporææ*.

Class, *Pentandria*. Order, *Monogynia*.

This is an unpublished species of *Pittosporum*, the *P. tenuifolium* of Solander's MSS. and is figured in Parkinson's Botanical Drawings, made during Cook's First Voyage, deposited in the British Museum: it is named *Tafiri* by the natives of New Zealand. This tree attains the height (as far as I could observe) of 12 or 15 feet, but small in circumference: it yields a gum-resin, which is of a very agreeable fragrance: the capsules are black, containing a number of small black seeds, and seem also to contain a fragrant resin of a greenish yellow colour. The trees, from which

I procured my specimens, were growing on the banks of the river Wykéré, Bay of Islands, and collected on the 8th of July, 1829, but only in fructification.

Podocarpus Species.

Tótara of the natives of NEW ZEALAND.

Natural Family, *Coniferæ*.

This is an unpublished species of *Podocarpus*, and is figured in fructification in Parkinson's Botanical Drawings, made during Cook's First Voyage, now deposited in the British Museum. It is named *Tótara* by the natives of New Zealand, and attains the height of 80 or 90 feet, and in circumference from 15 or 20 feet, and is considered the next in diameter to the *Kowri*. The timber produced is of a reddish colour, becoming darker in colour from age; it is of excellent quality both in plank and spar. The wood is in high estimation among the natives of New Zealand, particularly for the construction of their canoes, its lightness and durability causing it to be highly valued and preferred before all others. I do not observe any gum-resin from it, but the specimens of the tree when dried have a very fragrant smell, which I did not perceive in them when recently gathered. This species of pine did not appear so abundant as several of the other kinds: I had no opportunity of seeing it until the 4th of July, 1829, when making an excursion up the *Kowa-kowa** river: I observed it growing on the woody hills, as well as on the banks of the river, from the young to the stately and lofty tree. The value placed on this tree by the natives is sometimes the occasion of quarrels, terminating in bloodshed, if it is cut down by any excepting the party by whom it is claimed; for which reason, and that it might be known as belonging to certain individuals, a mark is placed on the tree, and it is then reserved until it has attained a sufficient magnitude for use: so that it is not unusual for the trees to descend from father to son.

* *Kowa* signifies, in the New Zealand language, any thing bitter.

REMOVAL OF THE PREJUDICES
AGAINST DISSECTION.*To the Editor of the London Medical
Gazette.*

SIR,

FROM a sense of the difficulties which have hitherto been thrown in the way of the anatomical student, arising in a great measure from popular prejudice, and the severe legislative enactments respecting the possession of dead bodies for dissection, and which difficulties are not likely to be diminished by the publication of the recent horrible and disgusting occurrences, I am induced to offer you the following suggestion, which resulted from a conversation with an ex-overseer of an extensive parish in London, on the methods of supplying the dissecting-rooms of the metropolis with subjects. He had served the office several times, and had numerous opportunities of gathering the opinions of the officers of the neighbouring parishes. He referred the necessity of resorting to degraded men for a supply, to the enforcement of an absurd hospital regulation, which makes it incumbent on the friends of paupers admitted into the establishment to indemnify the hospital for the burial expenses, in case of the death of the patient; and in case he was friendless, obliging the parish to which he belonged to remove his body. This arrangement was absolutely necessary to the admission of the applicant. My informant seemed to lay the fault immediately to the government of these establishments. He said that he for one would cheerfully be absolved from the obligation of burying the paupers of his parish. It was a vast deal of trouble (he said) to him, besides making a very considerable addition to the expenses, and he did not doubt but that many overseers in the metropolis were quite of his opinion. He did not suppose that the feelings of the poorer classes would by any means be harassed by the supposition, that their bodies were destined for dissection, for many of them would be unconscious of the fact, others would be careless about it, and their friends (if they had any), in the certainty of Christian burial being afterwards properly performed over their remains, would cast off many of their prejudices.

I told him I thought that his suggestion would be of service to the medical community and the public at large, and would perhaps be the means of preventing future outrages upon the feelings and safety of his fellow-creatures. I am induced to address you, Mr. Editor, on this occasion, from the interest you have always taken in the advancement of science, and from your general openness to truth and liberal discussion.

I have the honour to remain,

Your obedient servant,

F. A. B.

Reading, Dec. 7.

ANALYSES & NOTICES OF BOOKS.

L'Auteur se tue à allonger ce que le lecteur se tue à abréger.—D'ALEMBERT.

The Cyclopædia of Practical Medicine.
Edited by JOHN FORBES, M.D. F.R.S.
ALEX. TWEEDE, M.D., and JOHN CONNOLLY, M.D. To be published in Monthly Parts. Part I. pp. 112.

SUCH a work as that of which we have here the commencement, has long been a desideratum in this country: British medicine ought to have set itself forth in this way sooner; and we have often wondered that, with the example, particularly of the French, and their "Dictionnaires" before their eyes, the profession and the enterprising publishers of Great Britain did not enter upon such an undertaking as the present long ere this. Before the month expires, we mean to expend some gentle criticism upon certain articles in this Part: but we may just mention now that some of them seem to us to be rather out of proportion with the others, in point of length, considered with reference to their relative importance. This is the Editors' affair, and shall be touched on hereafter. And, by the way, we cannot conclude without remarking that there are here only as many articles as form a fasciculus of seven sheets. This, we must say, is quite unreasonably too little for the money. We should either get better measure, or the price should come down. We are decidedly for the former alternative; and are of opinion, that, if it be the present intention of the Editors to complete the work in 16

Parts, of seven sheets each, they would act prudently in altering their plan a little, and by adding some two sheets or so to each number (which we think they could very well afford), finish off the Cyclopædia in 12 Parts, and let the whole be completed within the twelve-month.

MEDICAL GAZETTE.

Saturday, January 7, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

THE IRISH GRAND JURY BILL.

THIS, we doubt not, will seem to some of our readers rather an odd title for a leading article in the Medical Gazette—at a time, too, when topics so purely medical on the very face of them are pressing importunately for our notice: but a truce to the supply of the anatomy schools (and we have a large number of letters to answer on that subject), and truce to the ravages of cholera in the north—while we lay open to the astonished view of the profession in this country the disgraceful intriguing which is attempted to be carried into most mischievous effect, by a shameless gang of monopolists in the west.

Of a more illiberal and offensive measure than that which would fain be imposed upon the legislature by the Irish College of Surgeons, we have never happened to have cognizance; nor would it seem possible to find any thing equal to it in unblushing effrontery, did we not recollect the well-tried capabilities of some who would endeavour to supply it.

A bill has been for some time past in preparation, for the regulating of Grand Juries in Ireland, chiefly with regard to the presentments for money to be levied off the counties; and among other clauses which it contained in its first, and, as we shall presently shew, its more

perfect form, was one to the following effect: that the surgeons of the county infirmaries, provided they could produce the Governors' certificate for having duly and faithfully executed their duty, were to be "presented" for their salary without further requisite. This, however, was far from being in accordance with the policy of the Irish College, whose system would fall to the ground if so much liberality were permitted. To work they set with all their engines; and in especial took care to send over two wily deputies to be examined before the Committee of the House of Commons. And how did the worthy deputies proceed? Why, in the most straightforward way possible. They asserted boldly that the like situations were closed in England against the members of the Irish College. This was going upon a broad principle, and seemingly not an unfair one: the Committee were taken by surprise, and, acting upon the impulse of honest though deluded men, they at once inserted the limitation upon which the Irish College had set its soul. The clause, as it now stands in the draft of the bill, is in this form:—after proposing that it be lawful for the Grand Jury to present a certain sum at assizes, to be levied off each county, for the surgeon's salary, it says, "Provided always, and be it enacted, that no such presentment shall be made, unless, together with an application therefor, a certificate signed by the Governors of such Infirmary or Hospital shall be laid before the special sessions, &c.; *nor unless a true copy of the Letters Testimonial from the College of Surgeons in Ireland*, by law required to be obtained by every surgeon, shall be laid before such special sessions, &c." Now the words in *Italics* are those which have been inserted by the Committee, instead of those which originally stood there, referring to *any* of the legalized Colleges of

Great Britain and Ireland; and having been so inserted upon false and fraudulent representations, *must be erased*. They must be erased, unless the great body of Irish surgeons, who hold the diploma of the London College, and of Colleges in Scotland, with right on their side, choose, rather than bestir themselves, to sit down tamely, overreached and bullied by a paltry knot of interested monopolists. They must be erased, unless the members of the legislature choose to submit to a gross imposition, practised upon their credulity. And, most assuredly, erased they must be, if the public indignation weigh any thing against the base practices of the illiberal, unveiled to the public view.

But the monopolists are not without their *reasons*. They have enjoyed this privilege, they say, since they were a College, so that, just or unjust, they have a right to it; it is an endowment upon them by the law of the land, and, whatever other laws, conferring endowments, the wisdom of Parliament may see it useful to repeal, this they ought not to repeal; theirs is the most complete college; they turn out the best surgeons; their examinations are a severe test; for strictness and publicity they surpass those of any other College in the British empire; while the examinations of their opponents bestow upon the community "slaughterers of their fellow-creatures," "legalized murderers," &c. This is the quiver full of arrows which they have in reserve; their first weapon being what we have stated—the bold assertion that members of the Irish College are excluded from similar posts in England. Now we will take the liberty of demolishing this whole fabric of defence—this barefaced defence of what is, in the eyes of all people, unfair and monstrous in the very assumption; and we will do so by, in the first place, giving the most positive con-

tradiction to the audacious statement of the deputies, that there is any exclusion in this country exercised towards the members of any of the legalized Colleges; there never was a more impudent fabrication: but as it answered the immediate purpose for which it was intended, we presume it will not be further insisted upon.

But this monopoly is their "privilege," and they will by no means resign it. We think they are right; it is the main pillar of their support. If they are once deprived of this exclusive privilege, it is all over with them—they are thrown upon their own resources, and must shift for themselves as best they may in the general competition: but they shall not be permitted to vindicate their privilege—their all-in-all—at the expense of honesty and truth. If they tell us that it is an *old* privilege, and that therefore they should continue to possess it, we deny the consequence, and go a little farther—they should have been stripped of it long ago. The legal ordinance by which their *supposed* right is constituted, was an act of the Irish Parliament, passed about twenty years before the College in question had an existence; it was passed at a time when a national partiality might have had some excuse, and when Irish surgeons had no chance of getting employment abroad. There was then a Board of Examiners established in Dublin, to decide on the merits of all regularly-educated surgeons—candidates for infirmaries; and when the College was incorporated, the powers of the Board merged to the new body, and one of the first acts of the latter was to pervert those powers, by putting their own construction on the word *regularly-educated*. In this the monopoly has had its root. None were henceforth to be accounted regularly-educated who had not served an apprenticeship to some of "the College." And so has the custom been religiously kept up

even to the present day; notwithstanding the legislative union between the two countries, which took place at the beginning of the present century,—notwithstanding their own repeated (mock) demonstrations of fair dealing and candour,—and notwithstanding the general indignation of all the more enlightened part of the community.

But whatever shadow of excuse the old Irish parliament, seventy years ago, might have had for conferring the supposed privilege, (we say *supposed*, for we have no doubt the original intention has been perverted,) none whatever must be allowed for the effrontery of those who support it on the plea of their own pretensions. What! the Irish College (how droll) call in question the eligibility of the surgeons of Great Britain! pronounce upon their “incompetence”! nay, brand them as “legalized murderers,” “slaughterers of their species,” and what not*. Will any one be good enough to inform us, who are the men of this mighty College? What have they done? What have they written? What have they added to the stock of surgical science—what principle, what doctrine have they established? We ought to know something about them, if they ever made a stir in the scientific world; but really we cannot charge our memory with any thing of the sort. The only way which the existence of their establishment is brought to our recollection, from time to time, is by its extreme turbulence, and the angry bullying of one of its little teachers. And this, forsooth, is the *arbitrator scientiarum* that undertakes to pronounce on the merits and competence of English surgeons! It is too ridiculous.

Again, “the system works well.” In

what respect, we should be glad to know? We have no time for parleying, but we take leave broadly to deny the fact. Take an equal number of the *élèves* of either system, and just compare their respective value and consequence in the community. Of the English county surgeons, the world takes some account—those of Ireland are quite unknown. And take the best men of this Irish College—and who are they? Can they stand for a moment in competition with those at the head of the profession in England, France, or Germany?—We have heard a sort of boasting, too, about the excellence of the Irish licentiates in the army. If we are to judge of them by their deeds, we must return to our old query—what have they done? Where are their works—by their works we would know them—where are their contributions to science?

Now let us come to their famous examinations, so much be-praised by the retainers of the College, and by those who ought to have more discretion. “Their strictness and publicity are unrivalled.” We have heard strange stories of this same strictness. “If the splenoid bone be thrown upon a table, which side of it turns uppermost?”—Oh, for the pen of a Smollett to describe what we have heard; but let it pass. The *strictness* of the examination is certainly, we have no doubt, unrivalled. It affords a rich harvest to the *grinder*; and what is better still, if report speak true, certain members of the court have *ground* the very candidates whom they examine: or, if *they* have not, most undoubtedly some of their relatives and partners have. To the *publicity* we have only one objection to make: grinding being the order of the day, and avowed grinders, in the way of business, constituting the chief part of the audience, perhaps it would be just as well if the whole affair were managed with closed doors.

* *Qui facit per a'lerum facit per se*—if the College don't do it, their Professor (Jacob) does it for them,—and nothing in Billingsgate can equal the figures of speech in which some of this gentleman's public lectures are conceived and expressed, and some of his avowed compositions given to the public.

So much for the working of this precious system! We are aware that we have given it more space than its intrinsic worthlessness would properly warrant. In this country, the surgeons of the London College may look, with a smile of indifference, on the proceedings we have described: but in Ireland, we should rejoice if the consciousness that the eyes of British practitioners are turned upon them, would stimulate the members of the Royal College of London to redoubled energy in resisting the imposition which is attempted to be practised upon them. We have perused with pleasure the petitions to the legislature from Dublin and Cork; both are highly creditable to the framers of them; and we were delighted to observe appended to the former the name of an enlightened professor of the university, who, if the boobies of York-Street were to be credited, should, as a London surgeon, be *incompetent* for the high situation which he holds. But they have already got a lesson at Limerick, where all their efforts could not introduce their monopoly: let them learn wisdom from this betines, or at least patience, for they will soon need all the little philosophy they can muster.

NATIONAL VACCINE ESTABLISHMENT.

WE are happy to learn, from the best authority, that in consequence of representations made in the proper quarter, this very important and useful institution is to be continued. The intention to abandon it must be acknowledged to have originated in very mistaken notions of economy, as it is quite obvious that without some assistance from the Government, no small risk would be incurred of the supply of vaccine lymph becoming either very deficient or altogether lost. It is truly a national object, and it may prove satisfactory to our readers to be informed, that vaccination continues to be gratuitously practised at the several stations, and the virus transmitted, as heretofore, to

all parts of the world, on proper application.

PROFESSIONAL PROPHETS.

NOTHING can equal the absurdities into which the spirit of prophecy betrays its votaries in these latter days. Our worthy contemporary, Dr. James Johnson, ventured, some months ago, upon a display of this description with regard to cholera; viz. that the disease would either not make its appearance in this country, or, that if it did, it would come shorn of its fatality. These predictions, which were put forth in a manner amusingly oracular, had scarcely been made public ere the malady broke out at Sunderland, where, as well as in the adjoining places, the rate of mortality began and has continued at something above *one in three*,—a proportion of deaths quite unknown in any other disease of this country. The Doctor, coolly contemplating the circumstances, has candidly told us, in his Journal, that “he was a fool” for venturing on the prediction; but again waxing warm as he proceeded, and encouraged by the length of time during which the disease lingered in Sunderland, in the plenitude of inspiration he ventured once again to intimate, that he would probably prove a true prophet after all, and, in fact, that “as far as the events have yet gone (Dec. 10th) he had but little cause to repent of his prognostications.” This second and desperate “hazard of the die” had scarcely been risked, and though too late to be retracted, had not yet been published, when the sudden and prodigious increase of cases at Gateshead and Newcastle spread general alarm. Would it not be advisable for those who are bent upon withdrawing the veil from futurity, hereafter to announce their dogmas in the “unknown tongue,”—leaving their interpretation until the lapse of time shall have brought with it their fulfilment, and thus rendered the explanation safe?

WEEKLY REPORT OF CHOLERA.

	Total Cases.	Total Deaths.	Remaining.
Sunderland	551	200	1
Newcastle	432	150	116
Gateshead	285	88	89
North Shields ...	32	16	8
Houghton-le- } Spring..... }	38	19	6
Haddington.....	19	6	5
Pittington	1	1	0
Grand Total.....	1387	479	225

NEW STYPTIC.

On Tuesday last, MM. Talrich and Halma-Grand visited the London Hospital, to exhibit the effects of their new styptic. A considerable number of persons assembled in the anatomical theatre to witness the experiments. A sheep having been placed on the table, M. Talrich cut down upon the carotid artery, and, having raised it by means of a hook, a longitudinal incision, about half an inch in extent, was made in the vessel, and while the blood spirted forth in a continued stream some fine tow, steeped in the styptic, was kept applied with pressure for about ten minutes; the wound was then plugged with other pieces, which were retained by two pieces of string round the neck. The animal, after the operation, continued moving about, when fresh hæmorrhage broke out, and about a pint of blood was lost. The styptic was therefore reapplied as before, and the sheep kept quiet, when all bleeding stopped. A second sheep was then placed upon the table, and a small piece of the carotid artery removed with a pair of scissors; the styptic was applied, but the animal struggled violently, and fresh hæmorrhage ensued, which they were unable to check: it appeared to respire with great difficulty, and in a few minutes died. The foreigners appeared mortified by the result of this last experiment, and M. Talrich addressing the gentlemen present, requested them to suspend their judgment of the efficacy of his styptic until they had seen the result of further experiments. He said, that, in English sheep, the cellular tissue was so loose around the artery, that it was difficult to keep the styptic applied to the wound in it; and the difficulty was increased, in the second experiment, by the struggles of the animal, which was much pressed upon by those crowding around, and appeared to die from suffocation.

The next day the plugs were removed, and the first animal appears to be doing very well, no hæmorrhage having ensued.

REMOVAL OF THE SUPERIOR MAXILLA.

This operation was performed on Wednesday, at the London Hospital, by Mr. Scott. Pressure was made on the carotid artery at first, but the hæmorrhage was so great that it became

necessary to secure the vessel by ligature. The diseased growth was of a soft fungous nature, and the patient bore its removal extremely well. We hope to be able to give a detailed report of the case when the result is known.

NEW TREATMENT OF PAINTERS' COLIC.

M. GENDRIN, at a recent meeting of the Institute, stated the results of some trials he had made with alum in colic from lead: these were very favourable; in fact, the whole of 58 patients in whom it was used, recovered. It was given internally, in solution, and from the extent of from one to three drachms a-day. M. Gendrin, however, was led to suppose that the benefit which resulted was chiefly owing to the sulphuric acid contained in the salt; he therefore gave this acid in water and in lemonade, by which means he states that the cures were effected with equal certainty and greater rapidity than by the alum.—*Gazette des Hôpitaux*.

MORPHIA ENDERMICALLY APPLIED.

MM. TROUSSEAU and BONNET recommend the application of morphia to an open surface in a variety of painful affections. The method they have found most convenient is to remove the cuticle by means of vesicating ointment, containing ammonia, (concentrated ammonia and axunge, of each equal parts), by which means a blister is raised in ten or twelve minutes: the cuticle is then to be removed, and the muriate or sulphate of morphia, which are preferred to the acetate, as being more soluble, are applied to the extent of half a grain. This may be effected either by sprinkling the powder over the part, or making it into a solution with a few drops of water.—*Ibid*.

ERGOT IN MENORRHAGIA.

DOCTORS PIGNACA and CABINI have lately published some cases of immoderate menstruation, in which ergot, in doses of from ten grains to half a drachm in 24 hours, seems to have been of marked advantage. The same observation applies to cases of leucorrhœa similarly treated by Dr. Bazzoni.—*Annali di Medicina*.

MEDICO-CHIRURGICAL SOCIETY.

Tuesday, Dec. 27, 1831.

MR. LAWRENCE IN THE CHAIR.

THE attendance of members this evening was not very considerable, nor was the sitting prolonged to the usual hour of breaking up. The last part of the learned President's paper on tumours was read, but gave rise to no discussion. There was some conversation on hydatids, in which Mr. Langstaff took a principal part; and it was announced, that at the next meeting a paper, by Dr. Hodgkin, on the absorbents, is to be read.

With regard to Mr. Lawrence's paper; as we did not give a report of the first part, (read Nov. 8) and but an outline of the second, we shall take the present opportunity of giving the *whole*, in the shape of an ample abstract of

Observations on Tumors, with Cases: by
W. Lawrence, Esq. F.R.S.

The author begins by remarking on the great importance of the history and treatment of tumors—a subject hitherto only very imperfectly understood. There are many difficult questions which might be proposed, and to which no satisfactory answer can be given, until we possess better histories of particular tumors. Mr. L. professes only to contribute his portion of a labour which would be best performed by many observers; and he proceeds to elucidate some subordinate points, with the hope that other members may be induced to favour the society with the result of their experience bearing on the same subject. When Cullen wrote, it is plain that nothing was known of tumors, in the more restricted sense in which we now employ the term. Mr. Abernethy was the first who took a philosophical view of the matter, and showed that growths constituting tumors ought to be distinguished and characterized by their anatomical structure; he proposed to confine the term tumor “to such swellings as arise from some new production, which made no part of the original constitution of the body;” though, unfortunately, he does not adhere very strictly to his own definition, abandoning it, in fact, in the very same page. But this is not indefensible, if we only observe the difficulties which lie in the way: there is such an insensible transition from one structure of this sort to another, that the most accurate observer is often obliged to hesitate in assigning to each its particular place; and it is a false analogy that is commonly introduced, to compare diseases with the species in natural history; for the latter are distinctions established by nature, while the former may be more properly compared to the ever-varying results of different shades and combinations

of colour. Mr. Lawrence considers that Mr. Abernethy's original design of confining the term tumor to new productions ought to be strictly adhered to, and that the distinction between them and changes of structure, although it cannot always be satisfactorily established, is not only useful but very important, both in pathology and treatment. With regard to the natural history of these new productions, the author proposes, in the first place, to inquire into the mode of their origin and increase. Do they arise from the effusion of blood and its coagulation, and the subsequent organization of the coagulum? or from the effusion and organization of coagulating lymph? or lastly, from chronic inflammation? None of these explanations are satisfactory; for, according to any one of them, tumors ought to pass through successive stages, and to present different appearances at different stages of their development; in which, however, observation does not bear us out. Tumors in their earliest stage and smallest size have their peculiar structures, as well marked as in their subsequent progress and full development. Those explanations were probably suggested by John Hunter's statements, respecting the production of vessels in coagulated blood, the agency of this process in effecting the union of wounds and fractures, and its occurrence in the effusions of blood into serous cavities. We now know, however, that the union of wounds and fractures is not accomplished in this way; and moreover, that these processes most readily take place when there are no coagula present. But supposing for a moment that these explanations were feasible, it would be rational to expect growing tumors to yield to leeches, cold applications, and antiphlogistic treatment; whereas no advantage whatever is derived from this course in new productions—though there may be some in certain swellings caused by change of structure. Mr. Lawrence, however, confesses he has nothing to offer in place of those explanations of which he disapproves; and he instances our equally striking ignorance on the analogous subjects of the capillary vessels, in one part laying down muscle, in another bone, and in another fat, and so forth.

In general it may be observed, that the new or accidental productions constituting tumors correspond in structure with the parts in which they are produced. Thus we have masses of fat formed in the subcutaneous adipous tissue, and on the other hand tumors of cellular structure occur in that kind of cellular tissue which does not contain fat. The latter are by no means so common as the former; indeed we have no where in books a clear case of cellular tumour—a circumstance which induces Mr. L. to relate one or two cases which occurred to him. Like adipous tumor, it is unat-

tended with pain, and becomes troublesome or dangerous only by reason of its bulk.

Cellular tumour.—In the year 1826 Mr. Lawrence was consulted by a lady twenty-eight years of age, handsome and well formed, with fair complexion and light hair. She talked of having a rupture; but upon stripping to show it, astonished Mr. L. by the sight of a tumour, twice as large as his head, hanging from one of her buttocks. It exceeded in breadth the transverse measurement of the two thighs. The complaint had existed for four years, and had not grown fast during the two first. Even at present it was only troublesome by its weight, bulk, and inconvenient position. It had commenced at the posterior part of the left labium, and had extended gradually along the buttocks and behind the os coccygis. The tumour was soft, but not fluctuating—the skin loosely connected with it, and in some parts partially excoriated from pressure and friction. The basis was the smallest portion of the tumour, expanding into the large mass described. It extended from the labium to the os coccygis, and from the edge of the gluteus magnus to the anus. This part (the basis) measured twenty-one inches in circumference, was quite moveable, but of uncertain relations in front. Mr. L., however, satisfied himself that it had no connexion with the structures usually engaged in rupture. Mr. Wardrop concurred in the propriety of the removal of the mass, and this Mr. L. effected in September 1826, cutting *through* the tumour only at that part where it was necessary to do so—where it extended into the vagina. There was much hemorrhage, and the lady was put to bed very faint. Some feverish symptoms came on, but they did not prove troublesome. In the course of a fortnight she left town with the wound nearly healed. The tumour, on examination, was found to consist of a fleshy mass, elastic, and almost fluctuating to the touch. Its texture was uniform, though rather more compact when pressure had been made; it was tough and fibrous, made up wholly of condensed cellular tissue, entirely free from fat. The fluid in the cells was not examined, but it is probable that it was similar to the fluid usually found in the interstices of natural cellular tissue. There was some return of the tumour in 1828, and a second operation was performed by Mr. L. after the lady's confinement of her first child. The anterior prolongation was now carefully traced, as it ascended along the vagina becoming gradually smaller. While being put on the stretch for the purpose of examination, it suddenly gave way and was found to have tapered to a point. There has since been no recurrence of the complaint.

Between these tumours and the enormous swellings in which the male organs of generation are sometimes involved, there is this distinction: the former are new productions

—the latter mere enlargements of the cellular and cutaneous tissues, resulting from interstitial deposition consequent on repeated attacks of inflammation attended with the usual local and constitutional symptoms. Mr. Lawrence removed the prepuce of a gentleman who had resided in the West Indies, and who had contracted this complaint. The part was about the size of one's fist; but it was clearly manifest that the increase of bulk was owing altogether to the distention of the cellular tissue: there was scarcely any change perceptible in its anatomical character.

Tumors in the vicinity of the parotid gland.

These are of the nature of what Mr. Abernethy calls Pancreatic sarcoma, "made up of irregularly shaped masses, in colour, texture, and size, resembling the masses which compose the pancreas." Mr. A. however, does not recollect any case in which he met this sort of structure, except in the female breast; in one instance indeed he observed something like it in a man who had "three diseased lymphatic glands, each the size of a large plum, situate beneath the basis of the jaw upon the mylo-hyoideus muscle." Mr. Lawrence has met several cases of tumors of this character occurring close to the parotid gland, and near the angle of the lower jaw; and once, under the basis of the jaw, close to the submaxillary gland. Do these growths derive their character from their situation? Do they resemble the salivary glands because they are in the vicinity of those structures? There is, Mr. L. thinks, as much analogy in the case, as we ever see between any accidental production and a natural part; nor has he seen any similar tumour in any other situation—the case to be presently adverted to where the new growth occupied the angle of the mouth scarcely affording an exception. They are uneven, knotty, or lobulated on their surface, as if made up of masses more or less distinct; and they are sometimes as hard and incompressible as the most dense scirrhus swelling of the breast; but in general they are not so hard—but still firm. They are loosely connected with the surrounding parts, and therefore easily moveable; a character which they retain as long as they exist, and in this respect they are readily distinguishable from scirrhus, with which they might be confounded if the mere circumstance of hardness were attended to. They are not attended with pain in their growth—grow slowly—and the largest size Mr. L. has seen was about that of an orange; nor are they painful on pressure; when cut through, they generally exhibit a very light brownish yellow tint, and an obscurely lobulated arrangement. The texture is less hard and tough than in scirrhus, and instead of being unyielding will break short off. Some of the larger growths, however, will creak under the scalpel, and oc-

casionaly exhibit a few bony particles; and the admixture of some streaks and patches of coagulated blood might seem sometimes to promise that the complaint was of a malignant character. This sort of tumour has commenced about the 20th year with some patients; with others, later. It has always been on the left side, so far as Mr. Lawrence's experience goes, and generally in contact with the parotid gland. It is innocent: the absence of pain, and the sound state of the neighbouring lymphatic glands, are well marked distinctions between it and tumor of a malignant kind. It does not lead to any morbid constitutional disposition either, so that extirpation by the knife has been invariably and permanently successful in every case—except one, in which after an operation by Mr. Macilwain, death ensued from erysipelas. But removal should be effected at as early a stage as possible. Some cases in illustration (five in number) were here detailed: of these we have before given sketch: but we shall take this opportunity of giving more at length the case of Mr. R. the most important and critical of them all—whether we consider it with regard to the boldness of the operation, or the apparently dangerous nature of the complaint.

Mr. R. about 40 years of age, tall and robust, of dark complexion and hair, in the enjoyment of excellent health, consulted Mr. Lawrence, in the summer of 1826, about a large swelling over the left side of his lower jaw, which had existed there for eight or ten years, increasing slowly, without pain or inconvenience of any description. Recently, however, it appeared to have become more active in its growth, and was constituting rather a conspicuous deformity. It was as large as a middle-sized orange, irregular in its surface, and with a prominent knob in front equal to a large nut. The integuments were loosely connected, so that they could be pinched up, and the connexion was equally loose in all other directions. The whole mass could be moved easily, but it was doubtful whether the middle of the basis might not be more fixed. The evils, however, to be apprehended from its continued growth, suggested strongly its removal, though the boundaries of the disease towards the integuments of the neck were so uncertain. The patient having given his consent, an incision was carried over the swelling, from the chin to behind the ear, of which the lobe was considerably elevated by the tumor. A perpendicular incision along the cheek joined this at right angles. The tumor being then quickly denuded on its external surface and sides, was found to adhere firmly at its base; and several arteries were divided in detaching it. Mr. Lawrence could not separate it in the middle, as it went obviously behind the jaw; so he cut it off, and laid open a central cavity, from

which fluid of a yellowish watery appearance escaped. After tying some arteries, a considerable mass, which appeared to be the remainder of the disease, was removed from behind the jaw: it was, in fact, a portion of a cyst, with thick sides, which, when fitted to the part previously removed, completed the cavity. There still remained, however, a dark, livid, bloody substance, of spongy texture, going inwards behind the lower jaw. On its front edge the external carotid was beating, completely denuded for about an inch. A close examination of this spongy part shewed it to be contained in a thin white cyst, which was connected by loose cellular tissue: it was dissected out cautiously, but still it was impossible to get behind it, and the external carotid was palpably beating on its posterior margin. The mass broke down readily under pressure with the finger, which passed behind the pharynx, between it and the spine, to the middle of the neck; the fragments were dark, reddish, soft, and friable. After the operation, it was observed that the mouth was drawn to the opposite side, and was much distorted. The tumor was of tolerably firm texture, with a large central cavity, the sides of which were smooth; but no communication could be ascertained between the interior of this cavity and the spongy prolongation behind the ramus of the jaw. The appearances, on the whole, naturally suggested the inquiry, whether the tumor was of the fungous class, and consequently malignant; and whether the part left behind would be likely to throw out a bleeding fungus, or lead to reproduction of the disease? Mr. Callaway, who assisted Mr. L. in the operation, and other gentlemen to whom the tumor was shewn, thought unfavourably on these points; and Mr. Lawrence himself partaking of the same impression, gave a very bad prognosis to the patient's friends. Mr. Wardrop, however, decidedly pronounced the structure not to be malignant; and his opinion was justified by the event—for the patient had recovered, and was out in a fortnight, and has continued perfectly well to the present time. There was some inflammation and discharge, but not of an unhealthy character, from the wound, for about a fortnight after the operation. Mr. R. then left town.

The parotid was much cut in the operation, but no inconvenience was observed to result.

At this time, Mr. Lawrence reports, the cicatrix and surrounding parts are quite healthy, and there is no more fulness than in the sound side. The paralytic distortion of the face, which was considerable, is diminished; but the left corner of the mouth still drops, and the eyelids do not meet in attempts to shut the eye; but no inconvenience is experienced from this circumstance.

With regard to Mr. Abernethy's doubts,

as to whether the swellings in the female breast, which he mentions as illustrations of his pancreatic sarcoma, are new productions or changes of structure, Mr. Lawrence is decidedly of opinion that those tumors are new productions, "being generally loose in their situation, surrounded by a distinct capsule, and exhibiting a structure, which, although analogous to the gland in which they are more or less imbedded, is yet clearly distinguishable from it." Sir A. Cooper well describes these in his "*Illustrations of the Diseases of the Breast*," under the name of chronic mammary tumors. They differ, however, from the tumors above described, by being much softer and looser in texture, and more distinctly and minutely lobulated throughout. They never exhibit the same scirrhous hardness, nor are they unattended by pain; these mammary tumors, in fact, are often the cause of severe suffering. In this respect, as well as in their anatomical characters, they are obviously assimilated to the part in which they are produced.

Tumors which put on a Malignant Aspect.—It is much to be regretted that the distinction between innocent and malignant tumors, which is of great importance in practice, more especially with regard to the question of operation, has never been clearly traced. Tumors which, in their regular progress, destroy life by the changes occurring in the affected part, such as ulceration, bleeding, sloughing, or by causing similar productions in other parts of the body, more particularly in important internal organs, or by both together, are considered malignant; and the occurrence of serious local and general symptoms, the developments of new growth in other parts, and the existence of such constitutional suffering as leads to the suspicion that organs of consequence are involved in the affection, are generally regarded as decided proofs of malignant character, and insuperable objections to an operation. Mr. Abernethy has described a disease of this sort, under the name of *tuberculated sarcoma*, which he seems to have chosen in consequence of the secondary growths consisting of small, hard masses, or tubercles; it is, he says, of a very malignant nature. Mr. Lawrence relates a case, in which the original tumor was of a most threatening aspect, in which several smaller swellings had shewn themselves in other parts, and the patient had been brought to the brink of the grave by severe constitutional disturbance, but where amputation was performed with complete success, or life, at all events, prolonged for many years. The case serves as an excellent caution against concluding hastily upon the malignancy of a disease, and the hopelessness of a patient's situation.

A gentleman, 27 years of age, had a tumor which made its appearance spontaneous-

ly in his left thigh, and was increasing rather rapidly. He consulted a very eminent surgeon, who at first advised amputation, but on the day appointed, was so alarmed at the local and general symptoms, that he determined not to proceed until a consultation should have been held on the case. Four of the most celebrated practitioners resolved that the affection was malignant, and that an operation would not be justifiable. The patient, however, (resolved to avail himself of any opportunity, however slight) adopted the suggestion of a friend, and called in Sir W. Blizard and Mr. Lawrence. These gentlemen found a tumor of an elastic feel, undefined in its circumference, about 4 inches in diameter, and with the skin shining and bright red on the anterior and inner part of the left thigh, a little above the knee. There was a firm indolent swelling as large as a hen's egg imbedded in the soft parts at the back of the pelvis, and a similar one in the back, near the spine; one as large as a nut over the left eye, and several smaller ones just under the skin in various parts. All these smaller productions had shewn themselves subsequently to the appearance of the large tumor in the thigh. The patient was almost worn out by pain and want of rest, excessively emaciated, and troubled with profuse fetid perspiration. Mr. Lawrence confesses that he considered the case to be very hopeless, from the strong probability that the disease had reached some of the vital internal organs. Things, however, did not appear in quite so unfavourable a point of view to Sir W. Blizard, and amputation was accordingly performed two days after, (March, 1819.) This was about half a year after the first appearance of the swelling. Relief was immediately experienced, and the patient eventually recovered. The tumor in the eyebrow was removed in 1825. In December, 1828, the same gentleman had a tumor removed from his forearm, which used to give him indescribable torture, darting upwards and downwards like electric shocks along the line of the ulnar nerve. The disease proved to be seated between the flexor carpi ulnaris and the bone, and the nerve adhered so closely to it that a portion was removed with the tumor. In December, 1830, he came to Mr. L. again about another tumor, seated in the flesh of the stump, which had become troublesome within the preceding six weeks, and was attended with electric dartings of pain from the spot to the body. Upon its removal, the swelling was found to be covered with a thin white capsule, and homogeneous in compactness, toughness, and colour, it approached to the characters of schirrus. The pelvic and dorsal tumors remain nearly as they were twelve years ago. The appetite, health, and strength, are tolerably good; but there is of late in-

crease of suffering, strange sensations, sudden dartings and shootings, occasioning convulsive movements, and compared to the effects of electricity, are often experienced.

Productions of dissimilar character are often found combined in one and the same morbid growth; hence much difficulty arises in determining the nature of tumors, and deciding the question of operation. The tumors of bones afford an example. From the interior of these organs, more especially the femur and tibia, there are frequently produced growths of the medullary kind, decidedly malignant; or of the fibrous and osseous description, of an innocent character; or growths of both these kinds blended together. Now all these productions pass under the vague term of *osteosarcoma*, a term which, Mr. Lawrence thinks, it would be far better to banish entirely from medical nomenclature: to the terms fungus hæmatodes, melanosis, cancer of bone, fibrous and osseous growths, &c. he has no objection, as they serve to render our descriptions sufficiently clear and intelligible, and enable us to lay down rational rules of treatment. The following is a case of tumor of mixed character: the result of the operation has been highly satisfactory.

A poor woman, aged 30, came into St. Bartholomew's Hospital at the latter end of June 1831. On the left leg, immediately below the knee, was a swelling nearly as large as her head; it rendered the joint nearly motionless. It was apparently composed of smooth rounded masses, separated by slight depressions. The skin upon it was distended and nearly fixed, but not unhealthy looking. The superficial veins enlarged and numerous. In front of the head of the tibia (when the disease was first noticed), the integuments were glossy and livid, with a shallow ulcer about the size of a half-crown in the centre. This was where a slough had been produced by caustic. In some parts the tumor was of bony hardness, while in others it was soft and elastic. The glands in the groin not enlarged. The disease began in December 1828, after a hurt from a fall.

Its growth was gradual and without pain, until February, 1830, when it grew much larger and was occasionally very painful. The patient was in other respects healthy, and not an unfit subject for an operation. The thigh was amputated July 24, 1831. Much blood was lost during the operation, the vessels of the part being very numerous; but every thing went on well. On the 29th the ligatures came away, and by the 11th of August the stump was healed, and the patient sent home cheerful and well. The tumor, upon examination, was found to arise from, and be inseparably connected with the upper six inches of the tibia; there was a continuity of osseous substance between

them, divided by a vertical section; the origin of the mass was traced to the centre of the tibia; the whole consisted partly of tough fibrous tissue, with bone in abundance, and partly of medullary matter, containing numerous cells, of which the largest were from one to two inches in diameter. The cells were filled with watery transparent yellow fluid, and some of them contained a little coagulated blood. But the medullary substance composed the nucleus of the tumor. (Preparations of the mass were exhibited.)

In cases of medullary growths, the progress of the disorder is much more rapid than in the above instance; and it is attended with severe pain and constitutional disturbance. In a man 22 years of age, whose thigh was amputated in the summer by Mr. Earle, on account of a large medullary tumor growing from the former, the disease had come to a crisis in five months, and with most distressing annoyances.

Encysted Tumors.—The extreme variety observable in cysts, and the nature of their contents, would lead us to suppose that the same principles of treatment were not applicable in all cases of these tumors. Sometimes the cyst is like the thinnest serous membrane, containing a watery fluid. Sometimes it is fibrous, compact, and thick, perhaps with a cuticular or horny lining, or with a cartilaginous or osseous texture, containing either thick fluid or compact fat. Fibrous cysts must be either let alone, or removed by the knife: to irritate them is not only ineffectual, but very dangerous. This, however, may be done to the serous cysts: we make an incision into the structure and keep it open, and produce obliteration by inflammation.

In the year 1819, Mr. Lawrence had under his care, at the London Ophthalmic Infirmary, a man about 40 years of age, with a partial protrusion of the eye-ball. It had been of slow growth, attended with deep seated pain in the orbit, depriving the patient of rest, and reducing him greatly in flesh and strength. The globe was pushed out of the orbit, and at the same time thrust downwards, but was still covered with the lids. Vision was destroyed. A tumor was found to be projecting under the superciliary ridge, not distinctly circumscribed nor moveable. An obscure fluctuation having been perceived in it, the tumor was penetrated, when about a dessert spoonful of clear watery fluid ran out. Two days after, a soft opaque white substance presented itself in the puncture; it was removed with a pair of forceps, when it proved to be an hydatid. Two or three more came away next day. The puncture having been now enlarged, and tepid water, being injected, half a teacupful of hydatids of various sizes were brought away.

The globe of the eye now resumed its natural position, and the pain ceased altogether.

A puriform discharge took place from the opening, but soon diminished, and the puncture closed. The eye remained amaurotic, but recovery was complete in all other respects.

In conclusion, Mr. Lawrence observes that Professor Delpech in the work already quoted, has given two cases of serous cyst in the o bit: one of them containing a clear serous fluid—the other, a single large hydatid. In each case he made an opening and introduced charpie. But Mr. L. disapproves of the method, as likely to excite and keep up serious inflammation; and refers to the cases above related, as proving that the great object can be accomplished without having recourse to any such process.

METEOROLOGICAL JOURNAL,

Kept at EDMONTON, Latitude $51^{\circ} 37' 32''$ N.
Longitude $0^{\circ} 3' 51''$ W. of Greenwich.

THE warmth of the day is observed by means of a thermometer exposed to the north, in the shade, standing about four feet above the surface of the ground. The extreme cold of the night is ascertained by a horizontal self-registering thermometer, in a similar situation. The daily range of the barometer is known from observations made at intervals of four hours each, from eight in the morning till the same time in the evening. The weather and the direction of the wind are the result of the most frequent observations. The rain is measured every morning at eight o'clock.

Dec. 1831.	THERMOMETER.	BAROMETER.
29	from 32 to 43	from 30.17 to 30.12
30	25 41	30.20 30.34
31	22 37	30.30 29.16
Jan. 1832.		
1	20 33	30.16 30.12
2	21 37	29.92 30.80
3	27 32	29.76 Stat.
4	16 32	29.68 29.61

Wind variable, N.W. and S.W. prevailing.
Except the 29th, 2d, and 4th, generally clear;
foggy during the 4th and evening of the 3d.

CHARLES HENRY ADAMS.

SUPPLEMENTARY NUMBER.

We this week publish an extra number of the *Gazette*, containing a selection of Cases and Letters, &c. from the reports made to the Central Board of Health; together with numerous valuable papers and documents on the subject of Cholera. Compelled as we have been to adopt some method of keeping pace with the wishes of

our correspondents on other subjects, and at the same time of giving publicity to the increasing number of communications on Cholera, we have deemed it most expedient to throw them into the present form, leaving it entirely optional with our readers to procure them or not, as they think proper. It will be observed that the paging is so contrived, that the volume may be bound up equally well with or without the supplementary number.

The mass of original matter this week laid before the public is altogether unprecedented in the annals of any medical hebdominal journal; and having thus cleared off most of what had begun to accumulate to a formidable extent, we are once more ready to give immediate insertion to the papers of those who may honour us with their patronage.

We are requested by Dr. Alderson, of Hull, to state that Mr. Fielding and Mr. Craven were the gentlemen who accompanied him and assisted at the inspection in the interesting case of Simulated Cholera, contained in our last Number.

The papers of Dr. M. Hall, Mr. Smith, Dr. Copland, Dr. Alderson, Mr. Burnet, Mr. Bafeman, and Dr. Howison, are preparing for publication.

Mr. Earle on Diseases of the Hip, next week.

BOOKS RECEIVED FOR REVIEW.

Remarks on the subject of Lactation, &c. By Edward Morton, M.D.

A Treatise on Pulmonary Consumption. Second Edition. By John Murray.

A Treatise on Cholera Morbus, from the French of Baron Larrey. By H. Paterson.

An Antidote to the pernicious Doctrine of Non-Contagion, &c. By Henry Bird.

On Indigestion and Costiveness; with Hints on the important, safe, and efficacious Means of relieving Diseases of the Digestive Organs by Lavements. Second Edition. By Edward Jukes.

Elements of Practical Chemistry, comprising a systematic Series of Experiments. Second Edition. By David B Reid, M.D.

ERRATA.

In our No. for Dec. 31, p. 398, for "lubicundus," read "ludibundus."—P. 399, first column, between the 7th and 9th lines, insert a line, which had dropped out, viz. "the position accounted for—heart," &c.—Same page, col. 2, l. 16, for "inch and half," read "ounce and half;" also, in part of the impression of the Supplement to the present No. p. 519, for "mustard poultices," read "mustard emetics."

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THE LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JANUARY 14, 1832.

LECTURES

ON

THE THEORY AND PRACTICE OF
MEDICINE ;

Delivered at the London University,

By DR. ELLIOTSON.

PART I.—LECTURE XV.

Ultimate Treatment of Acute and Chronic Inflammation, and of the Terminations of Inflammation.—Specific Inflammation.—Discharges.—Hæmorrhage.

IN treating, gentlemen, *acute* inflammation, when you find you have carried depletion as far as is admissible, and there is still quickness of pulse, and considerable irritation existing, and you are quite satisfied that you cannot carry depletion any farther, either general or local, you have reason to suppose that the case is now one of irritation, and you have to support the patient well, perhaps give him stimulants, and quiet him by opium. So, too, in the treatment of *chronic* inflammation ; when you have persevered for a great length of time, and when you find, notwithstanding all you have done, constant evacuations, constant draining, constant abstinence—that the signs of local inflammation still continue, you have reason to suppose there is something more than mere inflammation, that it is not simply chronic inflammation you are treating, but that there is organic disease at the bottom, which is keeping up this chronic inflammation. In fact, for the most part, you have no other reason to suppose organic disease—unless you find enlargement and induration, in chronic inflammation, but that all you do has no effect upon the inflammation ; the symptoms continue or arise again, notwithstanding your efforts. In disease of the liver it is impossible, for the most part, to say whether there is mere chronic inflammation, or organic disease, such as tubercles, of various

descriptions—at least I cannot distinguish between them—unless you can feel tubercles forming upon its surface, or feel a considerable enlargement of the organ. But it is only when you find, notwithstanding all the rational means you can employ, and employ in full force, that no progress is made against the disease, that you begin to suppose there is something more at the bottom than inflammation.

In this case it is wrong to go on with mercury, or with antiphlogistic measures, to any extent. You will only increase the mischief by such measures. You must support the patient well. For the most part he will sink gradually under his complaint, but you must take care that you do not make him sink sooner than he otherwise would. In tubercular, and most other organic diseases, by supporting the patient well, tranquillizing him with opium, and attending to the general state of the constitution, you may protract life, and mitigate suffering, although you cannot cure the disease.

Treatment of the “ Terminations,” or Effects of Inflammation.

I now proceed to the consideration of what are called “ the terminations of inflammation ;” but before I do so, I must beg to urge the necessity of exercising great care not to adopt the idea of the existence of organic disease, either too hastily or too late. If you take it up too hastily, you will not subdue inflammation that might otherwise be subdued ; but if you take it up too late, and go on treating the disease as inflammation that may be subdued, you will certainly hasten dissolution, and cause the patient to sink so much the sooner.

In the treatment of what are called terminations, you will recollect what I stated formerly, that it does not necessarily happen that the inflammation ceases, because what are called “ terminations” take place. They are circumstances which take place, but the inflammation does not cease because these circumstances occur. We must always inquire, when any of these circum-

stances occur—such as effusion, suppuration, or mortification, whether inflammation is still present or not—if it be present, to what extent, and likewise of what character; whether it is active and tonic, or is attended with debility; and, accordingly, the same remedies may be necessary which were requisite in the treatment of mere inflammation before these “terminations,” as they are improperly denominated, took place. You have to treat the inflammation present accordingly as it is active, or more or less passive—accordingly as it is tonic, attended with strength of constitution, or accordingly as there is no power of constitution, whatever may be the activity of the part itself.

Effusion and Suppuration.—You have also to treat these things as inflammatory or not inflammatory, because sometimes there is no inflammation. If it so happen that fluid is generated, whether that circumstance has taken place which is called *effusion*, and which is merely an excessive secretion or collection in a serous membrane, to any amount, or if suppuration have taken place, so that matter has collected, it may be necessary to let out the fluids in either case, because fresh symptoms may arise from their pressure. This is frequently necessary in a collection of pus, and also in the case of mere effusion or the collection in a serous membrane. You may liberate the lungs, and avert impending danger, by allowing the escape of fluid effused in great quantity into the thorax; and some say that you may do the same with respect to the pericardium, but of that I never knew an instance; it sometimes may be necessary in the case of the peritoneum. With respect, however, to the pus itself, the escape of it by art is of the utmost importance, because it is found that it not only injures by its mechanical pressure and distention, but it frequently causes great mischief to the constitution by its peculiar properties. You will sometimes see, if the matter be confined, the system in a state of great depression, the tongue brown, and the pulse feeble; and on making a free incision, and letting out this fluid, the constitution will rally, perhaps, in the course of a day. When there is a mere collection of serous, or diseased serous fluid, frequently diuretics are very useful; but for the most part, the remedies of inflammation, moderately continued, are among the best means. By still giving mercury, by continuing low diet, you cause absorption of the fluid to take place, and you do much better than merely cause absorption of it—you prevent its increase, and if you can but effect that object, in a large majority of cases, nature will get rid of it by herself. Among the remedies that answer both purposes, colchicum and digitalis will be found useful. They will perhaps lessen the action that is going on, and keep

down the fluid, and also tend to lessen previous effusion. Colchicum, from exciting the kidneys, and also the alimentary canal, is particularly suitable, and so is mercury.

If a discharge have been accomplished by art, or if nature herself have effected a new opening, then you find it necessary to support the constitution well, for the purpose of enabling her to bear this copious discharge; but still it is frequently necessary at the same time to adopt more or less of an antiphlogistic regimen, to give abundant nourishment, but not, indeed, of a stimulating kind.

Mortification and Ulceration.—With respect to these “terminations,” as they are called, you may see a good reason for not supposing that the necessity of antiphlogistic treatment is over when they occur even in the instance of mortification and of ulceration. An ulcer is frequently owing to nothing more than the inflammation around. In the case of a sore nipple, I have seen this over and over again. When the breasts have been so sore that the mother dreaded the agony of suckling her child, by merely applying leeches at some little distance from the spot, but still on the breast—subduing the inflammation there, and afterwards keeping it down, the ulceration has frequently got well. So in the legs: it is very common for an ulcer to take place in them, and yet not to heal, simply because there is so much inflammation around. Here plaisters and ointment are of no use; but if you bleed the patient well, and purge him, you take off the tension and fulness of all the vessels—you subdue the inflammation, and nature then has power of her own to heal the ulcer. It may be just the same with respect to all the terminations of inflammation.

In mortification even we have to consider whether there is inflammation going on or not, and whether it is attended by tolerable strength of constitution or not. Inflammation may attend mortification after it has begun, when it is decidedly going on. The inflammation may be really active; there may be great redness of the part, great pain, and great heat, and there may be tolerable strength of the constitution at large. On the other hand, the inflammation may be more of a passive kind; the part may be dingy, swollen, and not in much pain, and the strength of the constitution may be in a deplorable condition. It is often necessary, in mortification particularly, to employ local antiphlogistic means while you support and sustain the system at large. The system itself is in a bad state, and therefore you have to support it with the best of nourishment, and perhaps wine, or even brandy; whereas the part itself is disposed to run into a state of excitement, and mischief arises from such treatment, unless you prevent the action of the part itself, perhaps by the application of

leeches, and perhaps of cold. Occasionally, of course, in mortification, it is wrong to employ soothing measures; you must adopt means of the most stimulating description; but it is to be remembered, that often the more you stimulate the part, the greater will be the mortification. If there be great irritation, you will find a poultice of carrots or turnips answer a very good purpose. The more you irritate, in many cases, certainly the greater will be the extent of the mortification. If, however, stimulus be absolutely necessary, then the oil of turpentine, I believe, is the best. Sulphuretted oil is often very good. I have seen the mortification apparently stop through these applications, when the constitution was well supported; but it is to be remembered, that these things are only to be employed when there is no violence of local inflammation.

It is always an object to prevent the gangrenous parts from injuring those that are not yet in a mortified condition. There can be no doubt that putrid animal substance is in the highest degree poisonous; and it is very necessary to prevent it from acting on the living part, and to prevent also the stench which proceeds from it. The stench itself may be removed pretty easily by charcoal powder put into a fermenting poultice. Stale beer grounds and yeast are very serviceable; but, above all, I should recommend the chloride of lime or soda to be continually applied. It is necessary, however, in using these applications, to remember that they must be continually applied—not two or three times a day, but as one portion of the solution of the chloride dries, so a fresh portion must be employed to counteract the putrescent tendency. You see them, for the most part, inefficiently employed; and, unless you look after nurses and attendants, they will not apply them sufficiently to remove the stench.

In mortification, bark was formerly much praised, and was even thought to be a specific; but that cannot be the case, for sometimes there is great excitement of constitution, requiring antiphlogistic means. At other times, however, in an opposite state of the system, where there is mortification with debility, bark may be of great use, and especially sulphate of quinine; for, in mortification, it is common for the stomach to reject bark. A combination of musk and ammonia I have frequently seen arrest mortification, at least apparently; I say apparently, because in such a case it is your duty to support the constitution well at the same time; but musk and ammonia united, and given every few hours, have appeared to answer a good purpose, and have been extolled particularly by some practitioners. In nearly all these cases of mortification opium is useful. There is great general suffering, great general restlessness;

and it is an object to lessen the suffering and tranquilize the system at large.

It might be supposed that, if you removed the mortified part, you would put a stop to the mortification; on reflection, however, you will see that it would be a bad practice. The mortification of a part depends generally upon the state of the constitution; and the fresh wound caused by amputating the mortified part will fall into the same condition, because it is not the part that is so much in fault as the constitution. If, however, the mortification depends entirely upon local circumstances, you may cut off the mortified part with safety. Supposing an artery has been injured to such an extent as to cause its obstruction, and the part supplied by it dies from the want of blood, the part may be removed without any danger whatever of the disease spreading, because the mortification depends upon a local cause, and not upon the constitution, and the fresh wound will not fall into the same predicament as the mortified part. In almost every case, however, where the mortification does not depend upon a local cause, it is the best practice to wait till nature herself has pointed out that the mortification will not spread—till a line of separation has begun—till you see that the powers of the constitution are sufficient to repair the injury—that the constitution is not in a condition for the mortification to spread higher up, but that, on the contrary, nature is capable of the healing process; then you remove the part, and effect the object that nature herself was carrying on; and you do it better. It is of great importance to recollect these few particulars in the treatment of mortification.

Specific Inflammation.

The observations that I have now made refer to common inflammation; but I stated that inflammation may be also specific—that we term inflammation specific when it runs a very peculiar course, or arises from a peculiar cause. Gout is termed a specific inflammation, because it runs a very extraordinary and very peculiar course. Small-pox is also an instance of specific inflammation; it not only runs a peculiar course, beginning on a certain day, and undergoing certain changes on others, but it arises from a special cause, which will produce no other disease. Gonorrhœa, also, is a specific inflammation, having its origin in a cause that will induce no other disease.

Treatment—In the treatment of this species of inflammation we sometimes have prophylactic measures, and can prevent these affections altogether. The disease of small-pox may be prevented, in the majority of instances, by the cow-pock. Then for some of these we have a specific remedy; for itch, which is a specific disease, we have sulphur; for syphilis, we have mercury. For the greater part,

however, of these specific inflammations, we have no specific remedy; and even if we had, it would still be of the highest use to treat them on the principles of common inflammation. Where we have certain remedies, as in the case of itch or syphilis, still we must keep in view the remedies of common inflammation, because you must recollect, that specific is common inflammation with something superadded to it; and by adopting common treatment, in addition to specific, the cure is much accelerated.

Some inflammations are specific at one time, and not at another. There can be no doubt that, in general, ophthalmia is not a contagious disease; but we have every reason to believe that sometimes it is. Even erysipelas has appeared to be contagious. I think I have seen a person die from erysipelas caught in this way; at any rate, as I shall mention hereafter, you may see some strong reasons for supposing it to be contagious, in a paper published by Dr. Wells, late of St. Thomas's Hospital. Catarrh is sometimes specific. Common cold is an instance of common inflammation; but now and then there is an epidemic catarrh, which depends upon some peculiar cause in the atmosphere, and not on the common causes of inflammation. It is a singular fact, that inflammation may be specific at one part of its course and not at another. Gonorrhœa is an instance of specific inflammation at first, but no doubt after a time it ceases to be contagious. When that time arrives we do not know, and therefore it would be bad policy to speculate on the presumption of its having occurred; but I think it is pretty certain that inflammation in the urethra, sufficient to keep up a discharge, will continue long after any contagion is to be feared.

OTHER GENERAL AFFECTIONS, FREQUENTLY INFLAMMATORY.

I mentioned that not only the chief essential symptoms of inflammation would arise from other causes—such as swelling, such as pain—but even other circumstances occurring in the course of inflammation, and those circumstances which are called “terminations of inflammation,” will not only take place with inflammation, but sometimes without it.

Inflammatory Discharges or Collections.

Now, among the circumstances which take place in the course of inflammation, or among the terminations of it, are discharges and collections. These may be sometimes mere results of inflammation, or they may be closely connected with inflammation, and sometimes they may occur without any inflammation at all. One of these discharges consists of blood, another is a discharge of mucus, another is not a discharge, but a col-

lection of serous fluid. A discharge of blood is called a *hæmorrhage*; a discharge of mucus is called a *flux*, or a *catarrh*, or a *gleet*, or a *profluvium*; the excessive collection of serous fluid that cannot be discharged, is called a *dropsy*. I shall therefore embrace this opportunity, before I come to the consideration of changes of structure, which are frequently the result of inflammation, though sometimes not, to consider in general hæmorrhages, fluxes, profluvia, and dropsies. These are frequently the mere result of inflammation; frequently they are closely connected with it, though perhaps not altogether the result of it; frequently they occur without any inflammation being present, and they will frequently continue long after inflammation can be supposed to exist. We shall find it just the same with respect to organic changes. Changes of structure, induration, and softening, are every day the effect of inflammation, but every day they appear not to be so.

Hæmorrhage.

The first of these of which I will speak, is hæmorrhage; which frequently occurs in inflammation of a mucous membrane, particularly of the mucous membrane of the intestines and of the urethra, and less frequently in inflammation of the bronchiæ; but it is a common occurrence to see the most violent inflammation of a mucous membrane without the escape of a single drop of blood. Why it happens that inflammation of a mucous membrane should in one case be attended by hæmorrhage and in another not, I do not think we can explain; but, nevertheless, such is the fact: inflammation of a mucous membrane may occasion the most profuse discharge of blood—or, at least, a discharge of blood, and frequently this will take place without any violent inflammation. You see that inflammation exists, but you cannot believe it is the sole cause of the hæmorrhage. That the hæmorrhage is inflammatory, is all we feel justified in saying; but it is scarcely the result of an inflammation; and in other cases we may see no symptoms at all of inflammation.

Active.—Now if hæmorrhage occur with signs of inflammation—with pyrexia, with a pulse full, or hard, or quick, together with pain and heat—we then call it an active hæmorrhage; just as we should call an inflammation active under similar circumstances. The disease is then to be treated simply as inflammation—we have to treat the inflammation, and the hæmorrhage will subside. We have in general to bleed the patient, and we most likely find the blood buffed and cupped; but not, of course, in every case. We have to purge the patient as in inflammation, and it is often quite safe and proper to give mercury. We must keep the patient on the same low diet that we do in inflam-

mation, and apply cold. Cold is particularly useful in such a case.

Passive.—But, like inflammation, hæmorrhage may be passive. There may be no pain, or but little pain; no heat, no pyrexia, no quickness and fulness of pulse; or, if there be quickness, the pulse is at the same time feeble. There may be, in such a state of things, congestion in a part, or there may not—that is to say, all the veins may be distended, or the extremities of the vessels only may be pouring forth blood without any congestion. When a hæmorrhage takes place without any symptoms of inflammation—without pain, without heat, without fulness of pulse, or quickness—the part itself may be in a state of congestion; but, on the other hand, that is sometimes not the case. You may, on opening a patient, find great congestion of all the vessels, and, in other cases, you find the most extreme paleness. Where there is a considerable discharge of blood, and no signs of inflammation at all, and symptoms of great fulness of blood in the part, I believe in general it is venous. This sort of hæmorrhage will sometimes be altogether mechanical. There may be an obstruction of some of the leading veins, and the blood may be poured forth, of course, mechanically. An obstruction in the spleen, or in the liver, is a common cause of a discharge of blood from the intestines; but this discharge will sometimes take place without any obstruction, in a peculiar state of the body which is not well understood. For instance, in typhus fever, in small-pox, in scurvy, and in that peculiar disease called *purpura*, one cannot tell why blood should be poured forth: but it frequently is, and that in great abundance, and yet there may be no signs of inflammation. A solution of continuity in some of the large vessels, of course affords another instance of passive hæmorrhage: there is no inflammation there. You have mere passive discharges from the want of mechanical resistance; but the want of mechanical resistance will not explain all these cases of passive hæmorrhage, because you have cases of the most extreme debility of body, where the patient has no hæmorrhage, and frequently there is hæmorrhage of a passive kind without inflammation, where there is not such an intense degree of debility.

Although it is generally the mucous membranes which pour forth blood, the skin, without any abrasion, has been known to do the same. I never saw an instance of this kind, but I have read of bloody sweats; and they occur in persons who have been bitten by some poisonous animals. Curious cases are to be met with in authors, of blood oozing from various parts of the body. There is an account of a person, in an American journal, from whose cheek blood suddenly oozed in

a considerable quantity without there being any abrasion.

Ulcers will frequently afford a great quantity of blood, but this especially occurs in women whose menstruation is deficient. There have been some curious instances in regard to the vicarious discharge of bloody fluid. If menstruation, however, be suppressed, it is not uncommon for women to have an ulcer break out on the legs, and to bleed once a month; or if there have been an ulcer before, for it to take on a bleeding character. I have seen hæmorrhage from the chest take place in the same way; an hæmoptysis has taken place once a month, simply because the menses were suppressed.

It is to be remembered that these two hæmorrhages, active and passive, just like active and passive inflammation, run into each other; and it is sometimes difficult to say what treatment we ought to adopt;—in fact, we are under the necessity of adopting both plans—of keeping down activity in a part, while we moderately support the system at large. You have to adopt just the same treatment that I mentioned as necessary in a doubtful combined case of active and passive, or tonic and atonic inflammation. Accordingly as the pulse is not quick, accordingly as it is not full and strong, accordingly as there is little pain or heat, you may adopt the remedies for passive or atonic inflammation. In proportion as you find the pulse strong or quick, provided there is still a certain degree of strength of the system, and as you find heat in the part, so you must adopt antiphlogistic measures more vigorously. Local means must sometimes be had recourse to, for diminishing excitement, while you support the constitution.

But if you find the hæmorrhage is decidedly of a passive character, so that it is out of the question to think of treating it as inflammatory by bleeding and purging, you have to trust more to the application of cold to the part itself, and to local, and perhaps even general, astringents.

Cold.—Cold may be applied to the exterior of the chest, in spitting of blood, with the most perfect safety. I have known phthisical patients sit up in a chair, with scarcely any thing upon them, and ice laid on their chest, without the least suffering being produced, or any unpleasant consequences resulting. In hæmorrhage from the kidneys, it is also a good practice to apply ice to the loins; and in cases of a profuse discharge from the alimentary canal, I should not hesitate to give ice by the mouth and rectum, and place it on the abdomen.

Supercetate of Lead.—With respect to internal astringents, I doubt very much the efficacy of nearly all of them, except in the case of hæmorrhage from the alimentary

canal, where you may employ catechu or kino with great advantage. In this case, you apply the remedies as locally to the part itself as if you applied them to the surface of the body. But with respect to those astringents which are intended to be absorbed, and thus to operate on a distant part of the body, I very much doubt their effect, unless they are of a saline character, or readily absorbed. There can be no question of the efficacy of superacetate of lead; I have proved it to my own satisfaction over and over again; but with regard to vegetable astringents, I very much doubt whether, in a case of profuse hæmoptysis, you can do good, if you fill the stomach and intestines with catechu and kino. You may operate, however, on distant parts by means of metallic astringents, provided you give those that may be absorbed, and the superacetate of lead is certainly the chief of these. In regard to the dose, one would not imagine beforehand, knowing the ill effects of lead, that you could give the large quantity that really may be borne. Not only may one or two grains be given every six hours, but two or three grains may be exhibited every two or three hours, for some days, with the most perfect impunity. I know you may give a scruple of it in the twenty-four hours, and even more, nearly half a drachm, with perfect safety, provided you carefully attend to the state of the patient's bowels; and I know that such doses are sometimes necessary, because I have found no effect produced till I have brought the dose up to that point. For a time, there is no occasion to push on the doses so quickly; but if you see the hæmorrhage obstinate, it is then necessary to do so. I have seen a stop put to the hæmorrhage when the dose had arrived at a certain point, and the hæmorrhage return when the dose was lessened from a desire of not overdoing it. The only inconvenience I have known occur, has been violent pain of the limbs. It is necessary to attend to the patient's bowels; to take care that they are freely opened every day; for it would be madness to give it otherwise: but it is perfectly right, I believe, in passive hæmorrhage, to give superacetate of lead very freely. The pain of the limbs is of no consequence if you save life; a patient on such terms may bear pain some little time after his recovery; but these may generally be removed by freely employing the warm bath once or twice a day, and the administration of colchicum.

Sulphuric Acid.—There is another astringent, which, when taken into the stomach, exerts an influence over a profuse discharge—not hæmorrhage, but profuse sweating. I think there can be no doubt that sulphuric acid does check profuse sweating. I think I have seen the same effect result from nitric

acid; but sulphuric acid will undoubtedly check the most profuse perspiration.

Oil of Turpentine.—The application in passive hæmorrhage of cold, and the application of astringents with the cold, and the administration of astringents internally, are among our chief measures; but you will frequently find the oil of turpentine answer a most excellent purpose. We have all seen it undoubtedly stop profuse hæmorrhage from the nostrils, by being introduced with a plug of lint; and I have seen a similar effect produced in hæmorrhage from mortified parts. I do not know that it will stop hæmorrhage in the internal parts, except from the alimentary canal and the kidneys; but it will stop hæmatemesis and melæna, that is, a discharge of blood from the stomach, upwards or downwards. I have had cases, almost innumerable, illustrating the great utility of this remedy. It so happens, that during the last six months I have had a large number of such cases, more than I ever had during the former period of my practice, and this has proved a most efficacious medicine. It should not be exhibited in large doses; that would do harm, by producing vomiting and purging, which, in such cases, might be fatal to the patient: twenty drops should be given every three or four hours. I have also seen it stop hæmorrhage from the kidneys. I have known people take it of their own accord where I should have been afraid to give it. I have known it taken in active hæmorrhage from the kidneys, and put a stop to it; but where the hæmorrhage was active, I would treat it as inflammatory, and then, after a time, give oil of turpentine.

Compression and Actual Caution.—If the hæmorrhage proceed from a part you can get at, and you cannot succeed in stopping it by other measures, I need not say that compression may be necessary by means of a ligature, compress, &c. or the application of the actual cautery.

Predisposing Causes.

Age.—It is in young persons that we have active hæmorrhage, and in middle age and advanced life that the passive form generally occurs; but, of course, there may be exceptions to this general rule. In children, it usually takes place from the nose; in young adults from the lungs and air-passages; in elderly people chiefly from the abdomen, but they are likewise subject to hæmorrhage within the head, producing apoplexy.

Women, both old, young, and middle aged, are very subject to hæmorrhage from the stomach, and also to hæmorrhage from the intestines, though in a much less degree than to hæmorrhage from the stomach. This hæmorrhage in females from the alimentary canal, is generally of a passive kind, and

will generally bear astringents in the first instance; it does not require any great extent of bleeding.

In the menstruating period of a woman's life, the uterus is very subject to hæmorrhage, and this is often inflammatory. Many, many cases of menorrhagia, without pregnancy, without delivery, have I seen yield presently to one or more bleedings and low diet; whereas, all the astringents you could give would be of no use in inflammatory hæmorrhage attended with pain of the loins, heat of body, and quickness of pulse.

Constitutional Tendency.—Besides this predisposition to hæmorrhage from certain parts, depending upon age, there are others depending upon other circumstances. Certainly, sometimes it is a constitutional tendency. Certain persons are known to be subject to hæmorrhage from certain parts, and some persons are subject to hæmorrhage in general, so that if they have a tooth out they will always bleed profusely, or, if they cut their finger, it will continue to bleed for many hours, in spite of means that are used. There is certainly an hereditary predisposition to hæmorrhage in many people. In some of these, an extreme thinness of the arteries has been found. After a tooth has been extracted, it has been found necessary, from the profuse hæmorrhage, notwithstanding the actual cautery was employed, to tie the carotid artery; and it has been found that the arteries were even thinner than the veins in other people.

Exciting Causes.

The exciting causes of hæmorrhage are, whatever propels the blood violently, either generally or locally; and whatever mechanically accumulates it, as posture, the application of a ligature, or obstruction in any part. Another exciting cause of hæmorrhage is the suppression of a discharge, or the sudden cessation of a natural discharge,—the entire or occasional suppression of the menses; the suppression of hæmorrhoidal discharge; the suppression of diarrhœa; and, indeed, the sudden cessation of a disease to which a patient has been very subject, although not an hemorrhagic disease, is sometimes attended by hæmorrhage of an internal part. I need not say that a solution of continuity is a common cause of hæmorrhage, and of course it is then of a passive kind.

Character of the Effused Blood.

In hæmorrhage which proceeds from the nose or the lungs, the blood is generally florid; whereas that which comes from the stomach and intestines is generally of a venous character. But the circumstance of the blood being black, does not necessarily prove that it comes from a vein; it may assume that

colour from remaining some time in the stomach or intestines, before it escapes; because blood, if allowed to be perfectly still, and not exposed to the air, will become black. If you tie an artery in two places, the blood between the ligatures becomes venous in its character; and therefore blood discharged from the stomach and intestines, while it may be venous, may be black merely from staying a considerable time in that situation previously to its discharge. In apoplexy, the blood effused within the head appears to be venous; but it may be black merely from the reason I have just stated.

Causes of respective Hæmorrhages.

The uterus is particularly subject to hæmorrhage during pregnancy and during and after delivery, but then the reason of this is for the most mechanical. An inflammatory state very often produces hæmorrhage from the intestines and from the urethra. Inflammation, if not the cause of menorrhagia in women who are not pregnant, and have not lately been delivered, is, at any rate, a very common attendant circumstance. Menorrhagia, that has nothing to do with the impregnation of the womb, is generally of an inflammatory character.

In the head, and from the lungs, the stomach, and the intestines, we often have hæmorrhage from merely mechanical causes—from ulceration, or solution of continuity. One cause of hæmorrhage in the head—apoplexy, is brittleness of the vessels, or tenuity of them, or ulceration of the bloodvessels of the head. Hæmorrhage from the lungs often arises from a mere inflammatory state of the bronchiæ or of the air-cells. We sometimes have hæmorrhage from ulceration in phthisis: although nature endeavours to prevent hæmorrhage by forming adhesions, and plugging and contracting arteries, yet sometimes an ulcer will in phthisical cases produce hæmorrhage, and sometimes sudden death. With regard to the stomach, hæmorrhage will sometimes occur from ulceration, and death may be the consequence. The same thing will take place in the intestines; there may be an ulceration of even a malignant character, and every now and then profuse hæmorrhage is the consequence. It will also of course come on in many of these parts from mechanical violence. It is very common for a person to spit blood after a blow on the chest or a fall; it is common to make bloody urine after a blow upon the loins, and to discharge blood from the intestines after a fall.

DISEASES OF THE HIP-JOINT.

Abstract of a Clinical Lecture,

BY MR. EARLE.

MR. EARLE adverted to the case of James Rowley, æt. 12, a little boy of fair but scrofulous complexion, who was admitted some months back into Pitcairn's ward, with disease of the hip-joint, of two or three months' duration. His treatment consisted of absolute rest over a double-inclined plane, and the application of issues, and attention to his general health. For a considerable time he seemed to be improving, but latterly his health has given way; the head of the bone has become dislocated into the ischiatic notch, and a large abscess, which has been opened freely about two inches below Poupart's ligament, is undermining his strength. Matter has also made its way through the acetabulum into the pelvis, and communicates with the bladder; large quantities of pus passing with the urine.

Mr. Earle also referred to two other cases of diseased hip—namely, that of Henry Rook, in Powell's ward, with scrofulous affection of the cancellous structures and slight shortening of the limb, but no dislocation; in which case abscess has also formed, and made its way to the surface, through the issue in front of the joint.

Lastly, to the case of Eliza Moore, in Elizabeth's ward, with acute inflammation in the hip-joint, probably confined to the synovial membrane. Mr. Earle took this opportunity to speak generally respecting diseases of the hip-joint. He stated, that there are two affections to which this joint is more especially liable—ulceration commencing in the cartilages, and a scrofulous affection of the cancellous structure of the bone.

The first of these, ulceration commencing in the cartilages, is by far the most frequent affection. It is of the first importance it should be detected at the very outset; for it is only at this period we have any probable chance of being serviceable to the patient. We should, therefore, carefully look into, and endeavour to become familiar with, the symptoms which may be said to usher in the disease. There is always more or less lameness present, producing a very peculiar gait. The patient never bears his full weight upon the joint, but places the affected extremity more or less at an angle with the pelvis, in order to avoid the direct perpendicular bearing of the trunk upon the head of the bone. That side of his pelvis also sinks, and, as speedily as possible, he throws the weight of the body upon the other extremity. The pain he suffers at this early period is but slight and occasional, being, however, gene-

rally increased towards night. This pain gradually becomes constant. After the disease has existed some time, a remarkable flattening of the buttock may be observed, on examining him from behind, and the trochanter appears to project further than in the healthy limb. This appearance is referrible to the wasting of the muscles of the buttock, which is constantly observable in the neighbourhood of diseased joints. The patient will now complain of pain in the front of the thigh, extending to the inner side of the knee, and even down to the foot; arising from a painful sympathetic affection of the anterior crural nerve, which passes over the affected joint. After a little while, much suffering and inconvenience are sustained from the spasmodic contraction of the muscles, occurring especially at night, and increasing with such severity that the patient is kept in a continual state of watchfulness and alarm, lest the bone should be torn from its socket. Long before the displacement occurred in this little boy Rowley's hip, he was observed to keep his hand over the thigh, lest the slightest movement might throw it out of its situation. His whole body, indeed, was remarkably curved and distorted, which, in some measure, explains the unusual displacement of the head of the bone into the ischiatic notch.

In the progress of the disease, abscess will form within the joint, and make its way externally; large collections of matter form also around the joint, taking various directions, and even sometimes extending within the pelvis, as happened in this little patient's case. Matter, before it was evacuated, could be felt above Poupart's ligament; and, from the circumstance of pus having subsequently passed with the urine, there is reason to fear the abscess communicated with the bladder or ureter, an instance of which Mr. E. never saw, although he has known a case (Sir A. Cooper relates a similar) where the pus formed a communication with the rectum. Next, the ligamentum teres becomes destroyed, and the brim of the acetabulum is rendered much more shallow by the ulcerative process, thus forming a much less effectual barrier to the displacement of the bone, which eventually takes place; a great portion of the head and neck being sometimes previously absorbed. Sometimes, however, the bone is, as it were, pushed out of its place by the deposition of pus and lymph in the cavity; and this change is evidenced by the great *actual* projection of the trochanter, as compared with that on the opposite side, to be distinguished from that *apparent* projection of this process which is produced by the flattened state of the buttock upon that side, always to be observed in these cases when a little advanced, and which has been already adverted to.

In cases where spontaneous dislocation has taken place, the limb takes pretty much the position it does in common dislocation, on the *dorsum ilii*; there is considerable shortening and inversion of the foot, the arch of the affected foot being turned over the tarsal arch of the opposite one.

In the other affection of the hip there is a gradual softening of the cancellous structure of the bone, with a deposit of serofulous matter into its interstices; commencing in the cancellous texture, the disease gradually spreads to other structures.

When displacement occurs in this form of the disease, the position is not similar to that which prevails in the other: the extremity is generally slightly bent, while the foot is somewhat everted. It is not so decided a dislocation as in the other case, but results from the crumbling down of the substance of the bone. The case of Rook, in Powell's ward, appears to be of this description.

In the former variety there is a greater disposition to the formation of abscesses, the contents of which are generally of a healthier character than in those formed in the neighbourhood of the serofulous bone. It is also much more rapid and destructive in its effects than the serofulous form.

However, in either case, unless we see the patient very shortly after the first set of symptoms have manifested themselves, we cannot expect to be of much service.

Generally the patient is gradually worn out by the continued constitutional irritation caused by so abundant a flow of pus for so long a period. In some cases, however, the discharge of pus abates, and ankylosis takes place, which is the most favourable termination we can now look forward to. There are, again, some very rare cases in which no dislocation has taken place, and in which no abscess has formed: here the cartilage lost by ulceration is not reproduced; but, after a while, there is formed upon the ends of the bones a hard, shining, ivory-like substance, similar to what we sometimes find interposed between the ununited portions of a fractured neck of a thigh-bone. This substance bears friction without exciting pain, and permits the motions of the parts to be continued; although, on laying the hand over the joint, a peculiar grating sensation is imparted to it whenever the limb is moved.

In treating these cases, when far advanced, we should bear in mind that our object is to procure ankylosis, in a position which will be most beneficial to the patient hereafter. Mr. E. knows a case where the limb is ankylosed in a perfectly straight line. This is convenient enough as long as the person is walking, but in other positions it is very troublesome. Thus the individual can-

not, when in the sedentary posture, bring that side of the buttock upon the chair, but is obliged to sit upon the edge of a high stool, with his leg extended out. Unfortunately, however, we cannot often do much in these cases; for the patient will lay in the position from which he derives greatest ease, notwithstanding our endeavours to correct it, should it be an erroneous one as regards his ultimate comfort.

When we meet with cases in the early stage, we should lose no time in applying remedies, as local depletion, counter-irritation, attention to the general health, &c., and especially in enjoining absolute rest. If the two inflamed surfaces be allowed to move over each other, even in ever so small a degree, it would be absurd to expect much good from any remedial means. There are no cases in surgery which so indispensably call for perfect rest as these; and if you can secure it in the commencement of the affection, you will frequently have the satisfaction of restoring to the patient the use of his limb. Mr. Earle conceives much good would result if the great remedial agency of quietude in surgical diseases were more sufficiently known and appreciated. It is often of the utmost importance to know when and for how long to employ rest, and when to permit and advise exercise of a part. In the present case half-measures will be of no avail; you must insist upon entire rest, notwithstanding the entreaties of parents and friends, who, not seeing clearly the object you have in view, often importune you to relax your discipline. The hip-joint is the centre of motion of every movement of the lower extremity upon the trunk; and then again every movement, even the most trivial, of the trunk, is conveyed to this joint; so that unless absolute rest, not merely of the lower extremities, but of the whole body, be maintained, we shall do but little good, and shall not prevent the attrition of the inflamed surfaces.

This perfect rest is best secured by the double-inclined bed; but a much less expensive apparatus may easily be constructed, to answer the purpose, and is, indeed, better adapted for children, who are by far the most frequent subjects of the disease, and in whom it runs its course much more rapidly than in adults.

A double inclined plane should be formed, by joining two portions of wood together in such a manner, that, when the child's limbs are made to correspond with the angle of junction, his legs and feet should extend down one plane, and there be confined to the foot boards by rollers, while his thighs and buttocks extend down the other. The foot boards will also have the beneficial effect of removing the weight of the bed-clothes from the feet, which often causes much suffering,

from the strain it produces on the affected joint. This object will be further assisted by having a proper bed-cradle, as in the case of Rowley, who cannot even now bear the slightest weight on his foot or leg. At the extremity of the plane for the thighs, opposite the anus, a small opening should be made, to admit the passage of the fæces. The whole trunk of the child should lie perfectly horizontally upon the bed, for if propped up, or suffered to move, the ends of the bones are rubbed upon each other, which cannot be as long as he maintains the perfect horizontal posture. To facilitate the placing a bed-pan under the aperture in the plane, the flock of the bedding for some distance around should be removed, and thus a space quite ample enough to place a vessel under the plane is obtained, rendering it quite unnecessary on the part of the nurse to raise the frame-work. By this means absolute and continued rest may be obtained; the parts may be kept in the most favourable and comfortable position, and the most powerful muscles become completely relaxed.

There are several affections which may be confounded with disease of the hip-joint. The first of these is of a purely nervous character, occurring in young hysterical females, who refer excessive pain to the region of the hip-joint, whence it often strikes down the thigh. Now it is of the highest importance to discriminate between these two. If it be not disease of the hip, treatment adapted for such disease would only aggravate the hysterical symptoms, by deteriorating the general health; while, on the other hand, if it should be actual disease, and yet treated as a hysterical affection, much valuable time, never to be replaced, would have been lost. In this hysterical affection you will find acute pain complained of from the slightest touch in the neighbourhood of the hip-joint; but movements of the limb, which bring the various parts of the articulation in close contact, and which, in disease of the hip-joint, would cause excruciating pain, in this affection produce little or none.

Another affection much more rarely occurring, is inflammation and suppuration occurring in the bursa, beneath the conjoined tendons of the psoas and iliacus; but, besides the absence of acute sensibility on jarring the articular surfaces, by compression at the sole of the foot, if we cause the patient to lie on his belly, we shall not see that flattening of the buttock, and prominence of the trochanter, so characteristic of hip-joint disease.

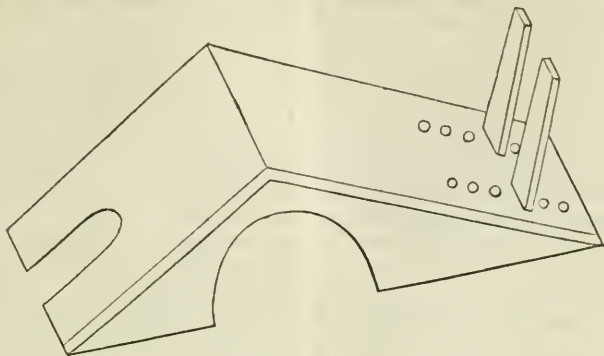
Lastly, we may have confounded with it a painful affection of the sciatic nerve. But in this case we shall have pain excited on pressing where the nerve comes out of the pelvis, some distance above the hip-joint;

as also by pressure in its course. In affections of this joint we generally, though not universally, find the sciatic nerve is unaffected, while the anterior crural suffers much, giving rise to those severe pains in and about the knee, which have sometimes been mistaken and treated for the original instead of the sympathetic affection. Occasionally, however, we have swelling following this pain in the knee, just as we have sometimes swelling of the testicle, which has been previously sympathetically painful, from a complaint of the kidney.

With respect to the employment of counter-irritation, Mr. Earle strongly advocated it in cases of ulceration of cartilage; and recommended the application of issues, or setons, in front of the joint, just at the edge of the tensor vaginæ muscle, in preference to the usual situation behind the trochanter, which renders more or less motion of the pelvis necessary when the wound is dressed. The issue, or seton, ought not to be too much in front of the joint, as the branches of the anterior crural nerve would be affected, and there would be danger of opening the inguinal vessels in using caustic potash.

Mr. Earle spoke at some length on the time and mode best adapted for opening abscesses in the neighbourhood of joints. The joint should be first tranquillized as much as possible, and the opening should be made at a depending part, and should be of sufficient extent to completely empty the matter as it is secreted. In opening all chronic abscesses, this point should be carefully kept in view. A mere puncture will often be followed by irritative fever, from allowing the matter to be confined and become putrid. Mr. E. next suggested the possibility in some rare cases, when the constitution was sinking under the discharge, of the employment of excision of the diseased head of the femur, similar in principle to the operations performed by Mr. Crampton, and others, on the knee and elbow-joints. It was, however, to be feared, that such an attempt would not be successful, from the diseased state of the acetabulum, which often remained a permanent source of irritation and abscess, after the head of the femur had become firmly ankylosed. Mr. Earle concluded by again very strongly urging the necessity of combining the strictest rest with counter-irritation, and attention to the general health.

Apparatus employed by Mr. Earle in cases of Disease of the Hip-Joint.



ON THE EFFECT OF NEUTRAL SALTS ON THE BLOOD,

Being an Answer to certain Papers and Observations by Dr. James Johnson, Dr. Hacket, and Mr. Greatrex.

[In order fully to understand the following paper, it will be necessary for the reader to keep in mind that Dr. Stevens, about two years ago, communicated to the College of Physicians a paper, of which we gave a copious extract, and in which he maintained that a change took place in the composition of the blood in certain malignant diseases, attended with the loss of its saline constituents. He farther argued that the red colour of the blood was dependant upon the *neutral* salts it contained, and that the most efficacious treatment, under the circumstances above alluded to, consisted in the administration of muriate of soda, nitrate of potass, and similar agents. In support of his views, he adduced the results of his own experience, and of that obtained at Trinidad after he had made known his views when on a visit to that island. But the truth of the statements made by Dr. Stevens have recently been denied by the editor of the *Medico-Chirurgical Review*, on the authority of Dr. Hacket and Mr. Greatrex, of Trinidad, these gentlemen admitting the diminished rate of mortality in the malignant fevers under their care subsequently to Dr. Stevens's visit to them, but denying that this arose in consequence of their having adopted his suggestions. As applicable to the subject of malignant diseases in general, but

more especially as connected with the use of common salt, so strongly advocated by some in cholera, and of which numerous successful trials seem recently to have been made*, the question is one of great practical interest. We may be permitted to say, that we have carefully perused the paper of Dr. Hacket, but that we have been wholly unable to discover in it any thing to warrant either the extravagant encomiums with which it has been ushered into the world, much less, *in any degree*, to justify the imputations which have been thrown on the veracity of Dr. Stevens. It appears, by the papers from Trinidad, that the fever which prevailed there had been exceedingly fatal. Dr. Stevens recommended the use of certain remedies: these were employed: the rate of mortality diminished. This is admitted on both sides. Dr. Hacket, however, says that the favourable result was the consequence of certain other agents, particularly the warm bath and sulphate of quina, which were likewise used. But all this amounts only to difference of opinion, and does not necessarily involve "*misrepresentation*," or "*petty larceny*," on either side: but we may with perfect safety leave Dr. Stevens to fight his own battle.—*ED. GAZ.*]

To the Editor of the London Medical Gazette.

Sir,—I do not mean to lessen the force of the serious charges which Dr. Johnson, and my other more able oppo-

* See our Supplementary Number of last week.

nents, have preferred against me by dwelling on subjects that are decidedly of minor importance; but if you will have the goodness to compare Dr. Johnson's letter and the Trinidad documents with the paper which was sent nearly two years ago to the Royal College of Physicians, you will find either that these gentlemen have but a sorry conception of my opinions with respect to the treatment of fever, or that Dr. Johnson, at least, has wilfully endeavoured to mislead the public, by giving a most unfair statement of my views upon this subject.

It has long been known that some of the neutral salts possess the power of reddening the blood, but it was not, I believe, generally known, at least until lately, that *every one* of the alkaline salts possesses the property of striking a red colour with the black colouring matter, and of converting even the blackest venous into the brightest arterial blood; and as a proof that this fact was not generally known, I may mention, that one of the latest writers on the blood in this country informs us, that the muriate of soda darkens its colour. It is also a fact, that the College of Physicians were so little prepared to admit the assertion, that *all* the alkaline and earthy salts, except those with a great excess of acid, redden the blood, as well as some other chemical facts which I had there stated, that they would not consent to the reading of that paper until they had first sent it to one of the ablest chemists of the day* to ascertain, by actual experiment, whether the circumstances which I had stated as chemical facts were really correct.

In the same paper I stated my belief, that the natural saline ingredients of the circulating current were in reality the true cause of the red or arterial colour of the blood, and whatever the colouring matter may be, that black is the natural hue of this substance, but that the whole of the alkaline salts possess the power of striking a rich arterial colour with this black substance. I observed also, that in the last stage of the yellow fever the blood, in bad cases, becomes black from the loss of its natural saline matter, but that by adding a small quantity of any one of the neutral salts to this black fluid, we could again restore it to its red or arterial appearance, even when the blood that we used had been

black as ink. The Trinidad physicians do not attempt to deny this; and Dr. Hacket admits, that the blood is invariably dark coloured and diseased in the West India fevers, without there being even "*one single solitary exception to this rule.*" The fact that the blood owes its red colour to its own natural saline ingredients is now, I believe, almost universally admitted by those numerous physiologists who have witnessed the experiments, which, in my mind, prove beyond all question that such is the case;—but of this hereafter. I stated also in the same paper that acids, alkalies, electricity, and in short every thing else that possessed the power of decomposing or destroying the neutrality of saline matter, destroyed the red colour of the blood, and made it perfectly black—I believe by decomposing or destroying that which is the true cause of its red colour.

But it seems, notwithstanding all this, that the Trinidad physicians have discovered that *alkalies* redden the blood out of the body*, but complain that the blood had not been reddened by *soda* in three men who had never taken one particle of that alkali, and who had died after having been carried to the Trinidad hospital half dead, and in the last stage of the yellow fever. This new discovery, however, that soda reddens the blood, either in or out of the body, is not correct, for the alkalies, as I have stated, do not redden but blacken its colour.

I have, for reasons which will afterwards be given, long adopted the belief that carbonic acid, and not carbon, is the cause of the dark colour of the blood in the venous circulation. I have long also been aware of the fact, that the aerial poisons which cause fever have an *acid effect*†, or rather an effect similar to acids, on the blood, and both darken its colour and otherwise change its appearance, even before they excite the disease. It is probable that I did mention these opinions to Mr. Greatrex, but it was neither soda, nor potass, nor any of the other alkalies, which I recommended these gentlemen to use; and certainly it is not my fault if neither Dr. Johnson nor themselves be acquainted with the difference betwixt a *pure alkali* and a *neutral salt*. It may be quite enough if I am made answerable for my own faults, but

* Dr. Prout.

* See Dr. Hacket's paper in Johnson's Journal.

† Ibid.

surely I am not accountable for the errors of others; and Dr. Johnson at least, who reviewed my paper on the Blood, and ought to have known better, has no right to put nonsense into my mouth which I never uttered; and, after having stated that all the alkalies blacken the blood, they have no right to call upon me to prove that soda can redden it, even in opposition to my own facts.

They state that soda had nothing whatever to do with the success in the above island: now this is a mere equivocation, for *I never said that it had*. I am glad, however, to find that they admit the success; for, by admitting this, they prove at least the one-half of my statement to be true. They state also, that after death the blood of "sodafied" patients was found to be precisely similar to that of the "unsodafied." Now if Dr. Johnson had allowed himself time to reflect on this subject, even for one moment, he never would have uttered any thing so absurd; for soda, as soda, or in other words in its alkaline form, had nothing whatever to do with the treatment of the Trinidad fevers. It was, as I have said, neither soda nor any of the other alkalies which I advised them to use, but the strongest, and perhaps the best, of all the non-purgative neutral salts, namely, a saturated solution of the muriate of soda, combined with the nitrate of potass. They admit that they did adopt the use of this remedy in consequence of my visit, and still continue, I believe, the use of it up to this day, *both in the proportions, and at the time which I had pointed out*; and I shall prove that, however numerous the deaths may have been previous to the commencement of this practice, yet from the moment that they began to attend to the diseased state of the blood, and adopted the saline treatment in their hospital, they had but few opportunities of ascertaining after death whether the blood had been *sodafied* or not.

"Permit me (says Dr. Hackett) to inform you, that every case that dies in our Military Hospitals, must be opened and reported on; and therefore the opportunities of the military practitioner for examining the human body, and viewing the morbid changes in its structure, are, perhaps, greater than that of any private practitioner, however great or extensive his practice may be." Dr. H., however, forgets to state,

that however numerous the deaths may have been previous to August 1828, yet, from the moment that they commenced, not the soda, but the saline treatment, they had scarcely a death from fever; consequently, though I admit that previous to my visit, there had been dissections enough, yet I shall prove that from that period their opportunities of examining dead bodies in the Trinidad Hospital could not have been so extensive as they had been before.

I have already referred to the three cases belonging to the Royal Artillery, where the men had been brought to the hospital in the last stage of the disease, and where, after death, the blood had been, as they say, in a state of "solution," but not sodafied.

The latest letter which I have from Trinidad is dated 4th April, 1830, that is, about twenty months after the commencement of the saline practice. During the whole of these twenty months, the Royals had lost, up to that date, only eight men from fever, even including three men mentioned by Mr. Greatrex as having died from organic disease in the lungs; and of the other five, two of them had died the same day that they had been brought to the hospital. Consequently, in the first twenty months which followed my visit, they had only three opportunities of examining the bodies of patients who had been under any thing like fair treatment; and these cases were probably not examined, or if they were, they have not given us an account of the result. These are facts; and from these it appears that, at all events, after the month of August 1828, up to the April 1830—that is, twenty months after they had commenced the saline treatment—their opportunities of examining the bodies, to ascertain whether the blood had been *sodafied* or not, have been exceedingly limited.

I know one private practitioner who, about the same time, but in a much shorter period, had three cases in his own establishment: the one of these was his partner, the other his apothecary, and the third his clerk. They were all treated according to the old system, with bleeding, calomel, &c.; and out of these three cases, which he had seen in the very beginning of the disease, he had as many opportunities of examining the bodies after death as they had had in the Trinidad Hospital (in the Royals at least)

during the first twenty months after my visit to that island.

During the first month after my visit to Trinidad, from forty to fifty fever cases were admitted into the hospital; they were all treated with the muriate of soda and nitrate of potass: out of this number three died, and all the rest, I believe, recovered. These three are the men to whom Mr. Greatrex refers, and probably the only cases in which he had an opportunity of examining the bodies after death; but I do not consider these cases as affording the slightest argument either for or against the saline treatment, for they were not fair cases either for this or for any other experiment. In the paper which was read at the College of Physicians, I stated, that, "where proper means are used to protect the organs from the increased excitement during the early stage of the disease, and after the excitement is sufficiently reduced—when proper nourishment is given, and certain saline medicines are timely and judiciously used—the bad symptoms are generally prevented. When proper saline medicines are used they do not fret the stomach; they act on the intestines as much as is necessary; they keep up all the secretions, particularly that of the kidneys, and enough is absorbed to enter the circulation and prevent the dissolution of the blood, and preserve it until the fever abates and all the danger is past. This I am warranted to state as a fact, inasmuch as this treatment was commenced in the West Indies in 1827; and since then it has stood the test in several hundred cases of the West India fevers, where it has been tried both by myself and others, and with scarcely a single loss when we were called to the patients within the first twenty-four hours after the attack, and with very few deaths where we were called in previously to the commencement of the fatal symptoms."

But though I said this, I did not say that the saline treatment was capable of re-animating the dead, or of saving soldiers from the grave who were brought to an hospital in the very last stage of the yellow fever, with their fate already fixed from previous neglect. The men alluded to were, all three of them, notorious drunkards; they had all been undergoing fatigue while exposed to the intense rays of a burning West India sun, and that, too, during the hottest season of the whole year; they had been

taken ill at a distance; they had not been bled in the beginning of the disease; they had been carried to the hospital during the heat of the day, and were exposed on their way to St. James's, while the fever was on them, to the direct rays of a burning sun. Two of them, at least, were brought into the hospital at a late period of the disease; and from these circumstances, but above all from their not having been bled in the first stage, it is more than probable that some serious injury had been done to the brain, and that the finger of death was already upon them before any attempt to arrest its progress could have been made. It is also admitted, even by the Trinidad physicians themselves, that it is only within the first twenty-four or forty-eight hours that good can be done by any treatment in these fevers; for, as they truly observe, after this the disease becomes, as it were, a labyrinth most difficult to unravel: consequently these three cases did not (and do not, in my mind) afford even the slightest proof against the saline treatment, or shew any thing else except neglect in the beginning, and imprudence in the manner in which these men had been brought to the hospital.

Twelve hours after death, the blood, in these cases, was found to be in a state of solution, but dark in colour. Now the truth is, that, in the yellow fever, death may be produced in one or other of two different ways: in the first place, by injury done to the brain, or to some other important organ, during the violent excitement which generally occurs in the first or inflammatory stage of the disease. But, even when they escape this, death may be caused solely and entirely by the diseased state of the vital fluid. When they die from the first cause, the blood may be in a state of "solution," as is said to have been the case in these men; but still it is diseased. I am not quite sure, however, that in these cases the diseased or "unsodified" state of the blood was the cause of death. It is true that, in these men, saline medicines had been thrown into the stomach, and that, too, at a time when this organ had probably ceased to perform its functions. Now, has it never occurred to these gentlemen, that if these men had escaped the injury which had probably been inflicted on the brain in the first stage of the fever, and if the diseased properties of the blood had been properly remedied by

the salts, they would not then have had an opportunity of examining their bodies after death, to ascertain whether the blood had been acted on or not by the neutral salts?—for these men, like the others, would probably not have died if they had been freely bled in the beginning of the disease; and after this, had had the saline medicines administered at a proper period, when they could have entered the circulation in time to prevent those fatal changes in the blood which are in reality, in these fevers, but too often, though not always, the sole cause of death.

The Trinidad army physicians appear to be perfectly aware that the saline treatment was, in reality, the cause of the great success in that island, and in point of fact, they admit almost as much; but they take great pains to conceal the circumstance that they had learned this practice from me; and the manner in which they do this is the only ingenious part of their whole proceeding. They use the word, saline, as seldom as possible, and endeavour to deceive their readers by talking constantly about soda, or other alkalies, thus trying to convince the public that these were the remedies which I had recommended them to use.

Dr. Hacket confesses that he is stupid enough not to comprehend how soda should produce a similar effect on the blood, in the living body, to that which occurs when soda is mixed with the same fluid out of its own vessels. He observes also, “considering, too, the very minute quantum of soda that can possibly pass from the stomach, direct and unchanged, into the mass of circulation, how small must be the means to produce such mighty effects.” Now what will the reader think of all this, who keeps in mind that, by their own statement, I never had advised these gentlemen to use even one particle of soda; and, however successful this sort of logic might have been with Mr. Greatrex, who was under his command, is it not an insult on the common sense of their medical readers to talk in this way, in the very same paper in which they admit that it was neither soda nor any other alkali that I had recommended, but the strongest of the non-purgative neutral salts? Is there even the merest tyro in the profession who does not know that alkalies and neutral salts are, in their effects, as opposite to each other as day is to

night; but above all, in their effects on the blood; for the former, as I have said, blacken arterial blood, and the latter convert even the darkest venous into the brightest vermilion hue? Now, if all this be true, it then follows, either that these gentlemen are grossly ignorant of this, or that they have tried to deceive the profession by the same sort of reasoning which they had found, perhaps, to be successful with those who are totally unacquainted with medical subjects.

I will not for the moment say any thing about soda, for with this in my practice I had nothing to do, nor had they in theirs; but, with respect to the action of the neutral salts on the blood, my belief is, that this sort of scepticism may be carried a great deal too far. That the salts do enter the circulation is beyond all question; for healthy blood, in every part of the world where it has yet been analyzed, has been found invariably to contain a given, and that too a large proportion of saline matter, to which it is indebted for its red colour, and in some degree for its stimulating power; and without this, the blood, as in the last stage of the yellow fever, becomes totally incapable of performing its functions. Dr. H. ought to be aware, that when strong salts, such as those which I recommended them to use, are taken into the stomach in excess, a great part enters directly, unchanged, into the circulation, through the medium of the vena portæ, and produces an instant effect, suddenly changing the properties of the blood; and when used in the last stage of fever, a part of the salts is retained in the circulation, particularly of those that are natural to the blood, while the excess, after having passed through the circulating current, is removed by the secreting organs, without having been decomposed either in the stomach or changed in their properties by the vital principle.

Had Dr. H. taken away small quantities of blood in the last stages of the yellow fever, from patients who had been treated under the old practice, as recommended either by Jackson or Johnson, he would have found that the blood every hour was becoming more altered in its properties, and blacker in its colour, chiefly from the loss, or at least from the great diminution, of its natural saline ingredients; or, had he made the same experiments on patients

who had been treated with the active salts, such as they used in consequence of my advice, he would have found in that case that the blood was hourly becoming, not merely more red in its colour, but less diseased in its appearance; or had he injected a small quantity of the same mixture into the veins of an inferior animal, he would have found that, even in living vessels, the vital principle presents no obstacle to the reddening of the whole mass of blood. He would have found also, that the action of the heart was not only augmented, but that all the secretions were immediately increased; and this even he himself admits to be a great object in the cure of fever. If, on the other hand, he had adopted a false theory (which has already been the cause of great mortality), and used any of the acids, as Chisholm and others had done, with the view of reddening the blood by means of their oxygen, he would then have found that the nitrous and other acids, which contain oxygen in an easily separable state, not only blacken the blood out of the body, and when used in the last stage of the yellow fever, kill, not quite so quickly, but as certainly, as if the patient had received a bullet through the head or the heart.

The Trinidad army physicians talk of having used the sulphate of quinine in the yellow fever, but forget to tell us at the same time, that this was neither a discovery of theirs, nor a new remedy in 1828. The practice of giving this salt in the yellow fever was commenced by the French physicians in the island of Martinique, as far back as the year 1823, and with considerable success. The sulphate of quinine, like all the other alkaline salts, reddens the blood out of the body, but it possesses this power in a much inferior degree to the muriate or the carbonate of soda, the nitrate, the chlorate, or the carbonate of potass. The sulphate of quinine has been long used, both by myself and others, in the islands of St. Thomas, St. Croix, &c., and we have found it a useful remedy; but we could only trust to it during the convalescence; and even the Trinidad physicians very properly did not use this remedy until, in ninety-nine cases in a hundred, the patients were out of danger—that is, until the diseased state of the blood had been previously corrected by other salts

which are much stronger in their saline property.

Dr. Hacket states, when I visited the hospital at Trinidad, that I admitted the similarity of their practice to my own. Now this is a mistake; for I knew from the rate of the previous mortality, that there must have been some radical error in their practice. I saw clearly what this was, and pointed it out to Mr. Greatrex. They make me to say also, the only difference in our treatment could not be great, for we were both in the habit of using *alkalies*. Now this is also a mistake; and I again repeat, that it was *not alkalies*, but the non-purgative *neutral* salts, which they afterwards used in consequence of my recommendation*. It is also stated, that I approved of bleeding in the warm bath, and that this, in the practice of both, was our *sheet-anchor*. Now, though I will not follow the improper example of these individuals, and assert that this is a misrepresentation, yet I do say, that in saying this they have fallen into a great error; for the fact is, that though I consider the free use of the lancet as essential in the first stage of the yellow fever, and said so to Mr. G.; yet, with respect to the warm bath, my conviction was then, and still is, that immersing patients in hot water is a most improper measure, particularly at the period which they used it—that is, in the first or inflammatory stage of the disease.

In an epidemic of the yellow fever, which commenced in the island of St. Croix in July 1817, after having found the mercurial and other methods of treatment in common use to be of no value, I then gave the warm bath a fair trial, but in every case that I used it the patients died; and from that moment to this, to the best of my recollection, I have not once used this measure. As I was anxious to prevail upon these gentlemen to give my treatment a fair

* They say that they had, even before my visit, used the super-tartrate of potass, as recommended by Jackson. This, however, is a most improper remedy, particularly where there is, as in these fevers, an excess of acid in the gastric organs. It is also true, that the salts which are formed with vegetable acids are decomposed in the stomach, and consequently are not to be trusted in these diseases; but the Trinidad army physicians appear to be totally unacquainted with this. Dr. H. states, that it was the wish of Mr. G. to employ the stronger salts, as recommended by me, and that he consented to the change; he is not, however, probably aware how many lives were saved by this great condescension on his part.

trial, I had no wish to fret them by finding much fault with theirs. But, under the above circumstances, I could scarcely have said that bleeding in the warm bath had been my sheet-anchor in St. Thomas; and it is well known in that island, that, in place of this measure, I invariably used *iced* water when it could be procured, for the purpose of removing the morbid heat from the body. I told Mr. G. that *ever since I had adopted the saline treatment* I had not lost one case, where I had seen the patient early in the disease. In place of this they make me say, that I had lost no case of fever *since I had adopted the practice of bleeding in the warm bath!*

Dr. Hackett does not deny that Trinidad is a sickly island, and admits that the cases which are met with there are of the severer type of tropical fevers. In a document which is dated Tobago, 23d October, 1830, he states that the fevers which he had been accustomed to see in Trinidad, for the last five years, were of the aggravated congestive form; and they do not pretend to say, that the cases that occurred during the first twenty months after my visit were more mild than those which had been met with previously to that period, but they forget to state, that before this, under their old Jacksonian practice, the mortality had been great, and that after this they were very successful. Now there must have been some cause for this sudden change; and, though I may admit for a moment that my theory may be wrong, yet, when we find out a method of treating the West India fevers, where, in the garrison of a large and a sickly island, during the period of twenty months, (out of probably about from seven to eight hundred cases of the various fevers to which the soldiers were subject), there were only about eleven deaths, and of these eleven only three died where the men had been under any thing like fair treatment;—I say, if this be true, and I believe that it is, we need not now quarrel with the saline treatment, merely because there may be some little doubt about the *modus operandi* of these agents.

But it seems that the saline treatment had been combined with other measures, which rendered it doubtful how much of the success was to be ascribed to the one, and how much to the other. Now, may I be permitted to ask, at what particular period did

these gentlemen become acquainted with those other measures? Did they know of them before the month of August 1828? And if they did know of means previous to that period, by which they could cure or lessen the mortality of the West India fevers, was it not criminal in them to allow such mortality to continue up to the very moment of my visit to that island? If they did know, previous to that period, of means or measures for preventing the mortality, what excuse can they make for having allowed so many of the soldiers to die? for there is one most important fact which these gentlemen have entirely forgotten to mention, namely, that the mortality had been very great in that garrison up to the very moment of my visit.

From the beginning of that year up to the middle of August, it is a fact that the Royals alone lost in hospital upwards of forty men from fever*. It is also true that the mortality ceased only from the moment that I had directed their attention particularly to the circumstance, that in the fevers which they had to contend with, the diseased state of the blood was the chief cause of death, pointed out the means by which they might prevent those fatal changes in the vital current which are in reality, in these fevers, the great cause of death. It is also true, that from that period they scarcely lost a single case from fever, except the three men already mentioned, who were beyond all redemption before they were admitted into the hospital, and two men who were so far gone that they died on the very day they were brought in. But I know well what their practice was, both before and after the middle of August 1828, and I know the result. I know also what those other *unnecessary measures* were which I did not recommend, and which they combined unnecessarily with the saline treatment, apparently for the purpose of taking to themselves as much of the credit as they possibly could, and taking as much as possible of it from me.

They knew, however, of all those other measures to which they allude, previously to my visit, yet they allowed

* It was in the first battalion of the Royals that this number was lost from fever: their actual strength was about 300 men; consequently they had lost nearly one-seventh of their number within a few months previous to my visit. So much for croton oil and their old Jacksonian treatment.

the mortality to continue up to that very period; but I will afterwards prove that some of those measures were too insignificant to have had any effect on the result, while others, but particularly the hot bath, were not merely inert, but actually improper, particularly when employed at the period of the disease at which they used it.

But it seems that this also reddens the blood. Be it so! and even this is no discovery of theirs; but, for this very reason of its reddening the blood, the hot bath ought not to be used during the hot stage of a burning fever. It is our duty, at this period of the disease, not to redden the blood by a hot bath, but to reduce the excitement by means of the lancet; and, if the one be proper, the other must be quite the reverse. Fortunately, however, they used this measure *only once*, and that, too, as I have said, during the hot stage of the climate fever*, when the blood was already too red, and then, in the dangerous stage of the disease, when there was risk of death from sudden diseased changes in the vital current, they did not trust the reddening of the blood to the hot bath, nor to soda, but to the strongest, and perhaps the best of all the non-purgative neutral salts, namely, a saturated solution of the muriate of soda and the nitrate of potass. This is what they trusted to after my visit for preventing the blood from becoming black, or more diseased in its properties, and after this, they had, as I have proved, scarcely one opportunity of examining the body after death, to ascertain whether the blood had been sodafied or not. But had they not used the saline treatment, it is very probable that their dissections would have continued after my visit to have been just as numerous as they had been before.

That the mortality was lessened after the middle of August 1828 is beyond all question; not that the season was more favourable for the British soldier, indeed August, September, and October, are frequently the three worst months in the whole year, and in twenty months these soldiers had been exposed to every variety of tropical climate. The mortality ceased, partly, as I believe, because I had warned them against the improper remedies recommended by

Jackson, Johnson, and a multitude of others, but chiefly, as I have said, to the circumstance of my having *drawn their attention to the diseased state of the blood as the cause of death*, and pointed out the proper means by which this fatal effect might be prevented in almost every instance where the patients were brought to the hospital early in the disease.

These gentlemen admit that great success did follow my visit to their hospital, but claim the sole merit of it themselves, except what is due to Dr. Jackson, Inspector Taggart, and other army physicians. Their grand dependence, they say, (or one of them at least,) both before and after my visit, was upon promptness and decision—bleeding in the warm bath, castor oil, and quinine. Now these, *except the warm bath*, are all very proper; but these of themselves are no more a match for the ardent or malignant fevers of the West Indies, than the mercurial practice of Dr. Chisholm, or the leeches and gum-water of Mons. Broussais. And may I ask if Drs. Jackson, Johnson, Mr. Taggart, or any one else, had taught them how to cure the West India fevers previous to my visit to that island? or, if the warm bath, croton oil, vitriolic emetics, calomel, sugar of lead, quinine, or alkalized toast-water, can cure these diseases, may I again ask, why they allowed so many of the soldiers to die, and why their dissections were so frequent up to the very moment of my visit?

Dr. Hacket, after an account of their other measures, gives, I must say, a most unfair view of the nature of the practice which they had adopted on my recommendation, and which has been attended with such signal success. He then states that "the sole cause of the cure may as well be attributed to any one of the other means, as that a *little soda* had caused all the advantages which they had obtained, that is, after my visit. The reader will himself see the art with which all this has been got up. Had they trusted in reality to a little soda, they would not have been more successful after my visit than they had been before; or if Dr. H. who is, I believe, still in the West Indies, will only return to Trinidad, and again give their other measures and a little soda a fair trial, he will find, without the assistance of the active saline agents which they have since used in consequence of

* This is also a disease of the blood, but it is not produced by a *poison*. One of the characteristic marks of this climate or seasoning fever is, that the venous blood is almost as florid as the arterial.

my recommendation, that their old Jacksonian practice and their other measures will again be just as unsuccessful as they had been previous to the month of August 1828.

When Dr. Johnson first announced the Trinidad documents, he promised to lay before the profession information of great importance. That they do contain facts of great value, is perfectly true, but these do not prove exactly the points which he wished them to establish; for, so far from shewing that the saline treatment in fever is of no use, they prove beyond all question that this is by far the most effective practice that has yet been tried, and infinitely superior to the remedies that he has been recommending all his life. There is also another important fact contained in these documents; namely, that the same treatment had been tried, and with a similar result, in a fever of a fatal character, which appeared in the island of Tobago in 1830. As to the other parts of their information, if there be any thing good in these documents, it is merely a repetition of what I have stated in my paper on the Blood. These gentlemen have evidently benefitted by my practice; but I confess that I can see nothing else for even Dr. Johnson to admire, except the temerity with which one of them, at least, misstates the facts, and the great ingenuity which both of them have shown in endeavouring to conceal from the public the true cause of the great success which certainly followed after they had begun to treat fever on the principle suggested by me. As to the rest, their papers are actually beneath all criticism. They have given an account of the derangement in the solids, such as they had met with after death, but they do not seem to have been aware of a fact which I will afterwards endeavour to prove; namely, that all this derangement in the solids is merely an effect of the diseased state of the blood.

I have stated that, "in a malignant form of the yellow fever, which is sometimes met with in the West Indies, the blood is nearly as thin as water, and literally speaking black as ink." In fact, it was so thin that even before death it could scarcely be retained within its vessels; and when taken from the heart after death, it was so changed that it could only be distinguished from the black vomit by a chemical test.

All this is well known to others as well as myself; and even Dr. H. admits that during the disease, the blood is sometimes so thin and dissolved that it oozes from the gums, eyes, &c. &c. before death. He admits, also, that this state of the blood, which I have described as peculiar to a malignant form of the yellow fever which is "sometimes" met with in the West Indies, had also been observed in the Military Hospital of Trinidad; yet, after having admitted this, which *I have said*, to be true, they put into my mouth, on their own authority, what *I have not said*, viz. that this appearance is "never absent;" and on the strength of these their own misstatements they venture to impeach me for want of candour. But the truth is, that if these gentlemen had been good enough to have founded their charges merely on what I have actually stated, they would then have had nothing to impeach; whereas, they fix assertions upon me which I have not made, and then accuse me of errors which I have never committed.

Whatever the immediate cause of the essential fevers may be, it is clear that this cause must invariably exist. Inflammatory affections of the brain, or morbid conditions of the alimentary canal, which are often, but not always met with, are merely accidental, and when they do present themselves, they are only an effect of the diseased state of the vital current. In the paper which was read at the College of Physicians, I stated my belief, that derangement of the blood was the immediate cause of fever; and that death, in *bad fevers*, was often, but not always, caused by its dissolution. Dr. H. admits, that on examination after death, the appearances the blood presented were invariably, and without a "*single solitary exception*," dark coloured, viscid, and grumous. That this is often the case in certain forms of the yellow fever, I have long known; for, under the old treatment, patients frequently died before the whole of the fibrine was completely exhausted, and the imperfect coagulation of this gives to the blood a grumous, or perhaps even when exposed to the air, a viscid appearance. All this, therefore, is nothing new; and I shall afterwards prove, that the diseased state of the blood, in these fevers, was better described than he has done it, long even before his time. Still, ad-

mitting every word that he has stated to be correct, it does not contradict one syllable of what I have said, but confirms it, namely, that a diseased state of the blood is the cause of these fevers, and is often, but not always, the immediate cause of death.

I have said that the blood is invariably diseased: Dr. H. admits that there is not even "a single solitary exception" to this rule; yet, he denies my statement, which is similar to his own, and then he appeals to others in proof of his veracity; and even refers to Dr. Johnson himself, who, I believe, has never seen the West India fevers, and who is, I may venture to say from his writings, totally unacquainted with this subject.

In the paper which was read at the College of Physicians, I stated that acids, alkalies, electricity, and every thing else that decomposes or destroys the neutrality of its saline matter, makes the blood black. I have also formerly stated, that these aerial poisons which act as the remote cause of fever, *have an effect similar to that of acids on the blood*; and not only blacken its colour, but derange its properties, even before they excite diseased action in the heart or any of the solids. I believe, also, that the black colour in the circulating current is not produced either by an excess or by a diminution of the natural carbonic acid of the venous circulation, for this blackness exists even in the arterial blood, and is, as I believe, caused in the first period of the disease by the effect of the poison on the blood, acting on the saline matter. The blood, however, becomes more red during the hot stage; but these fevers run their course with great rapidity, and in a very short period the blood is completely black from the loss, or at least from the great diminution of its natural saline ingredients, which are, as I have said, the true cause of not only its redness, but of its arterial colour.

Now when the blood has almost entirely lost this, as in the last stage of bad fevers, it is not by acids, or other agents that contain oxygen in a separable state, that we can restore the vital fluid to its healthy appearance; for this object can only be effected by throwing those saline ingredients into the circulation which are essential to its healthy properties. These have also, as I shall afterwards prove, not only a specific effect in preventing or destroying the

action of the aerial poisons, but they are, in fact, more essential to the healthy state of the blood than either the fibrin, the albumen, the colouring matter, or anything else connected with the circulating current; except, perhaps, its vital principle.

Such are, at least, some of the opinions which I have adopted, and I will only relinquish them when I am convinced that they are wrong. Yet Dr. H., who, I believe, has never even read my paper on the blood, undertakes to criticise it; and gives, I must say, a most garbled and unfair view both of my theory and practice. After this, he easily, and, as he seems to think, very cleverly, knocks down the fabric which he himself had made; and, to shew the great extent of his reading, he makes a very learned quotation from Dr. Paris's *Pharmacologia*; wherein it is mentioned, that, in a fever at Leyden, certain physicians had consigned a great number of their patients to an untimely grave, from the belief that a prevailing acid was the cause of the disease, in consequence of which they had trusted the cure entirely to absorbents and testaceous medicines. Now, in answer to this, which Dr. H. pretends to think quite sufficient to upset the whole of my views, I have only to say, that though this sort of logic might have satisfied Mr. Greatrex, who had, like himself, an interest in believing, or, at all events, in trying to make others believe, that they were entirely indebted to themselves for their success; yet it will scarcely be satisfactory to any well-informed member of the medical profession, particularly to those who have seen that the Trinidad physicians, by their own admission, allow that the remedies which I had recommended them to use, and which they did use, were neither "absorbents" nor "testaceous medicines." Had they attempted to redden the black and saltless blood, which we find in the last stage of the yellow fever, with chalk, or any other inert testaceous matter, in place of the active non-purgative saline agents which I had suggested, they would have been just as unsuccessful as the physicians of Leyden in 1699, or as they had been in Trinidad with their other measures, combined with their old Jacksonian treatment, previous to my visit*.

* The statement which I have made of the previous mortality under the old Jacksonian treatment, and the subsequent success under the new practice, is given on the authority of an

Whether the saline treatment was or was not the cause of their great success, or whether the treatment which I have recommended be or be not a better practice than that of either Jackson or Johnson, may still be with some a matter of doubt. Dr. Johnson and the Trinidad practitioners, may think that it is not better; I think that it is. But this is a mere matter of opinion, on which I will still retain the right of thinking for myself; and they have as little, perhaps less grounds for accusing me of want of candour for thinking that it is, than I might have for charging them with misrepresentation in the reasons they assign for thinking that it is not. It appears that Dr. H. is only acquainted with the contents of my paper on the blood, through Dr. Johnson's Journal; or perhaps it was through the medium of the Doctor himself that Dr. H. received information respecting those "misrepresentations" and "pointed assertions," on the strength of which he has founded his attack, and which I repeat *are not to be found in that paper*. It is only after he had already made a most unfounded, and, I must say, a most ungrateful attack, that he adds — "It is painful to me thus to express myself; if I do an injustice by my interpretation of Dr. Stevens's statement, I am truly sorry for it, and would call on him to correct it, or to set me right." Now I am in possession of more facts on this subject than these gentlemen are probably aware of; and, before I have done, they may be assured I shall "set them right." What will Dr. H. say when he finds that, in place of the "misrepresentations" which he says I so unblushingly made in the paper which was read at the College of Physicians, and on the strength of which they have founded their attack, it is proved that I stated nothing in that paper but what they themselves have admitted to be true to the very letter. But above all, what excuse can he make when it shall be proved (I trust to the satisfaction of the public) that even if I had laid claim to a share of the successful treatment of fever in that island, I might have done so with the most perfect propriety. If this be proved—and I shall do so even to their own conviction—is it not, then, probable that one of them, at least, will repeat his own

poetical quotation, and wish that he had "coined his heart, and dropped his blood for drachmas," rather than have been guilty of such conduct?

It has been stated that I have founded the superiority of the saline treatment on the result of this practice in Trinidad. This, however, is not correct; for the previous result of my own practice in the island of St. Thomas, was of itself quite sufficient to establish the superiority of the saline over the old mercurial, and, in fact, over every other treatment that has yet been tried. Even from the first, I considered the Trinidad practice as a mere secondary concern. At the time that my paper was read at the College of Physicians, I was on the eve of an unexpected visit to the United States of America; and, as I could not, at that period, publish the whole, I gave merely an outline of the facts, with a view of calling the attention of the profession to an essential part of the living body, which, I am of opinion, has been too much neglected; and unless we attend to this more than we have done, my conviction is, that, in as far as fever is concerned, we shall continue for ever in the dark.

I may now state that it was only in the year 1827, after a long residence within the tropics, that I found out what I believe to be the fatal error which had been, up to that period, the true cause of one half, at least, of the mortality from fever in the West India Islands. It was then that I commenced a different practice, on a new principle, and the success was such as to be almost incredible; for, from this period, there was scarcely one death from fever in the island of St. Thomas, except in the practice of those who were unwilling to admit any improvement which they themselves had not made. After this period, those who adhered to the old practice continued to be as unsuccessful as before. One of these gentlemen lost, at one period, in some cases, three out of four; while, in the same epidemic, Dr. Stedman and myself scarcely met with a single death, after we had adopted the new treatment.

In 1828, after I had given this practice a fair trial in the island of St. Thomas, we received intelligence that the yellow fever was raging in Trinidad, and was particularly fatal amongst the soldiers. It was then that I put myself to some inconvenience and went to that distant island, in the very middle of the

hurricane months, with the hope of being able to prevail on the army physicians in Trinidad to adopt the method of treatment which I had found to have been so successful in another island. On my arrival in Trinidad, and after having explained the nature of my practice to Mr. Greatrex,—I may be mistaken as to the precise words, for I quote from memory, and from a conversation which occurred more than three years ago—but, as far as I recollect, he told me, that they had tried the various modes of treatment as recommended in books, but that none of them had been successful; and that, under these circumstances, he should feel himself perfectly warranted in giving a fair trial to any practice that had been found to be successful in another island. Mr. G., who at that period had charge of the hospital, promised that he would give the method I recommended a fair trial, and let me know the result. It was, however, only after a considerable period had elapsed that I received any information from that island, and even then it came in an indirect way. It was only about the end of that year that one of the merchants of St. Thomas shewed me a letter which he had received from a friend of his, a captain in the Royals, dated Trinidad, 15th December, 1828, of which the following is an extract. “If you know Dr. Stevens, pray give him my very best regards, and tell him that since he was here, (four months ago, yesterday) the royal regiment *has not lost a single man*, which I attribute mainly to the lesson which he gave to our assistant-surgeon*, and for which God bless him.”

I had had the pleasure of meeting the writer of the above letter, as well as most of the other officers in that garrison, during my very short residence in Trinidad; but I had never seen him before, and I have not seen him since. It is true that he is not a medical authority, but his opinion is perhaps not the less valuable on this account, for he was not an advocate for any particular practice, and not biassed by any theory. He was not the partizan of either party; he was guided only by facts and common sense. He knew that under their old treatment, and previous to my visit, they had been in their fever practice most unsuccessful, for the battalion to which he belonged had lost from fever about one-seventh of its actual strength

within a few months previous to that period. He knew also that they had adopted a new method of treatment in consequence of my recommendation. He knew that from that period the mortality had ceased; and from these premises he had drawn his conclusion—conclusions which I believe to be more correct, and more in accordance with the facts, than those which have been drawn by two at least of the opposite party.

I believe I have formerly stated, that, in a letter which I received from Mr. Greatrex, dated Trinidad, May 12th, 1829, he admits that they had used a solution of the muriate of soda, and nitrate of potass, in the proportion and at the times that I had recommended. He states that this, my *sodaic* mixture, as he calls it, was given even during the night, and continued until in ninety-nine cases out of a hundred (even including cases of the yellow fever) the fever had abated; and then when the danger was past, they gave some large doses of the sulphate of quinine during the convalescence.

The great mortality previous to the month of August 1828, and the result afterwards, proves that there must have been some essential change in the treatment. It was not merely that the soldiers had become, all of a sudden, more seasoned to the climate, for the same treatment had been previously tried, and had proved equally successful, in the island of St. Thomas, even in robust sailors, and other strangers, who had just arrived from a cold country. In the same letter in which Mr. G. admits that they had adopted the saline treatment, he states that the “above system (that is, the new system, in which the use of the active non-purgative neutral salts constituted the great and the only essential difference betwixt their new and their old practice,) has been applied to three hundred and forty cases, or thereabouts, (now this was the number of cases which had occurred since the period of my visit, and on which the new system had been tried); and out of this number during the last seven months, there had only been three deaths from fever.”

I had received Mr. G.’s letter previous to the period at which my paper was sent to the College of Physicians; and in that paper I merely stated, that “in August 1828, at a time when there was a good deal of sickness in the garrison at Trinidad, *this practice* was

* Mr. Greatrex.

adopted in the military hospital of that island—that is to say, they bled freely, and used active purgatives in the commencement, to reduce the excitement, and afterwards the saline medicines were administered until the fever abated; and during the convalescence the quinine was given in large doses. In a communication which I received from Mr. Greatrex, of the Royals, who at that time had charge of the hospital, he states, “that the above system has been applied to three hundred and forty cases, or thereabouts, including both the remitting and yellow fevers admitted into the hospital, after the fever had existed variously from six to seventy-two hours, antecedently to an application to the hospital, with such success that, during the last seven months, not a case had died.” This document is dated about seven months after the commencement of the new practice. Mr. Greatrex also states, that within that time three* men died, having the remitting fever; but they had also abscesses in the lungs and purulent expectoration. As these three cases were complicated with extensive organic disease in the lungs, it is probable that they would have been fatal under any treatment; but, out of the three hundred and forty cases of essential fever which had been treated in the manner described, there was not even one death in the Royals from the time that this practice had been adopted; and I may add, that in the West Indies, Trinidad is generally considered as one of the most sickly islands.

Now, after all the noise that they have made, will the reader believe that, in the paper to which they refer, this is the whole of what I have stated with respect to Trinidad? or, after all the strong and unwarrantable language they have used, will it be credited that they themselves admit every syllable of this to be true? Dr. Hacket admits that this was “really their practice,” and Mr. Greatrex admits the result to be as stated by me. I did not say, as I might have said, that I had gone to that island with the hope of being able to lessen the mortality, by prevailing on the army physicians to adopt a practice which I had found to be so successful in St.

Thomas. I did not say that the saline treatment, or any other, was the cause of the great success which followed my visit to their hospital. I merely stated such was their practice, and such the result. But if I did not *say* then, that the success which followed my visit to that island was in some measure owing to that circumstance, *I do so now, with the most perfect conviction that such is the fact.*

If I did not in that paper notice the error into which Mr. Greatrex had fallen, about the blood not being reddened by soda, in three men who had never taken even one particle of that alkali; or, of calling the saline practice, as he had used it, the treatment of Jackson; or if I did not notice his still more serious mistake, about reddening the blood with a hot bath in the inflammatory stage of the climate fever, it was merely because I wished not to injure him—on the contrary it was my wish to serve him; for, whoever the author of this practice may be, I believe that the success which they have had in Trinidad has no parallel in the army practice in any other sickly island in the West Indies; and as the merit of having *adopted* this practice was due to him, I thought I might serve one who appeared to me to be a very interesting young man, by directing the attention of the Army Medical Board to this circumstance. Such was my conduct; these were my motives; and, God knows—I have had my reward.

In a letter written by Mr. Greatrex, *apparently in answer to a letter from Dr. Johnson*, dated July 24th, 1831, he states, “I have withheld, as you have seen by Dr. Hacket’s letter, all authority for the publication of a *part* of my letter; still Dr. Stevens has published a garbled extract from it.” Now, whatever Mr. G. may have said in his note to Dr. Hackett, or in his letter to Dr. Johnson, I do most solemnly declare, that I have never received any note, or any communication from Mr. G., except the document which Dr. Johnson has published, and which, I may here observe, is incorrectly printed in his journal; and I must add that the extract is not garbled, but correctly given—in proof of which I send you the letter for your inspection*.

* Dr. Johnson, in his version of Mr. G.’s letter, omits the word *three*; thus leaving it to the imagination of the reader to suppose any number that he pleases as having died. This may be an error of the press; but I think proper to notice it, lest it might be supposed that the error was on my side.

* We have compared Mr. Greatrex’s letter with the extract from it published by Dr. Stevens in his former paper, and find them to correspond entirely.—ED. GAZ.

Now, mark this: the Trinidad army practitioners admit, even by their own confession, every syllable to be true which I have stated in the paper to which they refer; they admit that their practice was such as I have described; they admit also that my statement was correct with respect to the result; they admit all that *I have said to be true*; and yet, in the same document, and at the same time, they accuse me of having made "pointed assertions" and "misrepresentations," which I do positively declare *I did not make*.

I am afraid that Dr. Hacket has been miserably deceived by some of his correspondents in this country; but, at all events, it was the imperious duty of that gentleman, before he ventured to make such an attack, first to have proved that I had in reality made even *one assertion* that was not correct, or drawn even one solitary conclusion which I had no right to have drawn. Had he done this, he would then, at all events, have been right in his premises, however wrong he might still be with respect to his conclusions; but the fact is, that he is wrong in both; for I deny that I either drew a conclusion which I was not warranted in drawing, or made even one single assertion which I had no right to make;—if I have, let him point out where these are, for I cannot find in my own paper those assertions which he says are not true, neither are they pointed out, much less proved, in his. It is only their own idle assumptions which they are pleased to call my assertions; and it is on this imaginary and baseless cause of complaint, which they themselves have conjured up, that they accuse me of misrepresentation and want of candour. It is not the statement which *I have made*, but pointed assertions which *I had not made*, that Dr. Hacket has been pleased to contradict, "in the most pointed and unqualified manner," and none certainly had a better right to do this than those opposed to me, for all the assertions that are not correct, and the whole of the misrepresentations which have been made on this subject, are of their own fabrication. It is they themselves who have mis-stated facts by asserting that I have made pointed assertions which I did not make, and that I had drawn conclusions which I did not draw, and then, after mistaking these their own assumptions for facts, they commence an

attack upon false premises, and in conducting this, two of them at least use language not tolerated among gentlemen, and which is certainly not becoming in deciding on the treatment of a fatal disease, and which would not have been at all necessary if their warmth had merely been the glow of truth.

Dr. Johnson states, that for the honour of the profession he had made up his mind not to publish the Trinidad documents; "but (says he) my attention being roused by the announcement of Dr. O'Shanghnessy's paper, and more especially by the very strong terms in which Dr. Stevens's discoveries were spoken of in the Medical Gazette, in which it is stated that Dr. Prout considered these discoveries as of the last importance to mankind, I felt that it would be culpable, if not criminal in me, to withhold from the profession the counter-statements of the army medical officers of Trinidad, from which it appears that *soda* had nothing whatever to do with the *success* of the treatment in the above island." Now if Dr. Johnson cannot bring forward any better reason than this for such an attack, he had better have kept it to himself, particularly if he has no other documents than those which he has hitherto produced to prove the charges which he made against me in the Westminster Medical Society, and which, I must say, were most unfounded. So much for the manner in which Dr. James Johnson proposes to maintain "the honour of the medical profession."

As I have said that Dr. Johnson has been guilty of wilfully endeavouring to mislead the public with misrepresentations about soda and sodafied blood, it may be but fair to shew, by his own evidence, that this is the case. I therefore beg leave to refer you to his review of my paper on the Blood, in which you will find the following extracts contained in his number for June 1830:—

"At a late meeting of the College of Physicians, a paper on the above subject was read by Dr. Stevens, who has, for many years, been a practising physician in the West Indies. As the views which Dr. S. entertains are novel, and the means which he proposes for combating a dreadful scourge of the human race, *are simple and practicable*, we have endeavoured to collect the substance of

the writer's observations as accurately as possible, to lay before our readers.

* * * * *

"Our author observes, that one common property of *neutral salts* is that of giving a rich arterial colour to venous blood. This property is common to them all; and the degree to which they possess it is, perhaps, the best test of their purity as saline agents. To ascertain the effects of different agents on the blood, he made a number of experiments, in which it was observed—

"1st. That all the acids give a dark colour to healthy blood, and, in proportion to their strength, change it from red to black, as certainly as they change vegetable colours from blue to red. Even the vegetable acids so completely blackened the blood, that the addition of a little water converted the whole into fluid exactly resembling the black vomit. Secondly, *the pure alkalies have a similar effect with the acids, in changing the blood from red to black, though not in the same degree.* Thirdly, the neutral salts immediately changed the venous blood from a dark modena red, to a bright arterial colour. Even those salts that contain a slight excess of alkali, the sub-carbonate of soda for example, immediately give to venous blood a beautiful bright arterial colour. The effects of these experiments are best seen when made on healthy blood. The agents ought first to be dissolved in a little soft water, and then well mixed with the warm blood, before it begins to coagulate.

"4thly. When the neutral salts are mixed with the dark and dissolved blood that had been taken from the hearts of those who had died of yellow fever, even the black and dissolved fluid was instantly converted from a black to a bright arterial colour.

"The nature of this paper (said Dr. S.) prevents me from entering minutely on the important effects which this saline impregnation produces in the vital fluid; but, in a work which will soon be published, I shall endeavour to prove, first, *that the blood owes its red colour to this saline impregnation.* Black appears to be the natural colour of the colouring matter; for, when we take a clot of blood, and deprive it completely of its saline matter, by immersing it in fresh water, the colouring matter soon becomes so black, that even oxygen has

no effect in changing its colour. But, when we immerse this black clot in an artificial serum, made by dissolving some saline matter in water, the black clot in this clear fluid assumes almost immediately a beautiful bright arterial colour. Secondly, that, to this saline impregnation the fibrin owes its fluidity: for it retains this form only so long as it is held in solution in the salt serum. Thirdly, that the change of form which this saline matter undergoes, when the blood changes from arterial to venous, and from venous to arterial, alters its capacity for caloric, and gives it an influence in supporting the temperature of the system. *The saline impregnation also adds to the stimulating quality of the blood,* and assists, even in a high temperature, in adding to its powers of self-preservation.

* * * * *

"As we have no doubt that Dr. Stevens will pursue this interesting inquiry further, and lay the results of his observations before the profession in a more extended form, we shall abstain from any comments on the present occasion. We have laid a very full and faithful account of the paper before our readers, and leave them, for the present, to draw their own conclusions."

As I trust that many of your readers will feel an interest on this subject, which I have endeavoured to divest of every thing like unbecoming personalities, and as most of them have not seen the West India fevers, it may now be proper for me to make a few observations on the nature and treatment of these diseases, but above all to point out the *fatal error* in the old practice which has been, as I believe, the true cause of one-half, and perhaps even more, of the mortality in the West India fevers. This I shall do as early as possible, and when, I hope, your readers will find in my remarks something to interest them, more than the controversial matters on which I have, in vindication of my character, been most unwillingly compelled to enter.

I am, sir,

Your obedient servant,

W. STEVENS, M.D.

Albany-Street, Jan. 9, 1832.

ADDITIONAL REMARKS

UPON

VACCINATION.

By WILLIAM HOWISON, M.D. F.R.C.S.

Vaccinator to the Royal Dispensary of Edinburgh.

*To the Editor of the London Medical Gazette.*January 1st, 1831,
9, Nicolson-Square.

SIR,

THIS winter, since commencing my duty on the 1st of November, as Vaccinator to the Royal Dispensary of Edinburgh, I have again turned my attention to the appearance of the vaccine vesicles upon the arms of the numerous children vaccinated at that institution, throughout all their stages. During the months of November and December, which have now passed away, I feel happy to state that the various vesicles throughout all their stages, have presented a much more favourable appearance than they did last winter during the above-mentioned months, as described by me, in the paper on Vaccination, which appeared in the Medical Gazette of July 1831, to which I request my readers again to refer, as the precursor of the remarks which I am now about to make. The vesicles of the past months of November and December have invariably appeared large, beautiful, and distinct, with pellucid virus, as much so as they did during the pleasant genial weather of June and July. And vaccinations, at a distance performed with virus taken from them, have proved equally beautiful and satisfactory. I have no hesitation in attributing this fortunate circumstance principally to the mild temperate weather, the wind being confined to the south, south-west, or west, which, with the exception of a few days of intense frost and snow following a northerly wind, about the beginning of November, has prevailed throughout November and December, resembling more the temperature of spring than that of winter. Small pox has besides ceased to exist for several months past in the city of Edinburgh and its neighbourhood, and perhaps the removal of that destructive disease from the environs of the Dispensary, may have rendered the cow-pox vesicles more complete and satisfactory than when it was extending its violence round

about, as it did last winter, when my remarks were made.

A few medical friends, interested in this subject, having done me the honour of reading the ideas that I have already thrown out upon vaccination, having requested that I should pay attention to the shape and size of the vesicle, the state of the cicatrice; how far vaccination is modified or destroyed by other diseases existing in the infant at the same time; the nature of what is termed modified small-pox, and the identity of small-pox and cow-pox; I now proceed to make remarks upon these, so far as my limited means and abilities will allow.

I.—*The Figure and Size of the Vaccine Vesicle.*

The figure and size of the future vaccine vesicle depends entirely upon the mode of inserting the virus at the time of performing vaccination. The vaccine vesicle is by no means confined to any particular figure or dimensions: it is not uniformly circular, as many authors have erroneously supposed and described in their writings; neither is it oval. I will say that it will assume any figure which the puncture or line of incision at which the virus is inserted may be made to take; forming an elevated boundary to that line. Neither is its size of any limited extent: were a circular line drawn round the arm, or even round the wrist, of an infant, and vaccine virus inserted throughout the track of that line, we would have the future vesicle following a similar extensive circular range. In public Dispensaries, a vesicle of an oblong shape, formed by inserting the virus by the incision of the round-pointed inoculating lancet, is absolutely necessary, to afford a sufficient quantity of virus for carrying on future vaccinations. In private practice, this circular vesicle, formed by the puncture of the common bleeding lancet, perhaps may be sufficient. The bleeding lancet, however, I neither use nor approve of, for reasons already given to the public.

II.—*State of the Vaccine Cicatrice at an after period.*

With regard to the vaccine cicatrice at an after period, during the remaining life of the individual, proving that the vaccination has been properly per-

formed and passed through, my experience is limited. I have no hesitation in stating, that the regular punctured cicatrice, formed by the destruction of the cutis viri, which can at no after period be regenerated, and subsequent contraction of the cuticle, which gradually takes place during the formation and progress of the vaccine crust, previous to its being thrown off, from its white colour, &c. continuing unchanged for the life of the individual, is a certain and infallible criterion to the accustomed observer that the vaccination has been properly gone through, and that no other disease or accident, so far as we know in the present enlightened state of medical knowledge, will ever assume the same regular appearance. To this circumstance, however, I must confess I have not as yet given my complete attention.

III.—*How far the Progress of Vaccination is modified, or destroyed, by other Diseases existing in the Infant at the same time.*

Upon this subject I have little to say. It is a circumstance admitting of no doubt, that the state of perfect health is the one most congenial to the efficacy, beauty, and future progress of the vaccine vesicle; and in proportion as the constitution of the individual vaccinated deviates from that state, in the same proportion does the vaccine vesicle deviate from its beauty and efficacy. My friend Dr. Sanders, who many years ago favoured the world with his observations upon this subject, in a conversation which we lately had, informs me that he repeatedly, by way of experiment, vaccinated infants labouring under varicella, or chicken-pox, in all its stages; and that both cow-pox and varicella went on together, throughout their whole progress, without interruption. I look upon varicella as a mild disease, notwithstanding the far more extensive experience of my friend. Whether, however, vaccination and other more severe constitutional diseases, as measles, scarlatina, &c. will go on in the infant at the same time, and pass through their regular stages, or whether one will destroy the other, my experience does not enable me satisfactorily to decide. Were I compelled to give my opinion, I would say they would overpower each other, the stronger gaining the ascendancy at last. I finally believe that

every other constitutional disease will either totally destroy the progress of the vaccine vesicle, or render it so imperfect as not to afford, at a future period, security against small-pox infection. This, in reality, is a subject of little practical importance, as no medical man can be justified, under any circumstances, of vaccinating individuals when labouring under the influence of severe constitutional disease; with the exception of scrofula, mania, or pthisis pulmonalis.

IV.—*Identity of Cow-pox and Small-pox*.*

V.—*The Nature of the Disease termed Modified Small-Pox.*

With regard to the nature of the disease termed modified small-pox, I subjoin the following letter, lately sent to me by Dr. Sanders, in answer to a request to be favoured with his ideas upon that subject. I decline accompanying it with any remarks of my own.

Duke-Street, Dec. 23, 1831.

Dear Howison,—With regard to vaccination as a preventive of small-pox, I maintain that the disease of the real small-pox never occurred either in its complete form, or under any modification, in a person who had undergone vaccination.

That it never occurred in its complete form, is proved as follows:—1st, During 25 years I have not seen even one instance of the small-pox, in a person who had been vaccinated. 2dly, That in the writings of the anti-vaccinists, as well as those who believe in the occasional failure of vaccination, there is not one instance of the small-pox. Respecting the voluminous works of the new Professor of Pathology on this subject, I agree with the authors of the *Dictionnaire des Sciences Médicales*, “that all the cases produced by Dr. Thomson want the proper characters of the genuine small-pox (varicella).”

That it never occurred under any modified form, is proved as follows:—1st, If a certain disease is modified by vaccination, then a form of disease must have been produced, which was unknown previous to the discovery of the vaccine virus, and which cannot appear

* This portion of the paper we omit, because it is merely a copy of Dr. Sonderland's remarks, already published in this journal.—E. G.

except in the vaccinated. 2dly, In the vaccinated, no disease resembling small-pox has appeared, that was not known and described long before Dr. Jenner, the discoverer of vaccination, was born; nor does any varioloid affection now appear in the vaccinated, that does not equally appear in those who never were vaccinated.

What is the disease called modified small-pox? On comparing the modern cases with those described above a century ago, we find that the eruption called modified small-pox, is no other than the disease distinguished by the name varicella, or chicken-pox, which is neither prevented nor altered by the influence either of small-pox or cow-pox.

I have not leisure to correct or copy, but such as it is you have it, from yours,

Ever truly,

JAMES SANDERS.

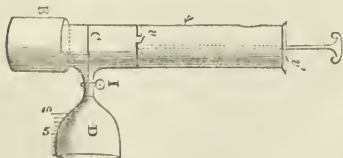
To Dr. Howison.

INSTRUMENT IN AID OF VENESECTION IN CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

As the abstraction of a certain quantity of blood, in the early stages of cholera, is generally admitted to be a most desirable proceeding in the treatment of the disease; and as this quantity of blood, owing to the collapsed, or collapsing state of the patient, very frequently cannot be obtained by the ordinary method of venesection, I beg leave (with great diffidence, not being a medical man myself), through the medium of your excellent journal, to submit to the consideration of the faculty the subjoined plan of an instrument, by means of which, in conjunction with the ordinary method, the object in view may be attained.



A, represents a brass cylinder and piston, about seven inches long; C, a diaphragm, or partition, in the cylinder,

continued into the lateral orifice to beyond the stop-cock, 1; D, is a graduated glass receiver, the receptacle of the blood abstracted; B, a glass cylinder, about two or three inches in length, accurately fitted on to the brass barrel, A, and having its lower open extremity well ground. The lateral tube is to be about 7-8ths of an inch in diameter, divided into two compartments (5-8ths and 7-8ths), by the continuation of the diaphragm. Two valves opening upwards are marked by v and v'. D, the receiver, is applicable at pleasure to the lateral orifice, by means of an airtight collar.

The principle upon which this instrument is intended to act being so well known, and the construction of it so simple, it would be superfluous in me to enter into a lengthy detail of particulars. It may, therefore, be merely necessary to remark, that the open lower extremity of the instrument being gently pressed around the orifice of the punctured blood-vessel, the piston is drawn back slowly, ejecting by this means a certain quantity of air. If the air withdrawn in this way be, to that still remaining in the receiver and glass cylinder (B), as one to two, of course the fifteen pounds pressure to the square inch, which the atmosphere opposed to the flow of blood from the vessel, is reduced to seven pounds and a half; and if it be desirable to procure a more abundant supply, or to promote the flow of blood still further, by restoring the piston to its former place, the operation may be readily repeated.

Before removing the receiver, for the purpose of emptying it, as it should not be allowed to hold too large a quantity at a time, care should be taken to turn the stop-cock, so as to close the passages.

And in conclusion, sir, allow me to suggest the general application of this instrument in all cases where the rapid abstraction of blood is desirable.

I am, sir,

Your obedient servant,

JAMES ALEX. VENTRESS.

London, Jan. 7, 1832.

NOTE FROM MR. AINSWORTH.

To the Editor of the London Medical Gazette.

January 8, 1832.

SIR,

IN case it may escape your notice (for the communication referred to bears sufficient evidence of the fact), I beg leave to request that you will deprive me of the credit of communicating a notice on the utility of common salt in cholera. I would not have troubled you with this note, but that Dr. Gibson had, long before Wardroppe's case, recommended the administration of mustard and of salt emetics, so much extolled by Mr. Searle and Dr. Barry; and in the meetings which were held at Sunderland, to discuss the new facts as they presented themselves, I objected, from mere principles of common sense, to their indiscriminate use. I, in one case, saw an emetic of salt administered with very beneficial results, but it was when the action of vomiting was indicated by other circumstances; and it produced relief from violent epigastric pains, by the removal of large indigestible masses from the stomach. In the case in which Mr. Tarbock refers to me, in evidence of the beneficial effects of carbonate of soda, I certainly can give my most willing testimony in corroboration of those results. I never saw so much benefit produced in cases where, if you will, the *choledic* symptoms predominate over the *asphyxia*, as by the exhibition of large doses of that substance.

I remain, sir,

Your obedient servant,

W. AINSWORTH.

2, Woodstock-Street, Broad-Street.

[The paper alluded to was attributed to Mr. Ainsworth, not by us, but by the printer; who, seeing his name mentioned, introduced it on the wrapper and in the running title.—ED. GAZ.]

COUNTER-IRRITATION IN CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

As important discoveries sometimes arise out of the most vague conjectures,

if it does not seem too Quixotic for a place in the Medical Gazette, I should like to submit to your readers a proposition (as far as I know) original, with regard to the treatment of cholera—that, namely, of endeavouring to set up a new action in the system, by the introduction of some acrid matter beneath the cutis.

The chief objection, I conceive, to be raised against such a measure would be the improbability of any thing acting with sufficient rapidity; yet as nothing up to the present time seems to have arrested the disease in its progress, perhaps it might not be arrogating too much to think the experiment worth trying.

Having been personally a witness to the effects of *Rhus Toxicodendron* on subjects inoculated with its juice, and the experience of the late Dr. Alderson, of Hull, proving its value in paralytic affections when administered internally, I would, in the absence of any thing better, humbly propose the inoculation of cholera patients with the former, and at the same time its being administered in the form of infusion, or tincture, by the mouth.

I am, sir,

Your obedient servant,

A. B. C.

TOBACCO IN CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

NOTICING in a late newspaper that a Dr. B. had successfully treated a case of cholera at Newcastle, or in the neighbourhood, with a tobacco clyster, or rather it should have been said, *that the patient had escaped death* to whom it had been administered; allow me to observe, that I consider it exceedingly unprofessional and wrong in any individual to promulgate such instances of fortuitous success to the public, as it may lead others to adopt such treatment, to the neglect of other better understood and appropriate remedies, to the sacrifice of not one individual's life, but perhaps of many in its trial. Indeed, it must be obvious to every one at all conversant with the subject, that in a disease characterized as cholera is by such universal atony and prostration of power, that a remedy like tobacco, in any

form, could seldom, if ever, be indicated; that it was doubly incumbent upon Dr. B. (I shall not mention his name) to have deferred his communication to the public until his further experience had justified its announcement.

Indeed, Mr. Editor, the too general want of success in the treatment of this disease is not, in my opinion, to be attributed to its incurable nature, nor to the want of appropriate remedies, so much as to the fact of the practitioner being but too often bewildered by the numerous writers upon the subject, who for the most part have never seen a case of the disease, promulgating doctrines and systems of treatment which my experience justifies me in saying, must as often prove injurious; and with this result, the practitioner loses all confidence in system, looks upon the disease as a *terra incognita*, and feels himself justified in having recourse to any thing which may strike him at the time; thus we hear of tobacco clysters, the actual cautery, ice pills, and the cold affusion in the journals of one week, and brandy with opium, cajuput oil and the vapour bath, on the following; and thus a system of experiment is perpetuated from one year's end to the other, and from the commencement of the epidemic's invasion at each and every place to its termination.

I remain, sir,

Your obedient servant,

S. C.

London, Jan. 10th, 1832.

LETTER RECLAMATORY FROM SUNDERLAND.

[We have no objection, in compliance with the writer's request, to insert the following letter, which refers to some remarks published above a month ago. If any of our readers take the trouble of turning to the Gazette in question, they will be the better enabled to judge whether or not the subjoined remarks be an answer to the observations alluded to.—*Ed. Gaz.*]

To the Editor of the London Medical Gazette.

Sir,

My absence from London deprived me of the opportunity of perusing the December numbers of your journal before this day. In the number for the

3d of the month, I find a communication which pretends to be a refutation of a statement of mine that appeared in the Times paper, and I must therefore request a place in your columns for the following remarks upon its contents.

I did not state, as has been pervertingly alleged by the writer of the above communication, that I was the first observer who discovered that a disease identical with Indian cholera was prevailing at Sunderland; nor is it correct, as the writer would make it appear, that Dr. Barry was the first physician who announced this conclusion: but I did state that I was the first observer who made the fact publicly known "that the different forms of cholera, at that time spreading in Sunderland, were one and the same disease—that disease being the Indian cholera." Cases of cholera, commencing with violent symptoms, were occasionally occurring, and these cases were admitted by several practitioners to be essentially coincident in their symptoms with the foreign malady; regarding another set of cases; however, which were of a comparatively mild character and frequently met with, a diversity of opinion was entertained. These comparatively mild cases were designated, in the official reports, "common cholera," and it was understood by some medical men who had suspected importation in respect to the severe forms, that these mild cases were of the English or indigenous description; while, on the other hand, the advocates for an endemic origin in every instance held that the various forms were one and the same disease, that disease being a native of the locality. By sedulously watching the progress of the comparatively mild cases, I found that a few of them proceeded, despite of medical treatment, to a fatal issue, developing in their progress, and after a longer or shorter time, the symptoms of the most violent type of the pestilential malady; and then I came to the opinion, the public expression of which I claimed, in priority, as my own, namely, that the cases of "common and malignant cholera" were one and the same disease, that disease being the Indian.

I have the honour to be, sir,

Your very obedient servant,

TIMES CORRESPONDENT.

Sunderland, Jan. 9, 1832.

ANALYSES & NOTICES OF BOOKS.

“L'Auteur se tue à allonger ce que le lecteur se tue à abrégér.”—D'ALEMBERT.

Catechism of Health, or plain and simple Rules for the Preservation of Health and the Attainment of Old Age. By A. B. GRANVILLE, M.D. F.R.S. &c. &c.

THE idea of publishing a Medical Catechism is not limited to Dr. Granville—he has Sir Charles Aldis as a companion; and the chief difference in the comparative merits of the works is, that the “Examinations” of Sir Charles are infinitely the more professional and unobjectionable of the two. We scarcely know in what terms to speak of this volume, so unusual is it to see a physician, with any pretensions to respectability, descending to the arts which have hitherto been confined to the charlatan. Yet there are reviewers who, instead of stigmatizing such productions in the manner they deserve, are actually bold enough to commend in the strongest terms this wretched collection of commonplace nursery precepts, flippant abuse, and puffing of the author's own peculiar *secret* remedies.

We are not wont to speak thus harshly of any of the authors whose works we have occasion to notice, and our readers will expect some specimens of a composition which we thus strongly denounce. The first part, then, against which we protest, and which every impartial critic will hold up to reprobation, is what we have mentioned last, viz. that the author deals in *nostrums*. He has his “drops” and his “embrocations,” the composition of which he studiously conceals, while he vaunts their efficacy as quacks do their infallible panaceas. It is true that Dr. Granville promises, like Dr. Clanny, “ere long,” to benefit the world by promulgating the composition of his elixir; but meantime he contents himself with the advantage to be derived from concealment, referring all who may desire to drink and be cured, to Mr. Garden, of Oxford-Street, by whom the “stimulating alkaline drops” are sold. Nor is this all; we have also an intimation of a lotion, which the author has been “fortunate enough to devise,” which produces a blister “in the brief space of a few minutes, or simple counter-ir-

ritation in less time.” It is a pity the author did not tell in how much less than “a few minutes” these marvellous effects resulted; but that too, we presume, is to be learnt when the specific is purchased of the chemist appointed for its sale. St. John Long is no longer without a rival.

We have now, we conceive, said quite enough to justify the strong disapprobation of this work which we have expressed. Let others denounce concealment only when it is adopted by the oncasts of the profession, who gain a disreputable subsistence by practising on the ignorance and credulity of mankind, and let them hold secret embrocations to be censurable only when they are confined to Harley-Street: we choose to follow a different course; and the higher the rank that any practitioner occupies in society, the more imperatively do we hold it to be our duty to remind him, that there are things which, in this country, no physician may presume to do, unless he be prepared to lose the respect of his brethren, and take his place among the lower grade of those who deal in patent remedies, and infallible but secret specifics. Even the Editor of the *Medico-Chirurgical Review*, who has had the complaisance to laud the work, (probably the due return for a certain portion of commendation somewhat awkwardly bestowed upon himself in the preface,) has some misgiving about the concealment of the prescriptions, and very sagaciously asks, “how are people in Scotland, Ireland, or country towns of England, to avail themselves of these remedies?” Why, they can send to Mr. Garden's, to be sure, and procure them, as they do Whitehead's essence of mustard, Godfrey's vegetable balsam, or Ching's worm lozenges.

And this leads us to observe, that the College of Physicians is bound immediately to interfere. The author of the work in which the use of these secret remedies is recommended, when he received his license, *took an oath*, upon his bended knees, to observe the statutes of the College;—now one of these runs thus: “Nemo in permissorum numerum admittatur qui medicamentum quodvis arcanum (*nostrum* vulgo dictum) in morbis curandis usurpare solitus fuerit;”—and then goes on to specify that a disclosure of the composition of any such secret remedy is a necessary

preliminary to admission. But it is quite clear that if no one can obtain a license who uses a nostrum, no one who does so ought to be suffered to retain such license. It therefore becomes the imperative duty of the Censors of the College, as they regard the honour and would defend the interests of that branch of the profession entrusted to their care, to insist that Dr. Granville shall *instantly*, and publicly, announce the composition of his "medicamenta arcana," or else that the erasure of his name from the list of Licentiates shall immediately follow his refusal. This it is in their power to do—this in other instances they have done—and this we repeat it is their bounden duty to do in the present case, and that forthwith. If they do not, the members of their own body—but more especially the Licentiates—will have just cause to complain of their supineness.

From this digression we return to the volume before us.

The absurd collection of "wise saws and modern instances," of which it is chiefly composed, is only calculated to amuse the medical reader by the excessive puerility which it displays. But doubtless these words of wisdom will serve the purpose of making nurses and mothers, to whom they seem principally addressed, look upon the author with profound admiration;—we can fancy them comparing notes as to their progress in medical science, and admitting as they read, that

"——— still the wonder grew,

How one small head could carry all he knew."

But our space narrows, and we must enable the reader to judge for himself of the novelty and importance of some of our author's observations. The following is particularly recommended to the attention of all grandmothers who have purchased the volume: it cannot, at the present time, be looked upon as otherwise than very seasonable.

"Q. Is it proper to give to infants cakes, sweetmeats, and similar kinds of food?

A. Such articles of diet, or luxury, had generally better be avoided," &c.

But perhaps the following, during the continuance of the present sharp frost*, is even of more importance to be attended to.

"Q. Is it proper to sit opposite to an open door or window, or between both; in other words, in a draft or current of air?

A. This is particularly injurious *after sun-set!*" &c. &c.

The above caution, as far as it goes, is exceedingly valuable; still we regret that the intelligent querist did not take the opportunity, while the oracle was propitious, of inquiring—Is it necessary to wear clothes during any part of the year? However, we must not repine, but e'en take what is vouchsafed us. What follows is very much to the point.

"Q. Is it necessary that man should take exercise?

A. Without regular active exercise of the body, its health cannot be maintained."

And farther:

"Q. Ought exercise to be taken in the open air?

A. Decidedly!" &c.

Nor are the observations with respect to rest less satisfactory.

"Q. Is a certain quantity of sleep essential to the health of man?

A. Yes, &c."

But then we must take care where we sleep: for instance, it would be imprudent to take our rest on the road, or even by lying down in Hyde Park.

"Q. Is it proper to sleep upon the grass, or ground?

A. This is highly injurious," &c. &c. Nay, even children must not venture upon this—at least if they be fatigued.

"Q. When children become tired from their exertion at play, is it proper for them to *lay* down on the damp ground, or grass?

A. No," &c.

Neither must we venture to eat every thing that comes in our way.

"Q. Is unripe or decayed fruit unwholesome?

A. It is so in the highest degree."

That unripe fruit should be deemed unwholesome, is, perhaps, rather remarkable; but the author delights to surprise by the originality of his views. We have looked carefully through the article on diet, but we can nowhere find it positively stated whether it be necessary to the health of man that he should take any food at all, or not? Probably this is one of those difficult points on which the author has not yet finally made up his mind: the public, however, will look anxiously for his decision.

* These remarks were written during the holidays, though we have not until now found room for them.—REV.

But of the whole collection, the most important question for the public to put, and the author to answer, is that which relates to the conduct to be pursued when any one is taken ill.

"Q. When an individual is attacked with *any disease*, what should he do?"

"A. Send at once for a regular and experienced physician."

And here the inference is unavoidably forced upon us, that, by some error of the press, a question and answer have been omitted; at all events, if otherwise, it must be entirely owing to the extreme modesty of the author; and we earnestly recommend, that, in the next edition, the query, "who is the *most regular* and experienced physician in London?" may find a place, with its obvious and appropriate answer.

One other remark, and we shall close our extracts. After having been informed that ardent spirits are pernicious, Dr. Granville's most docile and intelligent pupil puts the following pertinent question:—

"Q. Is their moderate equally injurious with their immoderate use?"

"A. Certainly not," &c. &c.

Now, as we think the *spirit* in which the volume is written is, in this respect, analogous to the distilled liquors here alluded to, we shall take the hint, and not run the hazard of what we might incur by its immoderate use; for *this* the ready respondent to the last sagacious question would probably tell us was equivalent to its *abuse*.

Dr. Granville is said to be a man of some talent, and we presume with justice; but we must say, that, in the publication of the present volume, he has tried a very hazardous experiment with his reputation. The only part of it which does not consist of the most common-place truisms, is what relates to cholera; and this we pronounce to be one of the most partial and incomplete pieces of medical writing we ever perused—made up entirely of special pleading, and interlarded with taunts against the late Board of Health, penned with all the bitterness of a disappointed partizan. There is one other point to which we must allude;—the author of the Catechism, indeed, may not be, and we trust is not, answerable for the sins of his publisher; but the system adopted of puffing it has been well worthy of the volume: paragraphs, of the most impudent description, by

some *contrivance*, have been inserted in the newspapers, in one of which, the "Catechism of Health" was compared, with almost blasphemous panegyric, to the catechism of the established Church, and recommended to be, like it, committed to memory! But the worst of it is, that these puffs constantly appear with new and deceptive headings; so that it is impossible, till the reader has got nearly through, to discover whether they relate to some event really connected with the introduction, or be merely a trap in which he is caught by the Protean panegyrist of—

"—thine incomparable oil, Macassar!"

or of the matchless excellence of Dr. Granville's "Catechism of Health, or plain and simple Rules for the Preservation of Health and the Attainment of Old Age!"

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Edinburgh Medical and Surgical Journal. Vol. 35, No. 110, Jan. 1832.

This is a very interesting and valuable number: the original communications are many of them highly important; and we shall take another, and perhaps more than one, opportunity of noticing them. At present we shall confine ourselves to the able paper of the Professor of Medical Jurisprudence, on the

EFFECTS OF OPIUM EATING.

Cases and Observations in Medical Jurisprudence.—Case X. *On the Effects of Opium Eating on Health and Longevity.* By JOHN CHRISTISON, M.D. Professor of Medical Jurisprudence and Police in the University of Edinburgh.

Who does not recollect the "Confessions of an English Opium Eater?" and who that has read that extraordinary little volume is not quite made up on the effects of opium, though he has never seen a grain of it in his life? But the jury who tried the Earl of Mar's case could surely never have read the book, or they would not have returned the verdict which they did; nor could the judge: both were in a barbarous and most unjudicial state of ignorance—an argument which Dr. Christison, by the way, should not have omitted in summing up his objections to the verdict. The misery—the ideal existence for thousands of ages in agonies unutterable—the short snatches of blissful repose at intervals; then the

maddening pleasure for an age again, yet all the while the wretched victim committing suicide by his unbridled indulgence in the use of a drug which he knew well was wasting the springs of life, and bringing with it a premature senescence and an inevitable wreck! The man survives; but by what a sacrifice! Is there any thing in the self-denial of the faquires of Hindostan, or of the monks of La Trappe, to compare with the pangs endured by De Quincy in his stubborn self-restoration? It may be all fiction, no doubt, but we feel it as if it were true; and we say, that if judge and jury, in Lord Mar's case, had been readers of good books—as they should have been to decide in a case so important—it would have been utterly impossible for them to decide as they did. But, perhaps, says the reader who knows nothing of Lord Mar's affair, perhaps it was as well that the judicial personages in question were divested of those trammels of strong feeling which would have tied and bound their minds, had they perused the "Confessions?" No; their minds would have been but the more enlightened, and they would have concluded right. The inference from the volume of De Quincy, and from the observations of Christison, are one and the same. But how differently are we led to the result! How does the wand of the philosopher triumph over the talisman of the romancer! How are the wild creations of this extraordinary sensualist softened down by the calm reasonings of the inquirer after truth!

We take it for granted, then, that all our readers are acquainted with the "Confessions." If they are not, we can only pity them, for they ought to be: they would then read the remarks of Dr. Christison with greater zest, and we should promise them a rich treat from the contrast of fact and fiction—if the confessions *be* fiction, of which *we* are by no means persuaded. Dr. Christison's observations are founded upon a trial which took place at Edinburgh a year or two ago; of which the following are the leading particulars:—In 1826 the late Earl of Mar effected an assurance on his life with the Edinburgh Life-Assurance Company for 3000*l*. In September 1828 he died of jaundice and dropsy, at the age of 57; and the amount of insurance was claimed by Forbes and Co. who held the policy

as his lordship's creditors. Payment, however, was refused by the Company; on the ground that Lord Mar had been an opium eater previously to and at the time of effecting the policy; that he continued the same practice during the remainder of his life; and that these facts had been concealed from the Company. Hence the action at law.

It appeared from the general evidence given on the trial, that in addition to the usual satisfactory answers of the party insured and his medical referee, that the latter, Dr. Geo. Wood, who was also the medical officer of the Company, in filling in replies to the queries contained in the schedule—while he replied favourably to the special question in regard to habits, "Are they sedentary or active?" and likewise to the other special question, "Are they temperate or otherwise?"—neglected replying to the general question, "Can you give any and what information respecting his habits?"

The plaintiffs maintained, that even admitting (which, however, they did not) that the Earl was addicted to opium-eating, yet that the company, as they had accepted the life, without this *general* question as to habits being answered, must be understood to have accepted it at a venture, a view of the case in which the judge warmly coincided with the plaintiffs. Lord Mar's factor, coal-manager, gardener, game-keeper, and woodman, as well as a medical gentleman and three private friends, proved that the Earl was temperate in his living, never appeared intoxicated, or as if under the influence of narcotic drugs; had no complaint but rheumatism to afflict him, and that he went out occasionally to the distance of one, two, or three miles at a time. His friends further stated, that there were no visible marks of premature old age about him; and two of them, who saw him frequently till about two months before his death, declared that at first his habits appeared active enough, as he went about his garden and plantations, attended county meetings, and rose early; that he was a shrewd man of business, and given to study; that his intellects were clear and acute; his memory, originally powerful, unimpaired, and his disposition cheerful, unless when the state of his affairs was talked of, when he always seemed desponding and depressed; but towards

the close of 1826, his friends all admitted a change in his habits, which, however, they attributed to the great disappointment of his hopes. The unfortunate Earl, in short, about that time discovered that he was not worth a farthing. He gradually secluded himself from society, lay long in bed, and even went seven days without being shaved; his health and appearance, however, as the gentlemen already alluded to avowed, did not appear to suffer.

But then none of the witnesses above mentioned could speak with regard to his private or domestic habits.

On behalf of the defendants, it was proved by the evidence of his lordship's housekeeper, that he was in the habit for twelve years before his death of swallowing some laudanum before he went out, whenever he was irritated, and at night when he went to bed; and that he had acquired the practice, too, of taking large doses, one of his housekeeper's giving it to him occasionally when he went to bed, at a table-spoonful for a dose. It was also proved by some of his female servants that when he first got up in the morning he was so stiff as to require to lean heavily with his hands on his arm-chair to raise himself, and that he was generally listless and in low spirits; that his appetite was so small that a partridge would serve him three days for his dinner, and that he sat drinking spirits and water till late in the evening, when he became not unfrequently intoxicated. Dr. Abercrombie also deposed, that when he visited the Earl of Mar in 1825, for stomach complaints, he found him with a constitution enfeebled and broken down, although he had not any definite complaint. It was further proved, that about the close of 1825, and the early part of 1826, he had purchased at the rate of forty-nine grains of solid opium and one ounce of laudanum daily; and that during the rest of 1826, as well as the two subsequent years, he purchased opium, chiefly in the form of laudanum, at the rate of two, two and a half, and three ounces daily.

Thus the evidence clearly went to prove that a confirmed habit of taking laudanum really did exist; yet it is a curious fact, that of several eminent physicians, among them Dr. Christison himself, with Drs. Abercrombie, Alison, and Duncan, who were examined on the

part of the Insurance Company, not one could prove from direct experience that such habit was prejudicial to the health. Mr. Macfarlan, indeed, a surgeon-apothecary, deposed that he knew one person, a female, who used laudanum to the extent of two ounces daily, for many years, and who was about the age of sixty—of what disease he could not remember.

The Lord Chief Commissioner summed up for the plaintiffs, and the jury decided in their favour, by finding the Insurance Company liable.

But who will not agree with Dr. Christison in his deliberately expressed opinion, that the verdict in this case was "neither founded in reason, nor supported by the evidence?" The presiding judge, in his charge, made some most extraordinary assumptions: he neither considered that Earl Mar took opium to the extent that should make it important to the Company to know it, or obligatory on his lordship to reveal it; nor did the Lord Commissioner think it possible that opium could have been taken to a pernicious extent, when Lord Mar, to his personal friends and out-of-door servants, never presented any of those effects usually believed to arise from such a practice. On these points it is well observed by Dr. C. that the judge must have been most lamentably unable to appreciate *our* fact given in evidence—namely, the habitual dose of a table-spoonful, sworn to by the housekeeper; nor could the court have been aware of the peculiar effects of the opium eater's allowance. "It will be seen," says the learned professor, "from what will be presently said of the operation of opium upon persons addicted to it, that in this country, at least, the usual effects of the opium eater's dose is neither to throw him into a state like the excitement of intoxication from wine and spirits, nor to induce for some time heaviness and stupor, but simply to remove dulness and depression—to make him alert in his occupations and conversable in his intercourse with others—to occasion, in short, a state of mind and body which no one would suppose, at all events, no one not aware of the habit would suppose, indicated any thing unusual in his condition at the time." Here, we must confess, the Doctor is considerably at issue with the English opium-eater: M. De Quincy would hardly allow that the

effect of *his* dose was merely to put him on a par with other people.

With regard to the question, "whether the habit of opium eating is detrimental to health and longevity?" Dr. Christison gives us some most valuable information. He does not think that it is by any means a *necessary* consequence that the habitual use of narcotics must tend to shorten life: but such he infers to be the *general* rule, though it must be confessed that the exceptions are hitherto overwhelmingly the more striking in the inductive list. "The following cases," says Dr. C. "have been communicated to me by several of my friends, on whose information I can place reliance."

"1. A young lady of five-and-twenty has taken it largely for fifteen years. It was first administered secretly by her nurse to keep her quiet and save trouble; and the unhappy lady was subsequently compelled to keep up the practice for her comfort. She enjoys good health. 2. A female, a patient of mine in the Infirmary, a martyr to the rheumatism, took it for ten years previous to her fortieth year in the quantity of a drachm daily of solid opium. She then gave it up. Six months afterwards she was attacked with jaundice; subsequently she was several times severely ill of rheumatism; and she died in her forty-third year of consumption. This woman, however, led a licentious life from an early period. 3. A well-known literary gentleman who has taken laudanum with some intermissions for twenty years, and occasionally to the extent of nine or ten ounces daily, has now attained his forty-fifth year. He is spare in form, looks older than he is, but is capable of undergoing a good deal of bodily fatigue, and enjoys tolerably good health so long as he takes sufficient exercise. His allowance when I had last an opportunity of conversing with him was about nine drachms of laudanum daily. 4. A lady in this city, after drinking laudanum to excess for upwards of twenty years, died about the age of fifty. No information could be supplied of the disease of which she died. 5. A lady of the same age takes about three ounces daily, and has used it for many years. She appears to enjoy good health. 6. A lady, about sixty years of age, has taken it above twenty years, and is in good health. 7. A charwoman, who had been in the daily practice of drinking two ounces of lau-

danum for many years, died at the age of sixty. The gentleman who has stated this fact, does not remember what disease she died of, although he dissected the dead body. 8. An eminent literary gentleman—[Here is our valued friend]—*I am informed*—[Why,—is not Dr. C. acquainted with him? Of course, he has read *the book*—] has been in the habit of taking laudanum since he was fifteen; and his daily allowance has sometimes been a quart bottle (twenty-six ounces) consisting of three parts of laudanum and one of alcohol. Enormous as this dose may appear, I am assured this fact is well known to his acquaintances. He is about sixty years of age, and enjoys good health. 9. A lady of seventy, now alive, has taken about an ounce of laudanum daily for nearly forty years. She enjoys tolerable health, and every year travels great distances to visit her friends. 10. An old woman of eighty died a few years ago at Leith, after taking about half an ounce of laudanum daily for nearly forty years; and she enjoyed tolerable health all the time."

These are, indeed, very curious facts, shewing how little are *à priori* assumptions to be trusted relative to the affinities between opium and the human stomach; and we are persuaded, that if Mr. De Quincy ever exaggerates, it must be in dwelling so piteously on the distresses of his digestive apparatus. It is quite clear from the above list that a considerable number of opium-eaters may attain a good old age, and that taken with caution, however largely taken, opium is no more injurious to longevity than addiction to ardent spirits is. Nobody will dispute the point with Dr. Christison, that both drunkenness and opium eating are dangerous habits, and very liable to kill. He will not, indeed, allow that even a fair proportion of opium eaters live out their due proportion of life; but it is a remarkable fact that he cannot produce *a single case* in which the habit proved prematurely and decidedly destructive. The probability, however, is, as he says, that many really die at an early age of the effects of opium eating whose habits are never heard of, simply from the circumstance that they die young, before their secret is detected.

How secretly the habit is sometimes indulged, may be conjectured from the following notes:—"In case sixth, the lady's medical attendant was

so completely ignorant of the habit existing, that, on being summoned to her aid on account of an attack of diarrhœa, he ordered an ordinary dose of opium, and was first warned of the true state of matters by the apothecary, to whom the prescription was taken, and who had supplied the drug for her daily use. In case ninth I have taken pains to ascertain the fact, that some of the lady's most intimate friends are not at all aware of her having fallen into the habit. In case third I can state from personal observation, that even after being told of the habit existing, no one could discover it from the gentleman's appearance, conversation, or acts. I am further inclined to think, that in many persons unaccustomed to eat opium, this drug, when it does not induce sleep or produce disagreeable idiosyncratic effects, occasions tranquillity and brilliancy of ideas; and that such effects will be often experienced, when its soporific influence is resisted by an exertion of the will. In all such instances, and notoriously in all opium eaters, the stage of elevation is followed by one of gloom, depression, and loss of appetite. But the opium eater usually takes care not to be seen at that time; and if he is compelled to appear in society, he alters the face of matters by renewing his dose."

To some it might appear strange that opium eaters are not harassed with costiveness, and perpetually obliged to have recourse to laxative medicine. Some importance was attempted to be attached to the circumstance that Lord Mar was very little affected in that way; but it was very properly abandoned at the time of the trial. The truth is, that a very large proportion of persons addicted to the use of opium seem to require no laxatives at all: the subject of case 5, for example, requires no laxatives; nor did the charwoman (No. 7) get laxatives at the shop where she got her laudanum. Yet the occurrence of costiveness after opium must still, in Dr. C.'s opinion, be accounted the *general rule*—the cases of unobstructed bowels just alluded to being most probably no more than exceptions.

And the conclusion to which his reasoning brings the professor is this, "*I cannot bring myself to think that the habitual use of a drug which produces such permanent narcotic effects as opium, disorders subsequently the diges-*

tive functions in so great a degree, leaves those who use it habitually in so miserable a state during the interval of using it, as appears from their own confession, and leads obviously to emaciation and a worn-out elderly appearance at an early period of life *can be consistent in general with the enjoyment of health, and the chance of an average prolongation of the term of human life.*"

The practical inference is, that the *opium-eater's life is uninsurable*; and with this persuasion, Dr. Christison thinks it his duty to remind both Companies' physicians, and medical referees, that the pernicious habit may exist where it is least suspected by medical attendants or intimate friends, and that it is by no means in general to be detected by the phenomena to which, according to popular belief, it usually gives rise.

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Letters on Cholera. By Dr. BECKER, of Berlin. Murray, 1831.

THIS is a very interesting document, abounding in details at once extensive and minute, followed by logical and important inferences. Dr. Becker is a contagionist, and the grounds of his opinion are of the most convincing description; but our space admits not of our entering upon them; and our object at present is only to point out to our readers where they may find a most lucid and perspicuous account of the origin and progress of the cholera in Europe generally, and in Berlin more particularly. The letter is addressed to his former preceptor, Dr. John Thomson, of Edinburgh; and from being entitled No. I., we hope that more are to follow. It is written in a style remarkable for its simplicity, and which, from the pen of a foreigner, may be looked upon as extraordinary, considered merely as a piece of composition. Dr. Becker shews himself to be a pupil worthy of his distinguished master.

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The Elements of Chemistry Familiarly Explained, and Practically Illustrated. Part I. *Attraction, Heat, Light, Electricity.* Murray, 1831. 100 Cuts. 6s.

A PRETTY little work, well executed, and capitably "got up." It will prove very attractive to the young chemist—rendering the first steps of the study simple and attractive.

A Treatise on the Diseases of the Heart and Great Vessels, &c. By J. HOPE, M.D. &c. Kidd, 1832.

DR. HOPE is well known to the readers of this journal, as the author of some very interesting contributions on the subject of the heart, published some months ago. In the volume before us we find his views more fully detailed, and his doctrines expanded into elaborate and comprehensive essays, constituting the most complete work in the English language on the subject to which they relate. At a future time we shall enter upon the consideration of its contents, in one or more articles; but, for the present, the press of other matters, if not of greater, at least of more immediate and current interest, denies us the space which would be requisite to do any kind of justice to the subject.

MEDICAL GAZETTE.

Saturday, January 14, 1832.

“*Licet omnibus, licet etiam mihi, dignitatem Artis Mediæ tueri; potestas modo veniendi in publicum sit, dicendipericulum non recuso.*”—CICERO.

ON THE CIRCUMSTANCES BY WHICH THE RAVAGES OF CHOLERA ARE INCREASED AND DIMINISHED.

WE endeavoured in two leading articles, published some weeks ago, to investigate the manner in which Cholera is propagated, and our inference was, that the disease, whatever its origin may have been, is now at least certainly transmissible, and often transmitted, from man to man. Our readers need not apprehend that we are going to resume the discussion on contagion; indeed we have nothing new to urge on that point; and if we did not succeed in convincing them before, nothing further which we could urge would be likely to win their assent to our opinion;—but we are desirous of directing their attention on the present occasion to some other points connected with this

important subject. As many are exposed to the causes of cholera with impunity, it becomes a very important inquiry to investigate, what are the circumstances which facilitate or retard its propagation? what is the condition or peculiarity of constitution which in some gives effect to that influence which the majority resist?

In the first place it is to be remarked that a certain, as yet unascertained, proportion of mankind, seem incapable of having the disease at all, and that a similar immunity is acquired by those who have had the disease and recovered. The first of these positions is generally admitted, while with regard to the second, the records of the Cholera in India have clearly demonstrated that those who escaped the first attack had very rarely indeed to encounter a second. It thus appears that the number of persons who present materials on which the pestilence may feed, must daily become diminished in every country where it rages, not merely by the deaths which occur, but by many purchasing their immunity by having passed through the ordeal with safety. Besides, it is distinctly made out by Mr. Jamieson* that not only those who had undergone its attack were comparatively free from its future ravages, but that individuals, and even large bodies of men who had been exposed to its influence—that is, who had been in places where it was prevailing and escaped, were thereby ascertained to be less liable at any future time to be brought under its influence. For example, the centre division of the Bengal army suffered very severely from cholera, which after a time subsided. Now “when this force broke up after the termination of the campaign, his Majesty’s 24th regiment of dragoons, and 87th regiment of foot, and the 1st battalion 8th regiment native infantry marched to Cawnpore, where they were stationed in April and May, when the city and

* Bengal Report.

cantonment were suffering from the disease. At this time the 24th dragoons remained quite free; the 87th had only two slight cases among recruits, who had not been with the centre division—no death; and the 1st battalion 8th native infantry had, according to one statement, no case, and according to another, three or four, all slight attacks*.” Yet the disease raged around them, and the corps last mentioned was actually placed right between two others, both of which suffered severely. This is an illustration of the diminished susceptibility resulting from having been previously subjected to the visitation of the disease; and on this we would remark, the true explanation appears to be afforded of the curious phenomenon occasionally presented by a regiment remaining healthy and untouched, while others in the vicinity are being decimated, a circumstance which has often been adduced to prove that the disease is not contagious. But it is obvious—if our data be correct—that such a body of men stands unharmed in the midst of surrounding cholera, just as persons who have previously had small-pox, or been vaccinated, enjoy comparative safety during a variolous epidemic. This principle of the great infrequency of second attacks, while it brings the disease closer to those which are acknowledged to be contagious, is in the highest degree consolatory and encouraging, because it teaches us that even if cholera should revisit, during the life-time of the same generation, any place which has once been fully subjected to its scourge, it will find the population much less obnoxious to its influence. We hear, indeed, some who have been in India talk of having had the disease more than once; but these cases receive a ready explanation in the notorious fact, that every attack in which vomiting or purging was present, was dignified with the name of Cholera,

while some are vain of “the dangers they have passed,” and boast of having had the disease half a dozen times. The most authentic records of the disease, both in Europe and Asia, however, are opposed to this idea. “We should, perhaps, not be far wide of the truth (says Mr. Jamieson) were we to affirm, that of the many millions attacked, the returns of the whole country do not afford a score of well-authenticated cases of a recurrence of the disease after the removal of the debility and every other consequence of the primary attack*.”

But in places which the malady has not yet visited, it is of still more pressing moment to decide what class of persons is most liable to its inroads. “The pestilence knows its victims,” says the apathetic philosopher of China, and regarding it as a convenient mode of thinning a population too dense for his resources, he takes no steps to mitigate its violence. True it is, that the lowest classes of society, whether their degradation arise from destitution or from vice, are the first to fall beneath the blow; but it is no less true that the multiplication of the disease, up to a certain point, increases in at least an equal ratio the intensity of the contamination, and thus brings under its influence those who would have escaped had this concentration of the virus been prevented. There is no well-grounded security in the *philosophic* view which this stoical indifference to the miseries of others would beget; and we insist upon this the more, because, though backwardness in the cause of charity is a reproach which cannot fairly be cast upon our countrymen, yet there is a disposition inherent in human nature to contemplate with more complacency dangers from which we deem ourselves comparatively free. Already the idea which is daily becoming

* Ibid.

* Bengal Report.

more prevalent, that cholera confines its attacks to the profligate and to the poor, has had a wonderful effect in reconciling their wealthier and more prudent brethren to the presence of the malady ;—indeed it is wonderful with what resignation to the will of Providence those who regard themselves as in the “ better ” class, behold the ravages of a pestilence which they conceive will never reach their privileged order. But if the general principle be true, it is so only to a certain extent, and its operation has been most conspicuous where the most active exertions were made to provide for the relief of those who had not the means to command it for themselves. In none of the towns of Europe which cholera has yet visited were preparations of a more extensive and judicious nature made than in Berlin. The city was divided into fifty-eight (!) different districts, each distinctly marked and provided with its committee of management and its medical attendants. There was no quarter of the town in which a cholera hospital was not opened, and persons were appointed to be on the look-out for cases, with a view to giving instant notice of their occurrence, that immediate assistance might be afforded. Under these excellent arrangements the disease was prevented from extending in a degree at all commensurate with its ravages in most other places, and its almost total exclusion from the higher ranks, may be gathered from the statement of Dr. Becker, in his most interesting letter to Dr. Thomson, in which, speaking of its comparative infrequency in private practice, he says, “ nor has a single case of cholera occurred in any of the numerous families who habitually consult my valued friend Dr. Horn, well known to have by far the most extensive practice in Berlin.” When, however, from a machinery thus admirably contrived and called into ope-

ration the moment it was required, we turn to the arrangements made in this metropolis, we must acknowledge that in some things arbitrary governments have still the advantage of us. From mutual jealousies, a spirit of independence, and unwillingness to submit to dictation, most of the parishes in London are to this hour lamentably devoid of the necessary preparations. In some few districts small receiving houses have been prepared, but they do not amount to the fiftieth part of what ought, ere this time, to have been completed. Not only is it requisite that the subdivisions into districts should be accurately made, but—houses to serve as hospitals, if required, should be prepared within a moderate distance of each locality when the disease is most to be apprehended. We are by no means among those who would advocate measures calculated to excite any thing like panic—and we have formerly expressed ourselves strongly upon this point—but we are fully convinced, from a careful study of the history of Cholera, that, as a great general rule, its ravages have been most appalling, where least preparation had been made for its reception, or where (as at St. Petersburg) the infatuation and brutish ignorance of the people rendered all sanitary measures abortive.

It is likewise but too true, as an able contemporary* has observed, that, “ misled by the ignorant conjectures and reckless averments of editors and other writers in the daily press, especially of London, many persons have been induced to believe, that there is little occasion to dread the arrival of the threatened visitor, or to make any serious preparation for its approach.” The truth appears to be, that in the moral as well as in the physical constitution of man, excitement is often follow-

* Editor of the *Edinburgh Medical and Surgical Journal*.

ed by perilous inactivity: thus the public having had their fears raised above all reasonable calculation, and soon finding that day after day passed on without the pestilence appearing among them, and that the machinery of society went on as before, began after a time to shake off the incubus of their terror; and finding that they had clothed the evil with more than its real horrors, have passed into the equally erroneous and more dangerous condition of doubting or denying its existence altogether; indeed some of the conductors of the public press have had the unparalleled folly to hold it up to the ignorant and uninformed as a phantom only—a mere political contrivance, got up for party purposes. This state of treacherous apathy has led, in many places, to a neglect of the necessary preparations, and, we believe, in none more remarkably than in this metropolis.

It would certainly be proper to have at least some hospitals in London, with every preparation and arrangement for giving an immediate trial to the numerous and important suggestions which have been made by various writers, but which the want of the necessary appliances and means has hitherto rendered impossible. We allude more particularly to the inhalation of oxygen and other gases—the application of galvanism, and other agents—which, though partially tried, have never been adopted to an extent at all calculated to establish either their power or their inefficiency. At Sunderland partial success attended the inhalation of a few bladders full of oxygen; and a perceptible effect upon the pulse was produced in some cases, even by injecting the gas into the rectum. Now, in order to try the effect either of this, or of nitrous oxyde, on the lungs, it ought to be so contrived, not that the patient is merely enabled to make a few laboured inspirations,

but he ought to be surrounded with an atmosphere of the gas to be breathed.

Medical men ought to be engaged to devote their whole time to the cholera patients, and they ought to be liberally paid. It was not without indignation that we heard an attempt had lately been made in the opulent parish of St. James's, to impose this great additional labour and responsibility on the ordinary attendant of the poor. The attempt failed indeed, but it is disgraceful that it was ever made. Taking the whole state of the metropolis, we regard the organization of the machinery by which it is intended to break the force of the visitation, should it occur, as exceedingly defective; but John Bull never will learn any thing, except under the hardest of all masters—experience; and your parish vestry is a variety of the genus, which even this severe monitor seems incapable of instructing.

But to return from this digression: the proportion of mankind susceptible of cholera seems to be decidedly smaller than holds good with respect to small-pox, plague, or other malignant epidemic or contagious diseases; we know of no data, however, on which any rational calculation as to the relative numbers can be founded. Then there are other certain peculiarities, which are distinctly traceable with regard to cholera: thus children, though not entirely exempt, are much less liable to it than adults; women too, according to general observation, are less susceptible than men, a circumstance which shews that something more than relative degrees of strength must be taken into the account*. But again, in all countries prostitutes have suffered very severely, proving that depravity is the most powerful ally to cholera in either sex alike. There are differences between

* The only writer who gives a different account on this point, so far as we know, is Dr. Ogden, of Sunderland, in an interesting letter published in our Supplementary Number last week.

most writers on certain circumstances connected with the disease, but on this point all agree:—intemperance gives a claim to the pestilence which it never overlooks;—in every town and every district, from the Ganges to the Wear, the drunkard has been the object of its earliest attack and its most ruthless visitation. Nor was this principle ever more conspicuously illustrated than by the occurrences at Gateshead, where intoxication, which was indulged in to a frightful extent on Christmas day, accompanied by a strong breeze blowing from the north and over Newcastle, where the disease existed, was followed by the lamentable irruption of cholera which has been so often alluded to.

Other causes, producing an excitable state of body, have been remarked as generating a similar proneness to the disease: thus, in India, pytalism was so frequently immediately followed by cholera, as to leave no doubt of the connexion between them; and the same was observed to hold good with respect to convalescence from fever, or other diseases attended with much debility. That any thing which weakens the body and renders the circulation languid, makes such individual more susceptible of the malady, may therefore be assumed as a well-established fact; but we cannot turn to the converse of the proposition with equal confidence—we cannot regard any degree of bodily vigour or of habitual temperance, as doing more than diminishing susceptibility; they can never entirely destroy it. “If a surgeon, on the arrival of a regiment in India, were to point out those whom he thought most likely to fall victims to cholera (says Mr. Orton), how many of his *marked men* would be passed over by the disease, and how many more would it single out whom he never suspected of predisposition. How often have we seen persons of the

most robust and healthy habit, and the last whom we should expect to be obnoxious to cholera, seized by it, even when it was not prevailing to any considerable degree or in its very worst forms?” Still, on the great scale, it is true that they who lead regular and temperate lives, and who are provided with proper clothing, are less liable to the disease than those around them who are differently circumstanced. “The almost complete immunity of the garrison of Berlin from cholera (says Dr. Becker), is chiefly to be ascribed to the pains taken to give the soldiers additional warm clothing, and to force upon them a healthy diet;” and instances of a similar nature might be multiplied to almost any extent.

Of all the obvious deviations from the ordinary condition of the body, that which weakens the defensive powers of the system against cholera most remarkably, is diarrhœa; a fact which has been observed alike in India and Europe. Nay, it would appear that purging, artificially induced, has occasionally been so speedily followed by an attack of cholera as to have been regarded as the predisposing cause. It may, however, be matter of reasonable doubt whether the looseness described as *preceding* the attack of cholera, be not in reality the disease itself, in its early stage or mitigated form. In many, particularly if met by proper remedies, it never goes farther; and we are inclined to hope that this favourable result may often be secured by means of proper remedies, particularly emetics*. Indeed, the evidence in favour of this class of remedies in the treatment of cholera, in its early stage, and in conjunction with bleeding, becomes daily more and more convincing; and is one among many proofs of the advantage

* See the papers of Mr. Searle and Dr. Lindsey, in our Supplementary Number of last week.

which has been derived from the many useful facts and suggestions, on the subject of cholera, communicated through the medium of the periodical press. It is true that the remedy is not new; but it is equally true that it was, to a certain extent, repugnant to the notions we had in this country formed of the disease, and that it is but recently, and since the introduction of the malady into Europe, that this treatment has become more generally adopted. It has, indeed, been used more extensively abroad than at home; probably because we are here more under the influence of the Indian practitioners. From the details contained in Sir William Crichton's excellent paper, and from the cases treated at Sunderland, which we published last week *, it will be seen that brandy, opium, and calomel, with or without bleeding, were the great remedies employed on the first irruption of the disease, but without any evidence which would encourage us to place confidence in them: in fact, the cases alluded to demonstrate, in a very striking manner, the inefficiency of mere stimuli, and, as a consequence, the necessity of having recourse to some other agents to rouse the circulation;—such agents seem to present themselves in emetics. But we must postpone till another opportunity what we have farther to say on this subject.

DR. CLANNY'S ALLEGED DISCOVERIES.

THE readers of the Gazette will probably recollect that we have, on several recent occasions, alluded to the equivocal light in which Dr. Clanny, of Sunderland, has lately exhibited himself, in consequence of having applied to the Government for a reward on account of certain discoveries regarding cholera, to be communicated to them, in the event of coming to terms. Dr. Clanny's first

address was passed over with silent contempt, but upon his repeating his proposals, he received a letter from Downing-Street couched in such terms as prevented any attempt at renewing the correspondence. Foiled in his endeavour to obtain the required price for his promised disclosures, the Doctor turns upon us as his betrayers, and in allusion to the Gazette, thus addresses himself to the Editor of that periodical, to which all turn with the certainty of a favourable reception, when they have any thing abusive to communicate:—

“The uncalled-for attacks made upon me by the Editor of that journal, to whom I never spoke, and whom I never could have offended in thought, word, or deed, astonished and disgusted me. What has this personage performed for medical science that he should thus treat a country physician, whose greatest delight for several years has been to investigate the nature of disease by the aid of science, and who, as all his friends know, has spent a fortune in these pursuits? Had such physician, after the history of the safety-lamp, and the investigation of the proximate cause of typhus fever, no right to ask the government of his country what rewards he might expect in the event of his making discoveries upon the subject of epidemic cholera directly known to that government, without having the advantage of publishing a volume on the spot where the disease commenced in these islands? Had he not a right in a free state, when his request was not replied to, to state that he was not inclined, at his inconvenience and detriment, to make such disclosures public, except he were suitably encouraged and patronised? And why did a “confidential” correspondence with Lord Grey and Lord Auckland find its way into such a publication as the journal in question? While, too, such a physician well knows that the ample taxes he has paid for the last

* See Supplementary Number, p. 511*, &c.

thirty years have been partly expended in pampering pensioners, he recollects that, with the exception of Dr. Jenner, no medical man, whether a man of science or not, ever received one farthing of public money since that physician commenced his professional career."

Divesting the sentiments in the above extract of the disguise in which they are clothed by Dr. Clanny, they amount to this:—1. That the Editor of this journal has no personal knowledge whatever of the Doctor, and therefore could not have animadverted on his conduct from any private or sinister motive. 2. That the present is not the first occasion on which Dr. Clanny has claimed a reward for supposed discoveries, nor the first time that such claim has been refused. 3. That he holds he had a right to make the best bargain he could. 4. That as he did not obtain his price, he was "not inclined to make such *disclosures public*." As to what follows, about his "confidential" correspondence with Lord Grey finding its way into this journal, we have to say,—first, that the idea of his having a *confidential* correspondence with the Minister on a matter so entirely public as the alleged discovery of a cure for cholera, is perfectly ridiculous; and, in the next, that the correspondence alluded to never was published in this or any other journal, but merely stated to have occurred,—a statement, the accuracy of which Dr. Clanny admits. Then as to the insinuation that there is any parallelism between his case and Jenner's, he forgets that the great philanthropist in question did not endeavour to make a bargain for the sale of his discovery while hundreds were dying around him, but promulgated his views at once and unconditionally to the world, subsequently receiving his recompense as the free gift of a grateful country. He says that we insult him, as the reward

of "his pains, anxieties, and cares:" not so; we censured him, because he offered, if paid, to communicate to the Government certain discoveries, which he conceives himself to have made, and that not being so paid, he has hitherto kept them to himself.

That any discovery has been made, we do not for a moment conceive, because, as we before stated, the rate of mortality at Sunderland has remained nearly one in three. Dr. Clanny, commenting on this statement, says, "this is false; as our daily lists show a gradual decrease of cases and of deaths up to this hour." Now the point alluded to is a question of fact and not of reasoning; it can only be settled by the official returns, and by them we find that, during the second week when the disease may be looked upon as fully established, the number of cases (deducting those of diarrhœa) was 44—of deaths, 15; that is, as nearly as may be, one in three: whereas in the sixth week, at the time we published our statement, the number of cases was 80, and of deaths 39; that is within a fraction of one-half;—or taking it in a different way, during the first three weeks, the number of cases 197, and of deaths 55, or considerably less than one in three. During the second three weeks, the number of cases was 223, and of deaths 97, or not much under one in two; nor, even if Dr. Clanny adds the whole of the cases of diarrhœa, will it bring down the mortality to so little as one in three during the period in question. It thus appears that Dr. Clanny has the unparalleled insolence to accuse us of falsehood for making an assertion notoriously true, corroborated by the official returns, and of the inaccuracy of which no proof is offered,—unless the sagacious observation, that the number of *deaths* had decreased with the number of *cases*, be admitted as such. Lastly,—and it is intended to be a very severe remark,—Dr. Clanny says, that

this journal "exists by patronage only." Most true it is by patronage only that it exists, and not only exists, but grows and flourishes. If Dr. Clanny will look at the number of that journal in which his own letter stands unhonoured amid two or three nameless contributions, couched in the same spirit of abuse,—we say, if he will turn from that to the corresponding number of this journal for the same day, he will find papers on a variety of medical topics, *from nearly thirty original correspondents*, at home and abroad. This is our "patronage,"—the general support of a liberal and enlightened profession; and long may the Gazette owe its "existence" to such extensive and wholesome supplies.

THE CHOLERA IN EGYPT.

At a recent meeting of the Académie de Médecine, a letter from Alexandria was read, dated Nov. 18, and containing among others the following particulars:—"The cholera entered this country by Suez, from Mecca, where it made its first appearance in May last. At Suez it cut off 135 persons on the three first days of its visit (July 30, 31, Aug. 1): thence it proceeded to Cairo, where the first thing the inhabitants did was to take to the Nile; the whole river was speedily covered with boats. In spite of a double *cordon*, the disease next broke out in Alexandria; and having reached the fleet, out of 500 men cut off 350 in twenty-four hours. Forty persons belonging to the harem at Cairo were seized with cholera, and the Pacha was so much alarmed, that he embarked almost unattended, except by his physician, to take refuge in Upper Egypt."

LITHIOTRITY.

M. LISFRANC has been lately operated upon, by M. Civiale, for stone in the bladder. A complete cure is said to have been effected.

MAGENDIE'S REPORT.

M. MAGENDIE, on his return to Paris, made the earliest report of his late mis-

sion, verbally, before a full meeting of the Académie des Sciences. He referred to his letter of the 3d of December, in which he had used the term *cadavériser*, as the most expressive he could find for conveying his notions of the most striking peculiarity of cholera; and he had since seen no reason for modifying that expression. The coldness, blueness, and clammy sweat of the skin, he described as very remarkable; the heart beating but twelve or fifteen times in the minute; and the respiration embarrassed exceedingly. There was one circumstance, however, regarding this complaint, which he thought looked rather favourably for the prospects of France, and that was, the sort of locality which it had nestled itself in at Sonderland. Here the speaker drew a picture of that unhappy town, which, however satisfactory and consoling for the learned academicians, would be any thing but ornamental to our pages, or agreeable to our readers. Suffice it, that all the colouring which could be imparted by the liberal use of every epithet indicative of squalid and hideous misery, was profusely supplied. He had seen after all, he said, but one body opened, and that by special favour. There was the most rooted prejudice at Sunderland to post-mortem examinations.

MEDICO-CHIRURGICAL SOCIETY.

Tuesday, Jan. 10, 1832.

MR. LAWRENCE IN THE CHAIR.

Polypus of the Uterus—Diseases of the Spleen and Absorbent Glands.

THERE were two papers read this evening: the first of them was by Mr. Langstaff, detailing the particulars of a case of *Polypus of the Uterus*, which had recently occurred in his practice, and of which he exhibited the wet preparation. It had passed through the os tincæ, and, on the part which protruded into the vagina, a quantity of coagulated blood was effused, which being washed off, the mucous membrane was found to be ulcerated and absorbed beneath; and Mr. Langstaff had no doubt but that the fatal hæmorrhage had proceeded from that part.

DR. LEE bore testimony to fatal hæmorrhage in this complaint, sometimes, at least, arising from the cervix of the uterus. In one case he had found the vessels of the cervix full of blood, and they had evidently

been discharging it copiously. Before sitting down, he would ask a question of Mr. Langstaff, whether he did not look upon the structure of polypus uteri as identical with that of the sero-fibrous tumors of the same organ, or differing merely in the membrane with which they were covered? It was his opinion that polypus, as a tumor of the uterus, only differed from the sort of tumor just mentioned by its position; in fact, one and the same kind of morbid growth might have its commencement beneath the peritoneal covering of the uterus, and protrude, in the course of its enlargement, into the abdominal cavity; or it might begin in the muscular coat of the womb, and protrude either way; or, finally, originating beneath the mucous membrane, it might push that membrane before it, and form a tumor in the uterus itself, which commonly went under the name of polypus uteri.

MR. LANGSTAFF would by no means admit the identity of these tumors. He understood Dr. Lee to assert that one sort originated in the peritoneal coat, another in the muscular, and a third in the mucous lining of the uterus. It was his opinion that the tumors began, in each case, in the cellular tissue under the membranes, which they pushed before them, and ultimately destroyed by absorption; but that in no instance did the morbid structure begin in the membranes.

DR. LEE explained: he had used the term "beneath," not "in"

MR. LAWRENCE was sure that there was no difference of opinion between the gentlemen who had just sat down; each seemed to admit that the structure or body of the uterus was the part primarily affected, and that the morbid growth took on its distinctive character from the direction in which it proceeded, and the membrane in which it was enveloped.

DR. HODGKIN made some remarks.

A series of preparations were now exhibited, belonging to Mr. Langstaff, and intended to illustrate the curious history of hydatids. In some of the specimens, of which descriptive accounts were read, there was seen one very large hydatid, containing within its globular cavity innumerable smaller ones. Specimens of flukes from sheep were also shewn, belonging to the same gentleman.

THE SECRETARY now proceeded to read the other paper to which we alluded: it was by Dr. Hodgkin, *on some morbid appearances occurring in the absorbent glands and the spleen*; a very elaborate performance, and rendered peculiarly valuable by the number of preparations and drawings with which the subject was elucidated. We intend next week to give an ample report or abstract of the paper; our limits at present not allowing us to go farther. After some valuable remarks

from the President, confirmatory of Dr. Hodgkin's views, the meeting adjourned; it being understood that the second part of Dr. H.'s paper is to be read on the next night of meeting.

MEDICO-BOTANICAL SOCIETY.

13th December, 1881.

AFTER the election of several fellows and the announcement of presents from Capt. Friend, Dr. Tutbill, Mr. Bennett, and others, consisting of a new species of sarsaparilla, among numerous specimens of plants of reputed medicinal efficacy in the countries the above-named gentlemen had severally visited, and of which further accounts were promised by the donors; Mr. Burnett proceeded to deliver his lecture, previously announced for this meeting, on the connexion of Medical with General Botany.

It would be impossible, within the ordinary limits of a report, to give a fair abstract of all the subjects which the lecturer discussed, as bearing on this question; we shall therefore content ourselves with a notice of their most characteristic features.

He opened the lecture with a view of the late progress and present state of medical botany, dwelling on the rapid advances it had made since the institution of the Medico-Botanical Society; and he seemed inclined to attribute its former comparative insignificance and neglect, to the very inefficient manner in which it could be taught, when merged in, and forming, with the medical student, only a subordinate branch of *materia medica*, which, in truth, should rather be esteemed (at least the medical department) a branch of it.

He then proceeded to shew that *medical* could not, with propriety, be severed from *general botany*, any more than *medical* can be separated from *general chemistry*; as, although the subsequent researches of the medical botanist and chemist may, and should, be especially directed to the medical departments of both sciences, he declared it as his decided opinion that such special pursuits could not be efficiently carried on by these who are unacquainted with the general principles of the sciences alluded to, and whose early studies have not been the same with those of the general botanist and chemist.

In the course of the lecture, the Professor took occasion to answer several objections which have been from time to time raised against the study of botany, and proved, by numerous examples, the advantage it is of both to the general and the medical philosopher. He commented especially on the light which vegetable anatomy, physiology, &c. &c. have thrown upon the structures and functions of brute animals and

man; and we only regret that our space compels us to omit the very curious illustrations he adduced, as well as the digression on the philosophy of final causes. The remarks on the medical application of botanical geography, as indicative of the temperature and salubrity of various places by the plants there growing, were likewise very interesting and important; but as a future lecture is to be devoted to this subject, we shall then have an opportunity of entering more at length upon a matter which now was only cursorily and, as it were, incidentally mentioned.

The meeting was full. Indeed this society seems to be better and more regularly attended by its members than most other learned associations that we visit; and we wish that many who lend their names to swell the lists would take our hint, and also lend their persons more frequently to fill the benches.

REPORTS OF CASES OCCURRING AT PUBLIC INSTITUTIONS.

GUY'S HOSPITAL.

Fatal Case of Puncturing a large Thrombus, connected with the Vena Saphæna—Post-mortem Appearances.*

SAMUEL BASSET, æt. 47, a man who had been much in tropical climates and led a dissolute life, presented himself at the surgery on the 26th of July, 1834, having a tumor on the right leg, which he wished to have removed: accordingly, he was admitted into the house under Mr. Key. The tumor was situated immediately below the head of the tibia, a little to the inner side of the leg, in the corner of the saphenic vein, was as large as a lemon, somewhat lobulated, and rather softer in some parts than in others. Directly below the tumor was a small, hardened varix: this was evidently connected with it; and the vein, nearly up the whole of the thigh, could be felt hard, like a cord, under the finger. The man stated, that for many years he had had a large vein, in the situation of the tumor, which always disappeared during the night, and he had also been subject to an ulcer of the leg; that about three weeks before his admission, he worked very hard at mowing grass; he then perceived the vein to become larger and inflamed, and for several days it had prevented his working. At the time of his admission the integuments were livid, but showed no disposition to ulcerate: the veins in the lower part of the leg were not at all varicose.

A short time after his admission he had

an attack of the catarrhal affection, which was so prevalent about that time: by the use of diluents and purgatives he recovered, and on August 15th the tumor was punctured with a lancet, and the contents, consisting of half coagulated, venous blood, looking like black currant jelly, squeezed out; no fresh blood was observed to make its escape. The wound was closed by lint, and pressure made with plaister.

16th.—A little feverish; slight pain in the leg; rather restless; bowels not open.

A dose of castor oil to be given.

17th.—Very restless; thirsty; pain in head; tongue furred and dry; pulse 120, and irritable; skin hot and dry; leg swelled, a little inflamed; constant nausea, with occasional vomiting; tumor rather painful, plaister removed; a small quantity of thin sanious matter, a little half decomposed blood, and also a very small portion of fresh escaped.

A poultice to be applied to the tumor; evaporating lotion to the leg.

R Tinct. Hyoscyam. ℥xx. c. Mist. Effervescent. 6ta hora sumend.

18th.—Exceedingly restless, constantly tossing about, yet is stupid and rather delirious; constant vomiting of green, bilious mucus; tongue red and cracked; skin dry; leg more swelled, the integuments inflamed and hot; does not complain of more pain in the course of the vein than elsewhere; bowels opened; pulse 125, weak.

R Ammon. Subcarb. gr. xx. Infus. Serpentinae ℥iiss. in statu effervescentiæ c. succo limonis 6ta hora.

19th, A.M.—No sleep; delirious; constantly muttering; tongue dry, cracked, and brown; sickness not so constant; pain on pressure over the liver; leg not quite so much inflamed; thigh a little so, as is the opposite foot, which, he says, is painful; pulse 130, small, and soft. The tumor laid open to its whole length, and a piece of lint introduced; the surface of it hard, dry, and irregular.

Continue the poultice and cold wash.

R Liq. Ammon. Acet. ℥ss. Tinct. Hyos. ℥xxx. ex Aquæ Menthæ, 6ta hora. Mustard poultice to the pit of the stomach.

P.M. No better; delirious; pulse small and very rapid; tongue brown; sordes on teeth; excessively thirsty; sickness stopped.

R Hydr. Submuriatis gr. ij. Opii gr. ij. bora somni sumend. and some brandy, and soda water occasionally.

20th, A.M.—No sleep; constantly muttering; bowels opened; does not answer when spoken to; no sickness; pulse rapid and feeble; a little subsultus tendinum.

Continue brandy with the soda water.

* This is an instructive case, which we have had made up for some time, without finding room for it.

P.M. Fast sinking; subsultus increased; pulse scarcely perceptible; teeth covered with sordes; leg of much the same appearance; is quite insensible.

21st.—Died at 10 this morning: a few hours before death, there was perceived to be a little swelling, as though matter were forming immediately above the right wrist; if this were pressed upon, he showed some signs of feeling, and to this only was he sensible.

Post-mortem Appearances, 27 hours after death.—Body in a very putrid state: vena saphena inflamed from the tumor to within two inches of the place where it joins the femoral vein, at which part there was a natural coagulum; below this to the varix, the coats were much thickened, and containing a sanguino-purulent fluid adhering tenaciously to the vein. The tumor was a dilated part of the vein itself; the coats of it were exceedingly thickened and irregular. The communication between the tumor and veins in the lower part of the leg was effectually prevented by the obliterated small varix, mentioned as being felt attached to the larger one; and the vein above was so thickened and contracted as only to allow of a probe being passed from the vein above to the sac below.

Into one only of the veins of the leg had the inflammation apparently extended: this contained a small quantity of healthy pus; all the other branches were healthy, and contained coagula; vena cava, femoral, and iliac veins natural; heart large and empty; lungs gorged with blood; spleen larger than natural; kidneys soft and mottled.

In the right arm, directly above the wrist, the cellular membrane was observed to be inflamed, and some serum to have been thrown out: this, probably, had the patient lived a little longer, would have gone on to suppuration, deposits of pus being one of the characteristic signs of phlebitis. N.

METEOROLOGICAL JOURNAL,

Kept at EDMONTON, Latitude $51^{\circ} 37' 32''$ N.
Longitude $0^{\circ} 3' 51''$ W. of Greenwich.

Jan. 1832.	THERMONETER.	BAROMETER.
5	from 20 to 30	from 29.63 to 29.61
6	29 39	29.58 29.42
7	33 41	29.35 29.33
8	32 40	29.33 29.39
9	32 43	29.45 29.37
10	36 49	29.51 29.64
11	39 48	29.73 29.61

Wind variable, S.E. and S.W. prevailing.
Generally cloudy; rain on the 9th and two following days.
Rain fallen, .25 of an inch.

CHARLES HENRY ADAMS.

SUPPLEMENTARY NUMBER.

WE have received a considerable number of applications on the subject of our Supplementary Number last week, our correspondents complaining that it was not sent to them. On inquiry, we find that this arose from the circumstance of some of the newsmen and venders declining to furnish any one with the extra Number by whom it was not expressly ordered. Those gentlemen who may wish to have their sets of the Journal complete, ought to insist on the person by whom they are supplied procuring it immediately, else they may be disappointed, as the Number is sold separately, and cannot be retained for the regular subscribers. The Contents are inserted in the Wrapper of the present Number.

OFFICIAL REPORTS ON CHOLERA—PLAGIARY.

WE last week published a set of Official Reports made to the Central Board of Health on the subject of Cholera; the *Lancet* of this day has copied from our pages twelve of the cases without acknowledging the source whence they are derived—the least that an honest journal would have done in reprinting the papers of a cotemporary. What may be further worth noticing in this matter is, that we had made various little corrections in the reports, and in one instance accidentally omitted a sentence at the end of a case: the copy in the *Lancet* is identical, and the proof of literary theft is thus complete. Unluckily, however, for our cotemporary—not aware that some of the papers following those cases were also “official”—he has omitted by far the most valuable part of the whole.

NOTICES.

The paper of one of our correspondents being of a controversial nature, and requiring an Additional Sheet, is published at the expense of the Author, and thus the present Number given to our readers without any additional charge.

We have to apologize to several of our correspondents, whose communications we have been obliged to defer. Those relating to the Anatomy Bill we hope to have an opportunity of attending to next week.

We cannot enter into the discussion between Mr. Russel and Mr. Burnet about their hot-baths, except inserted *extra limites*, and at the expense of the parties.

ERRATA.

In Dr. Gregory's paper, at p. 501 of last Number, transfer the two top lines of the second column to the top of the first column, and in the Notice to Correspondents, for *hebdominal* journal, read *hebdomadal* journal.

W. WILSON, Printer, 57, Skinner-Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JANUARY 21, 1832.

LECTURES

ON

THE THEORY AND PRACTICE OF MEDICINE;

Delivered at the London University,

By DR. ELLIOTSON.

PART I.—LECTURE XVI.

Hæmorrhage—Fluxes—Dropsy—Inflammatory Dropsy—Appearance of the Urine—Albumen—Dropsy from Organic Disease—Treatment.

I TRUST, gentlemen, you understand the general view which I have endeavoured to present to you in regard to certain diseases. I commenced with the consideration of inflammation, as the most general of all diseases, as that which might affect almost every part of the body, and might be connected with almost every disease.

Having finished the consideration of inflammation, I was proceeding at the last lecture to the consideration of certain affections which frequently are joined to inflammation—take place during the course of it; and which, again, altogether resemble inflammation, so far as the symptoms are sometimes decidedly of an active inflammatory character, and sometimes entirely passive, without any inflammation at all; just, indeed, as is the case with inflammation itself; for we sometimes have passive inflammation—mere congestion, in which it is doubtful whether we ought to use the word inflammation or not, and the passive form of these diseases is not attended by even congestion or passive inflammation.

Hæmorrhage.

Now the first of these affections, and of which I spoke, is hæmorrhage. I mentioned that hæmorrhage might occur like inflammation, if not in all parts of the body, yet in a variety of parts. I mentioned that it was a very frequent occurrence during the course of inflammation. I mentioned

that, like inflammation, it is occasionally active—in fact, united with active inflammation; but that sometimes it will be entirely passive, without any inflammation at all, more congestion than any thing deserving the name of inflammation. I stated likewise, that between active and passive hæmorrhage, as between active and passive inflammation, there were various degrees—that, in fact, the two ran into each other by insensible degrees.

I mentioned that in the treatment of one kind of hæmorrhage, we had little more to do than to use the remedies for active inflammation—that is to say, to bleed, to starve, to keep the patient quiet, and to lessen the temperature, &c.—in fact, to do very little more than treat the inflammatory state, only that we may apply cold at the same time to the part, and even apply astringents, provided we adopt evacuations and all the remedies for inflammation. With respect to the passive form of hæmorrhage, I mentioned that we must not treat it in this way, but as we would passive inflammation, by local stimulants, by supporting the system, and above all, by the application of astringents and cold, and by the application of mechanical compression, and even in extreme cases, by burning the part, so as to produce complete constriction of the vessels.

We, must consider that in the two forms of hæmorrhage the state is totally different—that in active hæmorrhage, that which resembles active inflammation, the blood is, as it were, *forced out*; whereas, in passive inflammation, the blood is *let out*. In active inflammation there is an intense circulation going on in the part—that is to say, the blood *may* be moving more speedily than natural: I do not say it is, but there is a *large quantity* of blood *moving* through the part, the action of the heart is strong, the blood is forced out of the extreme vessels; we have, therefore only to moderate the force of the blood, and the hæmorrhage for the most part ceases. But in passive inflammation, the force with which the blood is im-

pelled to the part is not increased, there is no great impulse behind, the blood is not driven on, but the extremities of the vessels are so relaxed that the blood escapes—is let out. We have therefore to employ different means—not to stop the force of the blood, but to close the extremities of the vessels, to close the open vessels themselves as much as possible, and diminish their diameter, that the blood may not be thus *let out*. In active inflammation, all the astringents, all the stimulants, and all the compression you could employ, would do little or no good; for there is an active hæmorrhagic tendency, the blood is driven on with great force, and if you merely treat it as inflammation, the hæmorrhage, for the most part, ceases; whereas, in passive inflammation, if you were to annihilate the force of the blood from behind, by debilitating measures, you would only increase the mischief, you would only increase the relaxation of the vessels, and make them more liable to give way than before. In mixed cases it is right to employ both modes. You must always remember that a great number of cases are inflammatory, and that others depend entirely upon relaxation, so that mechanical means, astringents, cold, and fire, may be proper.

I mentioned in the last lecture the various parts in which hæmorrhage, independent of mechanical violence, takes place in different ages. I also mentioned the kind of hæmorrhage that we have from different parts. I have not, however, completed the subject.

It is right to remember that you may not only have profuse hæmorrhage from the mucous membrane of the bronchiæ, from the stomach, from the intestines, and again, from what is nearly a mucous membrane, the interior of the kidneys, and indeed of the urinary bladder, but other parts are sometimes the seat of hæmorrhage that proves fatal. I have known hæmorrhage take place into the pericardium, and prove almost instantly fatal; and I have read of modern cases occurring in one's own time of sudden and fatal hæmorrhage into the spinal sheath without any evident cause. In fatal cases of hæmorrhage into the pericardium, it is said that the heart and the pericardium were found soft. In the case that I saw this was a fact, and as far as I can ascertain, it was the case in other instances.

When hæmorrhage occurs from a mucous membrane, it is, I believe, for the most part from a large number of minute vessels. It is extraordinary what a profuse hæmorrhage you may have in a very short time—in a minute or two, so as to prove fatal—without any vessel whatever being discovered injured. I recollect distinctly having a patient with some pulmonary disease—I believe phthisis—who was sitting up in bed; suddenly blood came from his mouth, he fell back, and was dead. On opening him we

found the stomach filled with blood; there was an immense coagulum, exactly forming a mould of the stomach; but after examining every part of that organ with the most minute attention, I could not discover the orifice of any vessel whatever. The same has been observed in the case of the pericardium. I think you will find it mentioned in Dr. Baillie's work on Morbid Anatomy that cases have occurred in which he was unable to find a vessel which appeared to pour forth blood, both in mucous and serous membranes. The pericardium will thus suddenly allow the escape of an enormous quantity of blood.

Fluxes.

The next sort of affections, which are precisely similar in principle, and occur from those parts which are most frequently the source of hæmorrhage—namely, from the mucous membranes—are fluxes, catarrhs, or profluvia. These occur particularly from the nose, the bronchiæ, the intestines, the bladder, urethra, and vagina. Those parts which during inflammation will frequently pour forth blood, will also during inflammation of course pour forth a much increased secretion. Their secretion is increased, in the first instance; it afterwards declines, but does not come down to the standard of health; and then, as the inflammation subsides, the new secretion becomes excessively abundant. You have, therefore, inflammatory fluxes, catarrhs, or whatever name you choose to give them, from the mucous membrane, exactly as you have inflammatory hæmorrhages, and after all the signs of inflammation have subsided, you will have these discharges frequently continue.

You have, however, another form, where there is no inflammation to be discovered, where the mucous membrane will pour forth a vast quantity of liquid for a length of time, and that without any signs of inflammation being present. You have passive fluxes, exactly as you have passive hæmorrhages; the only difference is, that in the one case you have blood, and in the other you have a secretion.

I mentioned that the skin, which ranks almost with mucous membrane and belongs to the same family of structures, occasionally, but in very rare cases, such as I have never seen, will pour forth blood. The skin, however, will every day pour forth an immense quantity of its own secretion. The name flux, catarrh, or profluvia, would not be given to discharges from the skin; but just as the mucous membranes pour forth their discharges in excess, so frequently does the skin. Profuse sweating is analogous to profuse discharges from the mucous membranes. Now we have sufficient proof that excessive discharge from the skin is, more frequently than not, quite devoid of an inflam-

matory character—in fact, in the highest debility, in the debility of syncope, in the debility of death, the skin will secrete most abundantly. Just so it undoubtedly is with respect to the mucous membranes: they will secrete most abundantly, without any signs of inflammation at all. It has been imagined that whenever a mucous membrane secretes in excess, it must be in a state of inflammation. But I think the instance of the skin, which is alluded to by Andral, (and it is a happy thought of his, namely, that the skin will secrete in the greatest abundance, will sweat profusely, without any marks of inflammation) is sufficient to show that the mucous membranes may be in a similar state without any inflammation either. This instance of the skin would enable us to say, *à priori*, that this is possible. But I think, *à posteriori*, we can assert that mucous membranes will secrete most abundantly without inflammation, for after fluxes they are often quite pale after death, just as often after passive hæmorrhage. It appears to me to be analogous to the case of inflammation itself, and also to hæmorrhage. I think you may have an active secretion from the mucous membranes and from the skin, as in acute rheumatism, and you may have it entirely passive without any mark of inflammation.

Treatment.—The treatment of these fluxes is precisely the same as the treatment of inflammation or of hæmorrhage. If they be of an active kind, attended with strength of pulse, with pyrexia, heat of body, with pain of the mucous membrane itself, the pain, which is characteristic of inflammation of a mucous membrane, and increased by pressure, astringents are altogether improper; at any rate they are altogether useless, just as they are in active hæmorrhage. It would be just as absurd to attempt to cure these discharges by astringents—to stop the discharge of gonorrhœa, for example, by an astringent injection in the violence of the inflammation—as it would be to attempt to stop an active hæmorrhage by such means. You have only to lessen the inflammatory state by bleeding, and the discharge will become diminished or put into a course of diminution; perhaps there will be a momentary increase, but it is followed by a diminution exactly as is the result in an active hæmorrhagic process. When there is no inflammation present, or the inflammation is subsiding, you may apply local means, such as stimulants and astringents, with the greatest advantage; in fact, then they become the proper remedy. If it so happen that no inflammation occur from the very beginning, you may at once apply stimulants and astringents. The instance of the urethra, which is almost before one's eyes, is a good illustration of all that occurs in the various mucous membranes of

the body. You will have, in inflammation, an increased secretion, and the inflammation and secretion are only to be subdued by anti-inflammatory measures; but you will have at last only a passive discharge, and then anti-inflammatory measures will only increase the mischief, and stimulants are to be resorted to; and sometimes you will have so little inflammation, that you may disregard it from the very first. It is just the same in diarrhœa. In diarrhœa, frequently the best remedy is to bleed the patient, to starve him, to leech the abdomen, to blister him; to give no astringents whatever, but simply to do this, or moderately to purge him, in order to increase the effect of the treatment, and then the inflammatory state will subside. In other cases you have to give astringents and opiates, and support the patient well, and in that way he recovers. It is the same, too, in bronchitis. In active bronchitis, any remedies which stimulate the air passages would be highly injurious: you have only to bleed the patient well and starve him, and treat him as labouring under inflammation, and the discharge of mucus gradually subsides; whereas if you have, in an old man, for example, a profuse discharge from the bronchiæ, forming what is called *catarrhus senilis*, without inflammation, but accompanied with emaciation and paleness, you must give tonics and good nourishment: such stimulants as snake-root answer the best purpose.

Dropsy.

There is another set of affections of other structures of the body which are in all respects decidedly analogous to fluxes and hæmorrhages, and these are called *dropsies*. A dropsy is not an affection of the mucous, but of the serous and cellular membranes. The fluid here is secreted in such excess, that it is not absorbed proportionately; and as the serous and cellular membranes are closed parts, of course the fluid does not escape as it does from the mucous membranes; so that instead of a discharge you have a dropsy. That is the sole difference between these two affections; in the one case the effusion escapes, and in the other it does not. These collections take place in the arachnoid, the pleura, the pericardium, and in the peritoneum, &c. together with the general cellular membrane of the body, and are continually of an active inflammatory kind, and to be treated not by diuretics, but simply by the remedies of inflammation.

In the arachnoid, the pleura, and the pericardium, we every day see dropsy with the most decided inflammation, both acute and chronic. Sometimes, though not so frequently as in the cases just mentioned, we see dropsy in violent inflammation of the abdomen, but very often ascites itself is a de-

cidedly inflammatory disease. We have in such cases the usual signs of inflammation of a serous membrane. When the affection is situated in the arachnoid, we have pain of the head, intolerance of light, perhaps squinting, and great quickness of pulse. In inflammatory dropsy of the pleura, we have, of course, pain in the side, dyspnoea, and cough. With respect to the pericardium, we have pain in the region of the heart, perhaps a rapid pulse, palpitation, and the pain in the heart darting from that organ in various directions. In the case of the abdomen, the pain is considerable, and is increased on pressure. In fact these are all so many *itises*—*arachnitis*, *plenitis*, and *pericarditis*, &c. All these affections tend to induce more or less effusion in every case, but occasionally the effusion is considerable, and the word *dropsy* is applied to the collection.

The same occurrence takes place in the cellular membrane, and when it is in a state of dropsy, that is to say, when it contains a preternatural quantity of fluid, we have a white, soft, and inelastic swelling of the surface, which swelling pits upon pressure. The swelling may sometimes be red, because the skin may become inflamed, as well as the cellular membrane beneath. It sometimes is not soft, but intensely hard, from the high degree of inflammation of the cellular membrane, and the fibrinous nature of the effusion. The tumor, of course, is the largest where gravity directs the fluid in the greatest quantity, so that any part which is dependent becomes much more swollen in consequence, and you may regulate the swelling by regulating the posture of the part. What is curious, the swelling will sometimes shift independently of any particular position. Where a person is lying straight in bed, you will one day see one arm swollen, and the next day the other. Sometimes it will shift to the leg, and then return to the arm again. It is a remarkable circumstance, but one that does occur continually, without any connexion with posture.

Inflammatory Dropsy.

When this dropsy of the cellular membrane is inflammatory, it generally begins in the face as soon as any where else; and I believe the reason is, because it generally arises from cold, and the cold is as frequently applied to the face as any where else. Usually, in these collections, wherever they are seated, the urine is scanty; but, when the cellular membrane is the seat of the affection, and the affection is of an inflammatory nature, it is very common to see the urine of its natural quantity; nay, very frequently I have seen it above what it should be. This inflammatory dropsy of the cellular membrane more frequently arises from cold and wet, than from

any other cause; whereas, the inflammatory dropsy of the arachnoid, the pleura, the pericardium, and the peritoneum, may or may not arise from that source. I hardly recollect a case of dropsy of the cellular membrane of an inflammatory kind which did not arise from cold and wet; and with it you will usually see there is inflammation of some internal part, or, if not inflammation, an inflammatory state. If you inquire into the state of the head, you will commonly find that the patient says he is heavy; perhaps he has violent pain, but generally it is heavy. If you press the abdomen, you will frequently find tenderness; but, above all, you will find an inflammatory state of the chest. The patient either complains of pain in the chest or of cough; or if you listen to the chest with the naked ear, or employ the stethoscope, you will find either the sonorous or sibilous rattles of the bronchiæ, or you will find the crepitous rattle of inflammation of the air-cells. In most cases of this inflammatory dropsy you find inflammation within the chest.

The pulse, in dropsy of the cellular membrane, is frequently hard and full, and it is from the occurrence of the local symptoms of inflammation in the head, chest, or abdomen, from the strength of the pulse, from the suddenness of the occurrence of the dropsy, together with the nature of the causes which have produced it—such as produce inflammation, that you judge that the disease is of an inflammatory kind; for, although the affection is so decidedly inflammatory, you will not find the cellular membrane tender; it certainly is not painful; patients, in general, do not complain of pain when you press the surface of the body, and yet the inflammatory nature of the disease is sufficiently shewn by the relief experienced on free bleeding, by the buffed and perhaps cupped state of the blood, and by the nature of the causes which produced the disease. The local inflammation that exists within the head, lungs, or abdomen, may generally account for the buffy state of the blood, and the relief that is experienced by bleeding; but when you find the dropsy itself cease so rapidly as it does after bleeding, it is impossible to refuse belief that the dropsy itself was of an inflammatory kind.

Appearance of the Urine—Albumen.—In some of these cases, the urine is of an albuminous character, that is to say, it contains serum, and we have been told that our guide for bleeding in dropsy—that our guide in forming a judgment as to whether it is inflammatory or not—is to be taken from the appearance of the urine. You will find it stated in a work published by Dr. Blackall, that the quantity and firmness of the coagulum of the albumen of the urine is usually proportionate to the existence of inflamma-

tion, and to the necessity for carrying into practice anti-inflammatory means. The albuminous state of the urine is shewn by heating it; by putting some in a spoon, and holding it over a candle till the temperature arrives at the point at which it coagulates; or by adding vinegar to it, and then prussiate of potass, when the albumen will *after a time* be precipitated. The former mode will do very well, but the latter is the nicest way: it is necessary to add the vinegar first, or you will not get the precipitate. Dr. Blackall says, that the firmness of the coagulum is usually proportionate to the marks of inflammation; but he says, that, notwithstanding that, there may be albumen in an opposite state—a state of debility, and in that case bark will cure the disease. His words, however, are,—“A correct guide to venesection may be found in the firmness, copiousness, and early appearance of an albuminous coagulum in the urine; its limits in the improvement of the discharge (that is to say, in the diminution of the coagulum) of albumen in the urine, in the state of the blood, and the relief of the other symptoms.”

Now Dr. Blackall has the great merit of pointing out an albuminous state of the urine in many cases of dropsy, and shewing that it frequently attends an inflammatory condition; but I am quite satisfied that if you take the state of the urine for your guide, you will practise very badly. It is to be remembered that it is a long time now since Dr. Blackall published his work, and it would be very strange if the practitioners who came after him had not improved our knowledge upon this subject. It is no reflection on any man who writes a work, that some of his opinions are proved to be wrong by those who succeed him. We go on adding experience to experience, and if any man who lived a hundred years ago, had luckily, or unluckily, lived to the present time, and had gone on making observations, he would correct much that he advanced at the beginning of his life. There is therefore no discredit in a man being proved wrong in certain opinions he has advanced, provided a number of things he has asserted are true. I have certainly found it very common for persons to be cured of dropsy by bleeding, although no albumen could be discovered in the urine; while, on the other hand, where there has been albumen in the urine, persons have not been cured of dropsy by bleeding. I have, again, seen abundance of albumen in the urine, where there was such debility of frame as to forbid bleeding altogether. I should therefore advise you to determine the question in every case of dropsy, whether it is to be treated by bleeding or not, simply by observing whether there are inflammatory symptoms or not—not to consider the state of the urine in the least, but to consider whether the symptoms are those of inflammation or

not. If you find in any case of dropsy a full, strong, and quick pulse, with strength of body, you are then to presume that the case is of an inflammatory nature. If you find there is local pain, and other symptoms of local inflammation any where, whether they be in the head, chest, or abdomen, that is quite sufficient to determine the inflammatory nature of the case. Again, if you cannot learn much from these particulars, if the pulse do not shew any phlogistic diathesis, and if the head, chest, and abdomen, are particularly free from inflammation, yet still if the disease have come on suddenly, as inflammation often does—is not the work of slow disease, like visceral affection, but come on suddenly, and come on from the common cause of inflammation, the application of cold and wet, especially when the body is over-heated—then the presumption is that the case is inflammatory; and you will not do wrong if you bleed, so far as the pulse will justify you, because the pulse will often *justify* this treatment though it will *not point it out*. You frequently must have recourse to the pulse, not to learn what to do, but to learn whether it will sanction you in doing what other symptoms indicate to be a proper procedure. Whenever a dropsy comes on suddenly from the application of cold and wet, and the pulse will justify me, I certainly treat it as an inflammatory complaint. When the pulse is full, accompanied by a phlogistic diathesis, and signs of local inflammation, there can be no doubt about the treatment of the case.

In regard to the albuminous state of the urine, we are indebted to Dr. Bright for the fact, that in organic disease of the kidney, the urine is generally in this albuminous state—that is to say, contains serum. Andral, in his Clinical Reports, previously mentioned a case where he found the urine albuminous, and the kidney in a granulated state. He simply mentioned the fact; he had no more facts, and he came to no general conclusion, nor would he have been justified in doing so. But Dr. Bright has collected a large number of cases, and he has found that when the kidney is in a disorganized state, the urine is generally albuminous. He does not say, so far as I can understand his book, that when the urine is albuminous the kidney *must* be in a state of organic disease—for he says that sometimes he has seen it only gorged with blood—but still the kidney was affected. Some have gone further than this, and I think without any reason whatever; they would have us believe that nobody can have albuminous urine without organic disease of the kidney. Now really I cannot subscribe to this assertion, and for this reason: I have seen patients who have been perfectly well a day or two before, but have got wet through; symptoms of inflammatory dropsy have come on; the urine has become

albuminous; but, on bleeding them, the dropsy has presently been got the better of, and the urine has recovered its healthy appearance instantly. Why these poor people should be supposed to go skipping on with diseased kidneys, merely because they had albuminous urine for a week, I cannot imagine; it is a mere assumption, I think, that these people must have diseased kidneys. I could not open them to ascertain whether their kidneys were diseased; but when they are in perfect health now, and had been in perfect health just before, and the urine is no longer albuminous, I do not believe there is any more foundation for supposing the existence of organic disease, than there is of supposing cancer of the stomach in every case of temporary dyspepsia, because when people die of dyspepsia we find more or less organic disease. It is the business of those who make these assertions to prove their correctness—to prove that these persons have organic disease of the kidney, and not our business to disprove it; we have nothing to do with it. It does not follow that, because when a person dies making albuminous urine you always find structural disease of the kidney—that when the urine temporarily presents the same phenomenon, and the person recovers, he has had any thing more than a functional complaint. Because the affection of the kidneys may arrive at such a degree of intensity as to destroy life, and you then always find organic disease, it does not follow that the temporary formation of albumen in the urine should be any thing more than a functional disturbance of the kidneys. I should argue just the reverse, and suppose that it must be functional, if the symptoms were temporary. Dr. Mackintosh, of Edinburgh, informed me the other day, that some medical students in that town had lately ascertained that when they ate pie-crust, and it produced dyspepsia, their urine became albuminous; they made this experiment over and over again, and the circumstance is nothing more than I should expect.

Dropsy from Organic Disease.—It frequently happens, however, that dropsy is not inflammatory. You have patients exceedingly pale, with a pulse exceedingly weak, with extreme debility of the whole frame; and the more you weaken them the worse it is—the more does the dropsy increase. Dropsy is frequently an accompaniment of organic disease in various parts. In disease of the liver, it is very common for the peritoneum to become the subject of dropsy; in disease of the womb, it is very common for the lower extremities to become œdematous, and at last it spreads through the whole body, so that you have what is called anasarca. Œdema is a local dropsy of the cellular membrane, anasarca a general dropsy of it. When we see no signs of inflammation, no fulness of pulse—there may

be quickness and not fulness—but on the other hand debility; and when you find the powers of the patient becoming exceedingly reduced, the disease must be put upon a footing with passive inflammation, or on a footing with passive hæmorrhage, or passive discharge—gleet from a mucous membrane; and every thing which debilitates the body will make the matter worse: *a fortiori*, this must be the case if there be any visceral disease, as disease of the liver—a liver full of tubercles, or indurated, or scirrhus, or carcinoma of the womb, or ulceration of the lungs. Dropsy, too, will sometimes be like hæmorrhage, an entirely mechanical business—not dependent on inflammation on the one hand, or debility on the other, but upon mere mechanical obstruction. Whatever compresses the large veins will produce dropsy; the vessels become so turgid, that, to ease themselves, they let forth the serous portion of the blood; and if that be not sufficient, then the blood itself escapes.

Treatment.—In the treatment of inflammatory dropsy we have above all to bleed, to purge freely, and to adopt low diet, and sometimes to give mercury. In this form of the complaint we need not trouble ourselves about diuretics any more than about astringents in active hæmorrhage from a mucous membrane. If you treat the inflammation you will generally find the collection of water gradually cease; however, sometimes it does not; and then it is well, when you give purgatives, to exhibit those which are called hydragogue, which cause very watery stools. That in general will answer every purpose. Purgatives are exceedingly beneficial in cases of inflammatory dropsy, for they not only act as anti-inflammatory remedies, exactly like bleeding, in diminishing the bulk of the fluids, by causing a rapid secretion of fluid, and thus act upon the cause of the disease, by tending to subdue the inflammation; but they act as palliatives, by causing the absorption of what is effused. It would be in vain to give diuretics in inflammatory dropsy, for they would not act in the inflammatory state of the system. Diuretics will not then act upon the kidneys; and the more you stimulate the kidneys the less urine is secreted. But when you are giving hydragogue purgatives you are doing the same as bleeding, and you hasten the absorption of what has been collected, at the same time; and in proportion as the inflammation is lessened, so do the hydragogue purgatives exercise more and more power over the kidneys. Among the best is a mixture of jalap and cream of tartar, and, on the other hand, claterium.

If the dropsy be of another description, such as I mentioned as being without any marks of inflammation, starving and bleeding will increase the collection ten-fold.

You have there to give wine (perhaps good nourishment), steel, sulphate of quinine, and various diuretics. In this case the disease hangs on the patient so long, and such great inconvenience is felt from the collection of fluid, that, although diuretics will not cure the complaint, yet they are necessary for the purpose of lessening the quantity of fluid which is collected in the body. Diuretics cannot go to the cause of the dropsy; they only tend to lessen the effect of the dropsy. Among the best are digitalis, calomel, squills, and acetate of potassa; and you will find it very useful to give these together. When you cannot increase the dose of digitalis, or squills, you may of the potassa; and so, by giving a great number of remedies, you do better than by trusting to one. We may give them to a full amount by combining several together. The hydriodate of potassa, and all the salts of this alkali, are strongly diuretic.

The effect of diuretics is very much increased by the exhibition of mercury. In inflammatory dropsy, mercury is highly useful as a remedy against inflammation; but, in the opposite kind, it frequently would do harm by increasing the debility, especially if there were much organic disease; but given in great moderation, so as to produce a slight affection of the mouth, it will enable the other diuretics to act much more powerfully on the kidneys. Though we sometimes succeed in curing dropsy by putting a patient under a profuse pyalism, when all other things have failed, yet generally that is not the case unless the dropsy is in some degree inflammatory, or there is inflammation of some organ; but, in all cases, the moderate administration of mercury certainly makes all other diuretics answer ten times better.

When the chest is oppressed, and we find the patient in danger of being suffocated, or the abdomen is distended, and the patient suffers great pain; and when the cellular membrane is distended, so that a patient cannot move about, and we cannot wait for the cure of the dropsy itself or its cause, be it debility or obstruction, or any thing else, it would be right to have recourse to means calculated to get rid of the fluid as soon as possible. Under these circumstances, violent purgatives, as elaterium, in full doses, answer better than diuretics, and for this reason—they discharge more, and frequently go to the cause of the dropsy, and remove some obstruction, some congestion, or some inflammation. But, of course, diuretics may be given with the purgatives, to increase the effect.

It is found to be useful to give diluents at the same time, for diuretics are frequently so very acrid that they irritate the kidneys much more than is necessary for secretion; and their operation is much facilitated by

the patient drinking largely of diluents. The same is observed with respect to purgatives; the purgatives seem to operate a great deal better if plenty of diluents be taken at the same time.

However, we sometimes find that patients are so oppressed with the fluid that they cannot wait any longer; we find that our remedies do not remove it, and then we are obliged to have recourse to certain mechanical means for accomplishing this object. These are tapping of the chest, tapping of the abdomen, and, in cases of anasarca, making minute punctures in the skin with a needle. Some persons scarify; but I am quite sure that every purpose may be answered with a fine needle. By making minute punctures in the skin, an immense quantity of water may be drawn away. There is no occasion to introduce the needle deep; you have simply to put it through the skin, by rotating it between the finger and thumb, and when you withdraw it you will see a bead of clear serum appear, which for a time will become larger and larger; it then trickles, and the oozing perhaps will continue for some time: I have known it continue for days, even after death. Twenty or thirty punctures may be made. I never saw harm arise from this when it was done above the knee. But although these punctures made with a needle are so minute, and the aperture is merely made through the skin, I know that patients have lost their lives through them; I know that gangrene has taken place through such a slight operation; but in every case that has come to my knowledge, of this description, the apertures had been made below the knee. Now it is an established rule in scarifying, to make the scarifications as high as possible towards the trunk of the body—near to the most powerful parts of the body; and the same rule should be observed with respect to acupunctures. I have used them with great success all over the trunk and upon the thighs, as low as the knees; and I have used them with equal success in *all* parts of the upper extremities, and I never met with any inconvenience. Before I was aware of the danger, I frequently used them below the knee, and never once did any accident occur to me which was at all dangerous. Gentlemen, however, have related to me cases which came to their own knowledge, where the operation was dangerous below the knee—where it was followed by sloughing of the skin, sloughing, indeed, of many muscles—so that one patient lost the gastrocnemii, and at the end of two or three weeks lost his life. That is what no one would have supposed, but it certainly is the case.

These mechanical means are now and then necessary, but not so often as might be imagined, if we treat the disease accordingly as it is inflammatory or not. If we care-

fully distinguish whether there is inflammation or not, we shall cure the complaint in a large number of cases. I have frequently, at St. Thomas's Hospital, when I have admitted a patient with the word *dropsy* on his letter, been asked "how I could admit such a case as that," because it was well known that dropsy could not be cured, and because Doctor Fordyce never admitted such cases? Certainly one of the greatest improvements in medicine is that of distinguishing inflammatory diseases from those which are not so, and the establishment of the fact that a great number of diseases are nothing more than so many inflammations. One half of the cases of dropsy which I see are of an inflammatory nature, and yield to bleeding and the other common remedies of inflammation.

CLINICAL OBSERVATIONS

Delivered in the General Dispensary,

By MR. COULSON,

Jan. 6th, 1831.

GENTLEMEN,—The cases to which I shall draw your attention to-day, are those of excoriations on the penis, stricture in the anterior part of the urethra, and encysted abdominal dropsy.

Excoriations of the Penis, or Gonorrhœa externa.

Richard Bainbridge, aged 19, was admitted last Monday for an affection of the penis. There was a copious yellow discharge from the corona glandis and the internal surface of the prepuce, with extensive red excoriated spots; there was great pain, particularly on drawing back the prepuce and at night, with a great sense of heat in the part. He was ordered to use the decoction of tormentilla root as a wash to the excoriations, and to take the aperient mixture occasionally. These cases commence with a sense of itching and redness in the glans, or internal surface of the prepuce; the skin is next destroyed, and a purulent discharge ensues, from which circumstance the older writers gave to the complaint the term of *gonorrhœa externa*. The abraded surfaces in favourable cases present a red appearance, with minute pointed granulations; and in those which are of a more unfavourable character, a dirty yellow appearance. The complaint frequently arises from inattention to cleanliness: the natural secretion from the glands of the part is allowed to accumulate, and this accumulated secretion acts as an irritant, especially in warm wea-

ther, and in a disordered state of the health. Very extensive excoriations sometimes occur after connexion, which renders it difficult (if you do not see the patient early) to distinguish between this complaint and venereal ulceration. If no remedial measures be employed, the discharge, when it arises from healthy excoriations, usually stops, and crops of warts grow from the excoriated surfaces, which frequently prove very troublesome. If, on the contrary, the excoriations are of a yellow unhealthy appearance, sloughing of the glans and penis occurs, together with swelling of the glands in the groin and great constitutional disturbance. The local applications which are employed in this complaint, are lotions of sulphate of zinc, in the proportion of six or eight grains to the ounce of water; weak solutions of acetate of lead, black wash, and lotions of nitrate of silver and sulphate of copper, containing ten or twelve grains to the ounce. These last applications I only employ when the ulcerated surfaces are in a very indolent state. But the best local application in this affection, at least when the surfaces do not present a very unhealthy character, is the decoction of tormentilla root as a lotion, in the proportion of an ounce of the root to twelve of water, boiled down to eight. I am indebted for a knowledge of this application to my friend, Mr. Alcock, who mentioned it to me in 1823. I have given it an extensive trial since that time, and found it a very effectual and speedy remedy for this complaint. Prior to the application of this, or indeed any other lotion, the parts should be cleansed with some luke-warm water; and if you are employing the tormentilla lotion, caution your patient not to let any of it go on the linen, as the stains do not readily come out. If the prepuce cannot be pulled back without much pain, let the warm water and the lotion be injected between the prepuce and the glands with a syringe.

The tormentilla lotion, and a little aperient medicine, will in general be all that is required for the cure of the excoriations of the healthy character; but those which present a yellow appearance frequently become indolent, and do not yield to the local applications alone, or at least not for some time. For the more speedy cure of these, I generally give small doses of blue pill till the sores have healed.

Stricture.

Richard Harrison, æt. 24, was admitted last Monday for difficulty in passing his urine. Says that two months ago he contracted a gonorrhœa, and that the difficulty in passing his water has only occurred since the discharge stopped, which is about a fortnight since. He used several injections to stop the discharge, and among them one with

sulphate of copper. The water is voided at present in a very small stream; and sometimes the obstruction is so great, that the urine comes away drop by drop; occasionally it passes off involuntarily. There are painful erections at night, and frequent shiverings during the day. On examining the urethra with a bougie, a firm contraction is felt about an inch and a half from the external orifice; and this part when pressed externally feels hard, and a little swollen and painful.

A small wax bougie to be introduced every other, or third day, and a mixture given with Infus. Quassie, ʒvj. Aquæ, ʒij., et Tr. Ferri Murialis, ʒiiss., cujus capt. coch. dua ampla bis die.

In the majority of cases of stricture that come under our notice, the obstruction is situated at the anterior part of the membranous portion of the urethra; but occasionally, as in this case, the stricture is situated a short distance from the external orifice (from an inch and a half to three inches and a half, and at the external orifice itself.) These cases are by far the most difficult to treat, and sometimes resist all the means which we employ for their relief; fortunately, however, they are of rare occurrence. The rapidity with which the disease has advanced in this case, is striking; in less than three weeks the smallest size bougie can scarcely be introduced. Here are two specimens (Mr. Coulson then exhibited them) of stricture of this portion of the urethra; in one the anterior portion of the urethra, to the extent of two inches, is completely obliterated, and the urine was voided by a fistulous opening below this part; and in the other the urethra is so much thickened, that I could not during life, without much pain and difficulty, introduce a fine catgut bougie. Lymph was rapidly thrown out between the erectile tissue and the mucous membrane, and the disorganization of the urethra thus produced. The origin of stricture is often attributed (and in most cases I think erroneously) to the use of injections; but in a case like the present, the rapidity with which the stricture has occurred, and its seat, would rather lead me to think it had been produced by this means. I ordered for this patient the Tinct. ferri murialis, and he is already much benefitted by it. The use of the bougie is to be persevered in, but I fear some time will elapse before the cure can be effected.

If the stricture should not give way to the use of the common bougie, I shall, when the patient's health is improved, try those armed with nitrate of silver. I frequently employ them in this institution, and have never seen any mischief ensue from their use. The armed bougie is especially adapted to cases where the deposition which produces the

obstruction is large, and does not readily yield to the pressure of a common bougie. I have been very particular in my inquiries of patients labouring under bad strictures of their effect on the powers of sexual intercourse, and I have not found the interruption to the passage of the semen, so great as Sir Everard Home describes. One case, in particular, struck me very forcibly: it was that of a person from the west of England, whose stricture was so bad as not to admit a catgut bougie. He was a dissolute fellow, and though with so serious a complaint, he ought to have abstained from connexion, I knew he did not; and he, on inquiry, assured me, if his testimony is to be relied on, that his stricture made little or no difference in the emission of the semen. Sir E. Home, in his excellent work on stricture, says, "that in some instances, and those not a few, the contraction is so great as altogether to stop the emission of semen, and force it back into the bladder; in others it (the semen) passes through the stricture after the orgasm has taken place, but with little or no force." A curious circumstance connected with this subject occurred to me about a year and a half ago. A man who had been under my care with a very bad stricture, either had an order of affiliation made against him, or expected to have one very soon made. The man, as soon as he knew it, came to the Dispensary, and anxiously inquired of one of Mr. Pereira's apprentices, if it were not possible for him to get off, by setting up as a plea the bad stricture which he had laboured under. The young gentleman having heard me mention the subject in a lecture on stricture, referred the patient to me; and I told him that I should be but a bad witness for him, as I thought a man with a severe stricture might still be the father of a fine child. The poor fellow made no observation, but went away evidently much disappointed.

Encysted Abdominal Dropsy.

Sarah Davis, aged 26, had laboured under abdominal dropsy for the last sixteen years, during which period she had been tapped thirteen times; on the last occasion (10th August, 1831) by myself, when above eleven quarts of fluid were drawn off. After the last operation she recovered more slowly than on the former occasions; the menses, which stopped when the abdomen began to be large, had always returned after the operation, excepting on the last occasion. Her mother told me (for I had not seen her for some time) that she had lately complained of great pain about the chest and abdomen, difficulty of breathing, cough, with mucopurulent expectoration, and swelling of the legs. She died last Monday, and on the following day I opened the body.

On laying open the abdomen, I found a large encysted cavity, occupying the whole of the belly, and adherent at every part anteriorly and laterally to the muscles, and posteriorly to the liver, stomach, intestines, and fundus of the uterus. On the anterior part of the cyst I could not make out the peritoneum or transversalis fascia; the cyst was very thick, and could be divided into two or three layers, very much resembling the fibrous tunic of the artery of a large animal; posteriorly it was equally thick, and no trace of the omentum could be seen, for it was quite involved in the cyst. Here is a portion of the cyst, which you can examine for yourselves; it is very thick, and easily separable into two or more layers. The liver was enlarged, and all the other viscera were more red than natural, but not changed in structure. The fallopian tubes and ovaries were healthy, but very small. The pleuræ costalis and pulmonaris were so strongly adherent to each other that I could scarcely separate them. The lungs were gorged with blood at their posterior portion, and when cut into a frothy mucus exuded. The pericardium adhered to the heart at every part excepting at its apex; and on the surface of this organ, between it and its serous covering, there were bony patches, some of the largest and thickest which I ever saw on the heart. I have examined the bodies of several persons who have died of dropsy, but this is the first case of its kind which has come under my notice; it is what is called encysted abdominal dropsy, the water not being contained in the peritoneal cavity, but in a distinct cyst, and the viscera placed to one side, or covered by the posterior wall of the cyst. It is said, that, in encysted abdominal dropsy, the swelling does not commence in the inferior part of the abdomen, but this, of course, will depend on the size of the cyst; for the swelling will first appear at the most depending portion of the cyst. M. Piorry says that the sound which is given on percussion is more dull in this form of dropsy than in ascites; the fluctuation also is less evident in encysted abdominal dropsies than in ascites. I fear that the diagnostic marks, however, are too obscure, in a large encysted abdominal dropsy, to enable us to distinguish between it and ascites. Portal, in his work on dropsies, says that this form of ascites is extremely difficult to recognize, but does not think it a very rare occurrence. Cruveilhier, in his *Pathological Anatomy*, (vol. i. p. 268, Paris, 1816,) refers to some cases of this kind.

ON THE TREATMENT
OF THE
BRONCHIAL AFFECTIONS OF
CHILDREN.

By MARSHALL HALL, M.D. F.R.S.E. &c.

To the Editor of the London Medical Gazette.

SIR,

I HAVE seen so many deplorable results from the undue employment of blood-letting, and so many happy effects from the use of ipecacuanha in emetic doses, in the bronchial affections of children, that I think it highly important to bring these two subjects before the profession.

According to my experience, blood-letting is ill borne in the bronchial affections of infants. The little patients soon become pale, with glossy cheeks, half-closed eyelids, slight torpor, and a very frequent pulse.

With these symptoms, the bronchial affection is frequently little or not at all improved. There is an undiminished tracheal and diffused bronchial rattle, and frequently a husky voice and croupy cough.

Such cases have been particularly frequent recently. One of the most interesting which I have witnessed, occurred in the little infant of Mr. Wright, surgeon, Howland-street, four months old. One leech only had been applied, but it had blanched the countenance, and induced the peculiar waxen hue of the cheeks, so significant of exhaustion. There was slight torpor, and the eyes were only partially closed. There was unrelieved diffused bronchial rattle, and an alarming croupy cough. Further depletion was inadmissible. The case was treated by five-grain doses of ipecacuanha, in the manner about to be described, with the most marked immediate benefit and the happiest result.

In another case, of an infant six months old, in which four leeches had been applied, the pulse had risen to 190! A truce from exhausting measures permitted the pulse to subside. The infant recovered without the further detraction of blood.

My object, however, is not to detail cases, but to give the general result of my experience. This is, that bronchitis in children neither bears nor requires much bloodletting; and that such

bloodletting is not *the* remedy for bronchitis.

On the other hand, the good effects of repeated emetic doses of ipecacuanha are so striking and so immediate as to leave no doubt of the efficacy of this remedy in this particular case.

In the first place, the mere effect of vomiting, which is first an effort of expiration, and finally full expiration itself, relieves the upper parts of the bronchial tubes of the accumulated mucus.

In the next place, the effect of nausea in subduing the powers of the circulation, and especially of the capillary circulation, is quite remarkable, whilst this effect, being unattended by exhaustion, is totally free from danger.

It is probable, too, that ipecacuanha produces an effect upon the bronchial membrane similar to that observed on the skin. The secretion of mucus succeeds to a state of dryness, and the mucus itself is of less tenacity and easier of expectoration.

However, the rationale apart, I may give the result of my experience in general terms, and say that of all our remedies for bronchitis in children, repeated emetic doses of ipecacuanha, after moderate bloodletting, is the most efficacious. In little infants, five grains may be given and repeated every three, four, or six hours. The dose must generally be augmented to ten or fifteen grains, especially in older children.

To the ipecacuanha I have always added castor oil, in the intervals of sickness.

Blisters and fomentations to the chest are also highly advantageous.

But the principal object of my present communication is to point out the danger and inefficacy of bloodletting, and the extreme value of ipecacuanha, in this particular disease of children. Bloodletting, to be safe, must be administered cautiously. The ipecacuanha, to be efficacious, must be administered in fuller and more repeated doses than those generally prescribed. I have repeatedly known a single emetic dose of ipecacuanha subdue a recent bronchitis; and I have known repeated doses subdue the most inveterate. These repeated doses are not attended by the slightest risk. Let the nausea and the vomiting subside, and the disease will frequently be found to be perfectly removed, whilst the powers of the system are unimpaired.

Few remedies of such powers are so free from danger, in the feeble and tender age of infancy. If the disease be obstinate, the remedy may be repeated until it *does* prove efficacious.

14, Manchester-Square,
Dec. 23, 1831.

A CASE

OF

SCALDING OF THE THROAT, IN A CHILD, FROM DRINKING BOILING WATER,

Cured by opening the Jugular Vein.

By MR. G. O. HEMING,

Member of the Royal College of Surgeons.

MR. EARLE'S interesting lecture, in the Medical Gazette for December 10th, has brought to my mind a case of the croupy affection, induced in children by drinking hot water, in which the use of blood-letting was distinctly and eminently advantageous. It may, therefore, form a useful addition to the cases placed on record by Dr. Marshall Hall, Mr. Stanley, and Mr. Earle.

John Noyes, aged two years and a quarter, was brought to me, about five months ago, by his father, who stated, that, seven hours previously, he had drunk some hot water from a tea-kettle.

The little boy was breathing with much difficulty, and the sound produced by respiration was similar to that which takes place in croup. There was an incapability of swallowing, and he seemed to suffer great pain.

I bled him from the jugular vein, to a state approaching syncope. The breathing instantly became considerably relieved. A cold lotion was recommended to be kept constantly applied to the throat, and, as he could not swallow, an injection was given to act upon the bowels.

Four or five hours afterwards I went to see him, accompanied by Dr. M. Hall (whose paper on the subject I had read some years before, in the Medico-Chirurgical Transactions), with the expectation that the child might require the operation of tracheotomy; but we were agreeably surprised to find that, since the bleeding, the respiration had progressively improved, and the peculiar noise had entirely subsided. The

little patient was quite well in a few days.

I would here just remark, that I have seen many cases of croup—one within the last few days—where leeches had been numerous and repeatedly applied without affording any relief; but the good effect of taking blood from the jugular vein to approaching syncope was evident immediately, and apparently saved the lives of the little patients. From the case related by Mr. Earle, it is quite evident how serviceable the loss of blood may be in such cases; as even the application of the leeches arrested the disease and afforded temporary benefit. In croup, the loss of blood is borne well, and one might suppose that the sensibility of the stomach may be in some degree diminished, as it requires larger quantities of ipecacuanha, or emetic tartar, to produce vomiting in this disease. This observation in regard to emetics, is also applicable to bronchitis in children.

Kentish Town, December 1831.

CASE OF IMMOBILITY OF THE JAW,

And Taliacotian Operation.

By VALENTINE MOTT, M. D. & C. & C.

[With a Woodcut.]

On the 7th of April, 1831, I was consulted in the case of Miss Mary Park, aged 17, of Southbridge, Massachusetts.

Fig. 1.



Her attending physician, Dr. Samuel Hartwell, gave the following relation of the case:—In the autumn of 1822, she had an attack of typhus fever: the symptoms were mild in the commencement of the disease, and nothing unusual occurred until the middle of the third week, when tumefaction and redness were discovered on the left cheek, accompanied with slight delirium and general aggravation of fever.

“At the end of the third week, a dark vesicle, about the size of a pea, appeared at the angle of the mouth, announcing the existence of sphacelus, which in a few days extended to about two inches in diameter upon the side of the face. A crisis of fever now supervened, which was followed by sloughing of the whole gangrenous portion, leaving the teeth and gums exposed. Upon its cicatrization, the jaws remained immoveably fixed, being apparently tied together by a ligamentous band within and about the cicatrix. Her food was introduced into the mouth through a space formed by the removal of a tooth on the right side. The first set of teeth and the alveolar process of the diseased side were detached by caries. Most of the second teeth were developed in a few years afterward.

“No mercury was used in the treatment of the fever. Her general health is now very good.”

Her countenance was much disfigured, and exhibited the appearance represented in fig. 1.

Fig. 2.



As the only means of permanently overcoming the closure of the jaw, was the removal of the cicatrix, I determined upon excising and replacing it by sound integument from the face and neck.

Accordingly on the 8th of April, assisted by Dr. Vache, and in the presence of Drs. Hartwell, Barrow, Wilkes, Hosack, and several others, I performed the operation.

It was commenced by carrying an incision from a little within the upper angle of the mouth, around the outer margin of the cicatrix, to a little within the lower of the under lip, and by the immediate removal of the newly-formed parts included within it. The adhesions between the jaws were next divided, which enabled me, in consequence of the relaxation thus produced, to insinuate between the teeth of the opposite side, the point of the lever used in my former cases, with which I finally succeeded in opening the mouth.

This point accomplished, the lips were brought together at the angle of the mouth by a suture, and I proceeded to detach a portion of integument sufficiently large and of corresponding shape, to replace the part removed. See dotted line, fig. 1. It was turned into the space it was intended to fill, leaving a tongue three-quarters of an inch in breadth, connected with the adjacent part and sufficient for all the purposes of circulation. The cut edges were adjusted with extreme accuracy, by means of interrupted sutures and adhesive straps: the lower wound was contracted as much as possible by adhesive plaisters, and the whole covered with lint, compress, and bandage.

Previous to the operation she took Sol. sulph. Morphiae, double strength of Magendie's formula, grt. xiv. The operation occupied about an hour, and was sustained with a firmness peculiar to the female sex.

Evening.—Had been sick at her stomach, and vomited some coagulated blood, which had no doubt been swallowed during the operation.

9th.—Hardly any perceptible swelling of the face. As far as the parts can be seen all looks favourable. She can depress the lower jaw, by the effort of the will, to the extent of about half the width of the finger. I advised her to continue the motion of the jaw, from

time to time, as much as the soreness at the angle of the mouth would permit.

12th.—Some little tumefaction under the eye, but she makes no complaint. Directed an emollient poultice to the hard dressings upon the wounds on the neck.

14th.—Felt great comfort from the removal of the stiff dressings yesterday. The swelling of the cheek has subsided. Changed the lint again to-day.

15th.—She makes no complaint of her face, which in all respects is very promising. I removed three of the stitches from around the angle of the mouth, and re-applied lint and plaisters.

18th.—Every part of the flap appears to have united. Dressed the wounds with dry lint. Advised her to move the lower jaw a little downward every day several times.

May 12th.—Patch in the cheek entirely healed, (see fig. 2) The wound in the neck is nearly closed. She can open the jaws sufficiently wide to admit solid food.

Although I have before often operated for immobility of the jaw, and with the happiest results, and have once removed a deformity occasioned by a hole in the cheek, by the Taliacotian method, in the New York Hospital, with perfect success, I have never before seen it necessary to combine both operations in one individual; and the gratifying termination of this long and arduous case is a convincing proof of the excellence of the means employed, and of the power of art in repairing the ravages of disease, and in relieving its distressing consequences.

P.S.—*July.* A letter was received from Miss P. by the lady with whom she resided here, in which she stated her health to be excellent, the jaw to have acquired increased motion, and her friends to be much pleased with her improved appearance*.

SURGICAL CASES,

BY GEORGE BENNETT, M.R.C.S. &c. &c.

Case of Scrofula, illustrating the Efficacy of Sea Water in healing the Ulcerations.

SCROFULA is considered to be of here-

* Condensed from the American Journal of the Medical Sciences. Nov. 1831.

ditary origin, but several occult causes also produce the disease, being found in individuals the families of whom have never, for generations, exhibited any symptoms of the disease. The case I am about to relate was an instance of this kind, the patient seeming the solitary individual of his family marked out for the disease.

Though generally an hereditary, it is by no means a contagious disease, and is often, perhaps, dependent upon a peculiar diathesis. Dr. Mason Good observes, that "he had a gentleman under his care who suffered from this disease, yet, of eight brothers and sisters who have reached the middle of life, he is the only one who has discovered any tendency to such a complaint; nor is it to be traced through any part of the family lineage, as far as it can be ascended."

Scrofula is a disease of weak vascular action, and its causes are various. Heat, in those constitutions unaccustomed to a tropical climate, might cause a languid action; excessive cold might have the same effect, as well as vicissitudes of temperature; but, in all instances, much must be attributed to individual diathesis.

When a primary affection, the disease is said to attack individuals of any temperament or habit of body; but when hereditary, "it most commonly," observes Dr. Cullen, "affects children of soft and flaccid flesh, of fair hair, and blue eyes, smooth skins and rosy cheeks; and such children have frequently a tumid upper lip, with a chop in the middle of it, and this tumor is often considerable, and extending to the columna nasi and lower part of the nostrils."

Mr. John C—, æt. 19, and midshipman on board the ship *Sophia*, came under my care, for glandular swellings on the neck, on the 23d July, 1830. This was his first voyage to sea, and also his first visit to a tropical climate. Previously to leaving England he had had no glandular swelling, and his health had been tolerably good. Since his arrival in tropical latitudes, he had been subject to eruptions of acne, and had had frequent attacks of diarrhœa. He is stout and thick set, of a fair and ruddy complexion, light hair, and blue eyes. He had had, for some time before his application to me, an extensive swelling on the right side of the face, especially

of the parotid and submaxillary glands, which increased very slowly. He supposes it to have been produced by sleeping under an open port, in a current of air, about three months since; the day after which he felt a stiffness of the neck, and soon afterwards noticed the glandular swellings.

August 1st.—Pain and swelling slightly relieved by fomentations and a blister. Blister to be repeated, and a purgative powder administered.

3d.—Swelling on the right side reduced, glands on the left side slightly tumefied.

7th.—Glands on the left side have enlarged considerably; the chin feels numb from pressure on the nerves.

12th.—The submaxillary glands of the right side are much enlarged, one of which extends far round the back part of the neck.

23d.—I opened one of the glands on the left side, which had suppurated, and let out a quantity of curdy or cheese-like matter. Poultices were applied, and an alterative treatment adopted. Several other glands suppurated, and the ulcerations from them remained languid and indisposed to heal, under the application of digestive ointments.

October 16th.—Sea water to be applied two or three times a-day.

24th.—Ulcers are looking well.

From this time the ulcers gradually, but slowly healed, the sea water appearing to produce an healthy action. Some of the glands appeared reduced by the application, as they did not proceed to suppuration.

This case was cured by the 10th of January 1831, and the patient has had no scrofulous symptom since that time*.

—

Case of immediate Death from Rupture of the Heart.

On July 18th, 1827, a young man, named Adams, aged 27, residing at Plymouth, met with his death by the receding of a cart, which crushed him

* There is a curious circumstance respecting this patient, that for some time (upwards of three years) he had been subject to the hordeolum, or sty, which would be almost continually forming and suppurating; it constantly troubled him until, on the healing of the scrofulous ulcers, it also disappeared.

against a wall, occasioning almost instant death. On inspecting the body, the second, third, and fourth ribs were found fractured at their angles; the third and fourth were torn asunder, the intercostal muscles having been lacerated, and a quantity of blood was found extravasated between the integuments; the cavity of the chest was found filled with blood, but the lungs were found, on a minute examination, free from any injury.

The sternum was fractured into two distinct portions, about the distance of three inches from the cartilago ensiforme. The pericardium was found entire, but, on laying it open, and examining the heart, a rupture of the left auricle was discovered, as well as one of the pulmonary veins; there was a slight adhesion of the lungs, which no doubt proceeded from an old complaint. The rupture of the auricle and pulmonary vein might have been occasioned by their being distended with blood at the moment that the sudden shock was received. The countenance of the unfortunate man was pallid, resembling that of a person who had died from hæmorrhage.

London, January 10, 1832.

LUSUS NATURÆ.

To the Editor of the London Medical Gazette.

SIR,

CONSIDERING the following case of *lusus naturæ* worthy of being placed on record, I have forwarded it for insertion in the pages of your useful journal.

After a fair and natural labour, H. L. was delivered of her first child, which presented the following appearance:—From the sacrum and ischia extended an immense bag, or cyst, measuring sixteen inches in circumference, smooth externally, and highly vascular, containing fluid, soft and yielding; from the point of the sacrum a hard band was felt, extending into the coats of the cyst. The vagina was in its natural situation; but the anus was discovered under the right thigh, about an inch from the labia.

In other respects the child was strong, healthy, and well formed.

In the ninth week, the cyst having increased much in size, it was tapped, when four pints twelve ounces of serous fluid was drawn off. The fluid did not coagulate by heat, but showed a slight albuminous appearance on dropping in Tinct. Gallæ. The cyst collapsed, giving the appearance of an empty scrotum. The child appeared but little affected by the loss. The measurement previous to the operation was 22 inches.

The child from this time to the twelfth week continued to get weaker and restless, with great emaciation: the cyst filled to the size of a cocoa-nut. On drawing off the fluid (post mortem), it was about a quart. The internal surface of the cyst was like the pericardium, and studded with small cysts, containing serum. The cartilaginous band was connected to the inferior point of the sacrum, and was lost in a gradual manner in the internal surface of the cyst; there was an evident connexion with the internal structure of the sacrum, extending upwards to the spinal canal, which contained serum.

I am, sir,

Your obedient servant,
L. HUNTLEY, M.R.C.S.

Brixton-Hill, Nov. 21, 1831.

HYDRARGYRUS PRÆCIP. ALBUS.

To the Editor of the London Medical Gazette.

SIR,

SHOULD you deem the following method of preparing the *submurius hydrargyri et ammoniæ* (otherwise termed *hydrargyrus præcipitatus albus*) worthy of a place in your valuable Gazette, you are at liberty to insert it:—

To two parts of bichloride, or oxy-muriate of mercury, dissolved in cold water, add one part of subcarbonate of ammonia, dissolved also in cold water. Pour the solution of bichloride of mercury into a glazed vessel, capable of holding four liquid gallons, to which add the solution of subcarbonate of ammonia. After the carbonic acid has escaped, and the whole appears tranquil, filter and dry the precipitate in the usual way, which last will be found of a snowy white colour. By this method

is obtained a preparation, not only cheaper, but less troublesome to make, than that afforded by the usual process; for the muriate of potash which is formed by following the directions of the London Pharmacopœia is required to be washed away by repeated quantities of water; whilst, by pursuing the method I have now stated, the washing of the precipitate is dispensed with.

I am, sir,
Your very humble servant,
JOHN COLEY.

Bridgnorth, Dec. 27, 1831.

DR. J. JOHNSON'S REPLY TO DR. STEVENS.

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To the Editor of the London Medical Gazette.

SIR,

IN your last journal, Dr. Stevens has thought proper to prefer some grave charges against my character as an editor, such, indeed, as have never before been preferred during the whole course of my professional life. The first charge runs thus:—"That Dr. Johnson, at least, has wilfully endeavoured to mislead the public, by giving a very unfair statement of my views upon this subject." What will the profession think of Dr. Stevens's candour and veracity, when I aver (and fortunately can prove) that the statement, and the only statement which I ever gave of his views, was *written by himself, and given to me by his own hands*. Not having heard his paper read at the College, Dr. Stevens called on me, and requested that, in order to avoid mistakes or misconceptions, I would insert a *report* or analysis of his paper, drawn up by himself. This I did; and this is the "unfair statement" which I have given of his views, and by which he says I "wilfully endeavoured to mislead the public."

The next charge, or insinuation, is thus conveyed:—"It appears that Dr. Hacket is only acquainted with the contents of my paper on the blood, through the medium of Dr. Johnson's journal; or perhaps it was *through the medium of the Doctor himself* that Dr. Hacket received information of those misrepresentations, &c." Dr. Hacket unequivocally states that it was through the medium of the paper in my journal that

he became acquainted with Dr. Stevens's views; and I have only to reassert, that *that paper was written by Dr. Stevens himself*. As to the insinuation about my conveying information to Dr. Hacket, I have only to say that I never communicated, directly or indirectly, with Dr. Hacket in my life; nor did I know that such a man existed, till I received the document which I published. I envy not the heart of that man who can publicly make insinuations for which there is not an atom of support, and in which there is not an iota of truth. Dr. Stevens next says, that Mr. Greatrex's letter "*is apparently in answer to a letter from Dr. Johnson*." The letter is stated to be an enclosure from Dr. Hacket himself; and as to Mr. Greatrex, I never heard of his name before I received the said document, and never wrote a line to him, or received one from him, since or before.

But the most finished piece of effrontery is yet to come. Dr. Stevens says that, as he has accused Dr. Johnson of "wilfully endeavouring to mislead the public by misrepresentations," &c. "it is but fair to shew by *his own evidence* that this is the case." Now, gentle reader, whence draws he this proof of my misrepresentations? From the report which he wrote himself in my journal, and which he calls, *my review of his paper on the blood!!!* This is too bad! But, indeed, the whole train of accusations against me leaves but one solution of the enigma—namely, that the poor man has taken leave of his senses, and with them all correct memory of the past. I cannot suppose that any man, in his right reason, would lay himself open in the way Dr. Stevens has done. Instead, therefore, of retorting upon him the charge of wilfully misleading the public, which he has kindly endeavoured to fix upon me, I shall charitably suppose that the man wrote under a temporary adumbration of his mental faculties; and sincerely do I hope that the worthy doctor is not destined to exemplify the adage of antiquity—"quem Deus vult perdere, prius dementat."

Dr. Stevens constantly endeavours to mix me up in the discussion between Dr. Hacket and himself; though I did nothing more than publish the two documents written by the contending parties themselves, without taking any part, pro or con, in the controversy,—

except that of warning the profession against adopting the opinions of the one, till the statements of the other were before them. This is what Dr. Stevens calls "misleading the public." Probably he may find that the public think otherwise.

Dr. Stevens says I candidly admit that an *evil passion* was the principal cause why I attacked him. Where did I make this admission? where did I attack him at all? I never mentioned him or his speculations, till they were brought to my memory in the Westminster Medical Society; and then I only stated that I possessed a document from Dr. Hacket, contradictory of Dr. Stevens's views and assertions, which document should be immediately published. This is what Dr. Stevens calls "an ATTACK dictated by *evil passion*!" Heaven knows how little I thought or think of Dr. Stevens and his doctrines. I have something better to employ my mind; and, as for evil passion towards the man, why what, in the name of common sense, could engender such passion? Up to the moment when I made known Dr. Hacket's communication, I had only acted the friend of Dr. Stevens, by inserting his own report of his own paper on the Blood. But having published the statement of his antagonist, without mixing in the strife, I am now converted into an enemy, capable of falsifying the truth, and "wilfully misleading the public."—I am, Sir,

Your most obedient servant,

JAMES JOHNSON.

Suffolk-Place, Jan. 16, 1832.

[It appears to us that Dr. Johnson has entirely mistaken Dr. Stevens with regard to the extracts alluded to: he did not quote the first report of his paper which was given in the Medico-Chirurgical Review as being incorrect, but, on the contrary, to contrast it with certain observations on his doctrines which subsequently appeared, and which he states to be erroneous. We make this remark with no intention of "mixing in the strife," but if possible to prevent the necessity of a rejoinder from Dr. Stevens on this point, which as a matter of course must otherwise have followed. We are anxious to terminate a controversy to which, now that it has become a personal dispute, we are most unwilling to lend our pages.—ED. GAZ.]

216.—1x.

WOUND OF THE ABDOMEN.

To the Editor of the London Medical Gazette.

Yoxford, Suffolk, Dec. 8th, 1831.

SIR,

SHOULD the following case be deemed worthy of a place in the columns of your valuable journal, you will oblige me by inserting it.—I am, sir,

Your obedient servant,

JOHN JAMES HALLETT.

I was sent for on the evening of the 21st November to a young man, E. B. ætat. 22, who had attempted suicide. It will suffice, in alluding to the history of the case, to state, that after being twice prevented (within a few minutes of committing the act) from using two knives, he rushed into a cottage, and seizing one from a table at which some people were at their meal, escaped to the road, and was soon found, with a wound in the abdomen, of a semilunar form, having the umbilicus for its centre: its direction was oblique from the right side downward, and from its irregular appearance seemed to have been made with a knife not very sharp. I found the sufferer on his back, on the floor, with a considerable portion of large and small intestine and omentum protruded: the portions thus exposed would have filled two quarts. There was a decidedly fœtid odour at the wound, but the displaced intestine was much inflated, shewing that at least those portions *without* the abdomen were free from wound. With great care and perseverance I succeeded in replacing the parts, and confined them by applying three interrupted sutures to the wound, and a light dressing, with a roller of calico, four times round the body. It was fortunate that I remained during the night in the house, as, at about 11 p.m. his struggles, which were very violent, again displaced two knuckles of large intestine, (colon) which, with great difficulty, I succeeded in returning between the stitches, in which situation they had been protruded. From that night he continued till the Sunday following (27th) perfectly tranquil, on low diet, and with a pulse under 80. I should say, that on Wednesday 23d the bowels acted freely, and have continued to do so up to this time. On the 27th there being, for the first time, a slight tenderness in the abdomen, I ordered twelve leeches to be applied,

2 Q

which had the effect of removing it altogether. On the 30th, as a measure of precaution, the leeches were again applied. The treatment has been most simple, consisting of—

Liq. Ammon. Ac. ʒij. Tr. Hyoscyam. ʒss. Aq. Cinnam. ʒvij. Syrup. ʒss. ʒi. haust. 4ti-horis sumendus c. Pulv. Antim. gr. ij. et Ant. Tart. gr. ʒss.

The wound has done well from the first, and is now skinning over rapidly. I should say, that the morning after the occurrence, the knife with which the wound was effected was brought me, having been found close to the spot where it was used; and what tended to confirm my idea of the intestine having been wounded was finding a quantity of feculent matter close to the point of the blade, and covering a great portion of it. In a day or two (Saturday) it is my intention to send him to the asylum. I shall refrain from any comments on this case for the present, but leave it to your readers to form their own opinion. The feculent matter on the knife corresponded in nature with his diet, which had been entirely of coarse bread for a day or two previously.

PAPERS AND CORRESPONDENCE

ON

CHOLERA.

EPIDEMIC AT SUNDERLAND.

To the Editor of the London Medical Gazette.

SIR,

If the following observations on the late epidemic of Sunderland are worthy of a place in your journal, you will oblige me by inserting them. As the Gazette is open to the expression of all opinions, no apology will be necessary if I have differed from you on some points.

The first cases which were publicly made known as epidemic cholera were those of the Sproats, three in number, one of which proved fatal on the 26th, another on the 31st of October. But several sporadic cases, exactly similar, had occurred from the commencement of August.

1. The first case that I am aware of was that of a middle-aged man, a potter, living near the river on the north side, about three miles from the harbour mouth, and two from the town of Sunderland. The case fell under the care

of Mr. Dixon, who, at the time, considered that it differed in nothing from the disease described by the Indian writers, and who has not since seen reason to alter his opinion. The man recovered slowly.

2. The second was that of — Arnott, a middle-aged labourer, living at Pallion, on the south bank of the river, about a quarter of a mile higher up than the first case. His residence was 50 or 60 feet above the level of the river, and about the distance of a stone's throw from it. He became ill on the 8th of August, and died in twelve hours. He also was under the charge of Mr. Dixon, with whom and Dr. Brown I had an opportunity of seeing the body the next day. I certainly have not since seen one in which the external appearances after death were more characteristic of malignant cholera. The collapsed face, sunk eyes, surrounded by a dark circle, blue hands and nails, contracted and shrivelled fingers, and great rigidity of the muscles, were most strongly exhibited. No examination was allowed.

3. The case of Robert Henry, æt. 43, a pilot, occurred next, and terminated fatally on the 14th of August. He lived in a row of cottages near the mouth of the harbour, at the east end of Sunderland, and in an airy situation.

4. — Pearson, a shipwright, middle-aged, living at the western extremity of Bishopwearmouth, was the next. He became ill on the 27th, and died on the 28th of August. He was a patient of the Dispensary, under the care of Dr. Haslewood, who assures me that every symptom of Indian cholera was present.

No connexion between any of the above cases could be traced, nor had they (with the exception of course of the pilot) any connexion with the shipping. They were regarded as cases of aggravated English cholera by the majority of the medical men, though it was strongly suspected by some that they were the forerunners of the epidemic.

Cholera continued very prevalent and severe during September, but I know of no deaths from it in that month. I have been informed by very good authority that a fatal case occurred in a middle-aged man, about the 12th of October; he had not been seen by a medical man, but he had spasms, and the characteristic vomiting and purging, and died rapidly.

Mr. Holmes has obligingly furnished

me with the following information concerning the Sproats, who were originally under his care:—

1. — Sproat, æt. 60, labourer, Long Bank, Sunderland; died 26th October.

2. — Sproat, æt. 10, his granddaughter, became ill half an hour after his death.

3. — Sproat, æt. 32, son of No. 1. father of No. 2, became ill on the 27th, Nos. 2 and 3 were removed to the Infirmary; No. 2 recovered; No. 3 died on the 31st. On the same day died,

4. — Wilson, a keelman, High-Street, Sunderland, and,

5. — Rodenby, a shoemaker, Monkwearmouth Shore; and, on the 1st of November died,

6. Elizabeth Turnbull, nurse at the Infirmary, who assisted in laying out the body of No. 3.

Between Nos. 1, 2, 3, and 6, there was an evident chain of connexion; 4 and 5 were distinct, and isolated from all known sources of contagion.

No deaths occurred between the 1st and the 6th of November; on the latter day there were several, on the 7th more, and on the 8th the cases were numerous, and the existence of the epidemic amongst us could no longer be doubted.

As to the origin of the disease, a subject involved in the utmost mystery, I am inclined to believe it to be as indigenous here as it was in Jessore in 1817. In hazarding this opinion, I am aware that I am in opposition to a mass of very powerful evidence, collected by the advocates of the doctrine of contagion; but whilst I firmly believe the disease to be contagious, I think also that too much has been ascribed to this principle. Contagion once granted, its supporters have attributed to it every case of cholera that occurs. Now if the disease arose spontaneously in one individual, it might in a hundred, or a million.

There has been great intercourse between this port and Riga, Petersburg, and Archangel, during the existence of cholera at those places; and it was the mortality among the British sailors, of whom several were our townsmen, that occasioned the institution here, of, I believe, the first Board of Health in the kingdom. It has been supposed that the sailors, on their return from those ports, brought the disease with them; the quarantine, which is a human institution, and incapable of subverting the

laws of nature, being too short to extinguish the “germs,” or the “seeds,” figurative terms by which diseases are said to be propagated. But if the sailors brought it with them, it was much more likely that it should make its first appearance in some other port than this; for although many Sunderland vessels go every year to Petersburg, &c. the intercourse is indirect, and an exceedingly small proportion return direct to this port. They return with cargoes to almost every port in the kingdom, from Aberdeen round by the south and west to Glasgow. Independent of the vessels which trade directly between this country from London, Liverpool, Bristol, Hull, and other places, there have been more Sunderland ships from Russia delivered at any one of the ports just named than at Sunderland.

It is to be recollected, however, that when the foreign voyages were over, and the ships returned to the coasting trade, there was a concentration, a *reunion*, in Sunderland, of a great number of persons who had been, at some period of the summer, in the unhealthy towns. Every sailor too has a chest in which he keeps his clothes, and whose entire contents he would bring to his family in the town, for purification. Moreover, some of the chests which came home had belonged to persons who had died abroad, and had probably never been opened since the clothes of the deceased were put into them. The combination of these circumstances may perhaps be thought sufficient to account for the introduction of any contagious disease. But they do not explain the unusual prevalence of cholera during the summer, nor the fatal cases of August; nor has it been observed that the families of sailors have been more visited by cholera than others. Sailors themselves have been wonderfully exempt during its prevalence here. Besides, the sailors of London and other places died abroad, and their clothes would doubtless be sent home in like manner; yet they have not brought cholera.

Whatever, therefore, were the facilities for the importation of cholera here, they were much greater in other places. So far from following the “great routes of human intercourse,” it has chosen one of the least frequented paths.

Here I beg leave to correct an error which appears at page 238 of this volume, where it is stated that a vessel

from Hamburg performed quarantine in the Wear. No ship has entered the harbour from Hamburg without a clean bill of health, which expresses not only that the crew, specifying their number, are in "perfect health," but that, up to "that period, perfect health prevailed in the town and port of Hamburg, and the country adjacent, which continued free from cholera morbus, or other contagious or infectious disease." This bill of health is granted by the British consul at the place. In addition to this, every vessel from Hamburg, since June, has been examined by a medical man before being liberated. What have been styled suspected vessels, were ships from Holland detained under observance or precautions; and it is to be recollected that, up to this day, there is no suspicion of cholera in Holland.

That the spreading of the disease is the effect of some unknown power, very different from contagion, is, I think, evident from the phenomena which it has presented in this country. Newcastle, a large and populous town, within twelve miles of Sunderland, and holding daily intercourse with it, escaped the disease a whole month. Gateshead, another populous town, on the *hither* side of the former, and holding similar intercourse with both it and Sunderland, escaped for another month. London, holding constant intercourse with all these places by land and by sea (by sea it is said to travel most rapidly), yet remains free, nearly three months after the appearance of the epidemic on our shores. The contagionist says this only proves the inhabitants of these places not to be predisposed to the disease at the time the first emanations of contagion reached them. The reasoning is very convenient; it assumes that contagion is constantly travelling, and arriving at these places, but that it is not until the constitution is undermined by predisposition that the population falls a prey to the venom of contagion. A corollary of the same proposition is, that *predisposition travels*; for contagion is assumed to be constantly travelling, but it is only when accompanied by predisposition that it is rendered obvious, by its effects, to the senses. Another contagionist reverses the order of the causes, and alleges that contagion constantly spreading, predisposes all the districts surrounding an infected place; the disease then lies dormant, or latent,

in the constitution, and all that is required to develop it is certain atmospheric vicissitudes, or debilitating causes, the peculiar nature of which it is not easy to comprehend, or is perhaps beyond our comprehension. But the former mode of reasoning is the most usual, namely, that contagion travels, and predisposition renders liable. If a populous town on one side of a river suffers from cholera, and another on the opposite side is free from it, it is because the people on the one side are predisposed, whilst those on the other are not. If the disease arises in Sunderland, and is presently found in various districts to a hundred miles north, and not to the distance of ten miles south, it is because the inhabitants to the north are more predisposed than those to the south. In short, it is a postulate with the contagionist, that, for the disease to prevail, the people must be predisposed; but what this predisposition consists in we are never told. We have certain general ideas about the predisposing effects of intemperance, exhaustion, privation, and the like; but these are not sufficient, for we find the people of one place just the same in these respects as those of another, but we never discover which of them were predisposed until the disease has attacked them. Hence, to say that people *must have been* predisposed, because we see that they have been attacked, conveys no information whatever. The term predisposition, in this loose acceptation, appears to me inadmissible in scientific language; it merely expresses an unknown antecedent state.

Even in a narrower circle, the effects of contagion are apt to be overrated. Nothing is more common than to find, where a death has occurred, that the rest of the family are affected with diarrhœa, which, if neglected, runs into fatal cholera in some of them. Hence a succession of deaths, giving the appearance of one person having taken the disease from another, whereas they were all affected with the precursory diarrhœa at the same time. In a family too there is generally a similarity of constitution, habits, clothing, diet, residence, and other circumstances; and it is peculiarly likely that the cause, whatever that may be, which produces cholera in one member, shall have the same effect on the others. Indeed, it is remarkable that it has so frequently attacked one member of a family, and

spared the rest, who might be supposed more than ever liable to its visitations, from the fatigues of night watching, and the depressing effects of grief; and what was unfortunately too frequently the case, the sudden deprivation of the earnings of the deceased, who had been their main support.

Throughout the whole epidemic, cases were constantly occurring wherein no connexion with any source of contagion could be traced. This was most striking in the commencement, about the 6th, 7th, and 8th of November, when the disease suddenly manifested itself in many separate points totally unconnected. I can hardly conceive it possible to account for this by contagion, unless we conceive the whole atmosphere filled with it. After so many foci of contagion had been established, it was not so easy to gainsay the possibility of a person's having been exposed to it; but the number of persons obviously exposed to it with perfect impunity, was very great. Medical men have been obliged to perform the duties of nurses, to assist in the application of warmth and friction, restore the patient to a proper position when he would not remain a moment at rest; to bleed, lift to the bath, raise the head, and administer medicine. After death, they have inspected the bodies, lifted them out of bed and replaced them, and imbued their hands in the animal fluids; yet no death, and perhaps scarcely a case of serious illness, has occurred among them. And I may here state that I have not heard of any medical man conveying the contagion of cholera to his other patients.

Having said that in many families cholera has attacked one person alone, I ought also to state that in others it has committed the most dreadful ravages, three, four, or more individuals being destroyed. In some cases there is much probability of their having taken it one from another by contagion; in others there is not this probability. Thus,

1. Robert Henry, pilot, died 14th August.

2. Margaret Henry (unmarried), cousin to No. 1, became ill Dec. 11th; died Dec. 15th or 16th.

3. John Parkin, æt. 4, nephew of No. 2, became ill Dec. 12th.—Recovered.

4. Margaret Henry, æt. 13, daughter of No. 2, became ill Dec. 13th; died same day.

5. Mrs. Parkin, sister of No. 2, mother of No. 3, became ill Dec. 16th; died Dec. 18th.

6. William Henry, alias Thompson, son of No. 2, became ill Dec. 17th.—Recovered.

No. 5 was in frequent attendance on No. 2 until No. 3 required her attention. Both 3 and 5 may have got it from 2.

Nos. 4 and 6 lived in the same house as 2, and may have had it from the same common cause.

Again,—

1. James Ellemore, æt. 60, died Nov. 6th.

2. Elizabeth Hopper, an elder sister, died Nov. 17th.

3. Jane Johnson, another sister, died Nov. 23th.

4. Thomas Ellemore, æt. 67, a brother, died Dec. 25th.

5. Ann, wife of No. 4, died Dec. 27th.

The first four persons lived in separate houses, from a hundred yards to half a mile distant from each other, and were ill at very different times. What the intercourse might be, I do not know.

On the important subject of cure, I fear there is little satisfactory to be said. The number of recoveries from *cold blue* cholera has borne a very small proportion to the deaths. Of the 330 recoveries reported, a great majority were not attended with the coldness and lividity of the extremities, great collapse, and loss of pulse, so characteristic of the dangerous cases.

The Indian rule of bleeding has been freely tried with various success: sometimes blood could be obtained, at others it could not; in the latter case, no harm was done by the attempt. When blood was obtained in the commencement, it was often productive of benefit, the circulation becoming equalized, the patient expressing great relief, the pulse improving and continuing perceptible to the termination, whether in death or recovery. Sometimes, again, it appeared to diminish the powers of life. On the whole, it is a doubtful remedy; a criterion is wanting to guide us in the employment of it. Dry external heat is of great importance: hot air baths have been tried, but the deadly coldness of the extremities is totally unaffected by them; the contact of hot solids, as bricks, bottles, bags of sand, is much better. Friction of the extremities with hot flannels should be next to in-

cessant: it is one of the few means we possess of restoring the circulation; indeed there have been recoveries principally due to the assiduity with which it was employed. Brandy is always used; often rejected a few minutes after it is swallowed. Opium sometimes relieves the sickness, sometimes it does not. Calomel, in the majority of cases, appears to possess little or no efficacy. Viewing the mucous membrane and the skin as one continuous surface, and considering the suspension of their powers of absorption, we are led to the employment of such remedies externally and internally as act as mere stimulating applications. Sinapisms are to be applied to the epigastrium, to allay the vomiting, and to the calves and thighs, to cure the spasm. A mustard emetic has often succeeded in restoring the pulse. Oil of turpentine by mouth in $\frac{3}{4}$ ss doses, and by injection, is a powerful stimulant. Camphor and the essential oils of mint and cajeput are inferior to oil of turpentine in stimulating power. I have not seen any trials of the fixed alkalies, but have heard of their failing like every thing else.

The consecutive febrile state is to be treated like fever under any other circumstances. There is usually very great cerebral oppression, indicating blisters to the nucha and shaving of the head. Small doses of calomel are also beneficial. The greatest care is required in avoiding errors in diet during convalescence.

Dissection has hitherto revealed nothing which accounts for the formidable train of symptoms whose cause is sought. There is an accumulation of blood in all parts of the venous system, with the curious exception of the *venæ portæ* and its branches. The engorged state of the lungs, and the oppressed respiration during life, point to the respiratory system as principally suffering, either idiopathically, or symptomatically of some altered condition of the nervous system.

Prophylaxis.—The attack is generally preceded by diarrhœa, varying in duration from six hours to three days: this is the critical time for successful treatment. I have found pills prepared according to the following formula very successful in arresting the diarrhœa:—

R Opii gr. xv.

Hydrarg. Submur. ʒj.

Pulv. Bacc. Capsici ʒiiss. vel ʒij.

Confec. Rosæ q. s. ut fiat pil. lx.

As they are in constant request when visiting the abodes of cholera patients, it is convenient to carry a box in the pocket. One or two may be given every three hours, until relief is obtained. They produce a state of constipation, which continues two or three days, and which it is quite unnecessary to remedy by laxatives, as the bowels soon resume their natural functions. When there is much pain of abdomen or griping attending the diarrhœa, it is alleviated by hot fomentations or a sinapism.

During the prevalence of cholera, there are observed among persons otherwise healthy, various anomalous affections of the nervous system, as spasms and cold sensations of the hands, feet, and legs, peculiar thrilling sensations of the extremities of the fingers and toes, and a feeling of great anxiety in the præcordial region.

I am, sir,

Your obedient servant,

HENRY OGDEN, M.D.

Sunderland, Jan. 14, 1832.

FURTHER REMARKS

ON THE

TREATMENT OF CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

London, January 16, 1832.

WITH reference to the observations lately addressed by me to the Central Board of Health, regarding the use of mustard emetics in spasmodic cholera, which appeared in your Journal last week, I now beg to offer some additional remarks connected with this subject, as my former communication was not originally intended for publication.

It is generally acknowledged, by those who have had the most experience in this disease, that relieving the venous congestion forms the principal indication of cure; it is also admitted, that this object is best accomplished by bleeding, when it can be carried into effect; but it too often happens, that, owing to a languid, or totally suppressed circulation, the blood cannot be induced to flow in sufficient quantity to afford relief; nor have the usual means used for the purpose of facilitating this, such as internal and external stimulating applications, been productive of any decided advantage.

As it has been ascertained that full vomiting has the power of restoring the circulation, if carried into effect before the stomach has so far lost its vitality as not to be acted upon by remedies, it appears to me that the combined practice of emetics * and bleeding†, under these circumstances, affords a fair and rational prospect of fulfilling the indication alluded to; as the flow of blood will be rendered more certain and effectual from the stimulus previously given to the arterial system.

Under such impressions, I would beg to suggest, that, in the commencement of the attack of spasmodic cholera, or as soon as the patient applies for advice, a mustard emetic be administered, and repeated if necessary, with the view of producing full vomiting. This being effected, a small portion of brandy, or any other diffusible stimulus that may be preferred, may be given in the patient's drink, and occasionally repeated, but only in small quantities at a time.

When the circulation and heat of the surface are sufficiently restored, (and which may reasonably be expected soon to follow the operation of the emetic) bleeding should be resorted to, and carried to such an extent as may be deemed advisable, from the effect produced on the pulse, or according to the judgment of the practitioner.

Independent of the impulse given to the system generally, by vomiting, this operation usually has the effect, in this disease, of producing bilious evacuations, which is a very important circumstance.

The bleeding (when a sufficient quantity can be obtained) is also productive, in addition to relieving the venous congestion, of two beneficial effects; namely, that of moderating the violence of reaction, and of lessening the tendency to cerebral affection in the consecutive fever—both of which are more easily prevented by anticipation, than combated when actually present. It may be inquired, what is to be done if full vomiting cannot be produced? In such a

case I would endeavour, if possible, to bleed the patient.

The remainder of the case I would treat in the manner recommended in my former Report, particularly by calomel, in moderate doses, combined with a small portion of opium, with a view of keeping up the action of the biliary system. When, however, pain or tenderness is felt over the belly, leeches should be *freely* applied, followed by warm fomentations for some hours. A strict antiphlogistic regimen must be observed; and, should the reaction shew itself in such a way as to indicate further bleeding, this must not be neglected, although it should be *cautiously* used.

When the head becomes affected, as is too often the case in the consecutive fever, local bleeding by cupping on the temples, or the application of leeches, should be employed, and ice applied to the head.

It may be necessary to add, that a strict attention to regulating the diet of a patient during the consecutive fever is no less essential in a state of convalescence; as I have myself observed some very marked instances of relapse of the fever, induced by irregularity in diet, all of which are well known to some friends of mine who were at Sunderland at the time. I wish it to be understood that I do not place reliance solely on the remedies just alluded to, but that I would avail myself of all other means, such as mustard poultices, frictions, heat * (particularly applied along the spine, &c.) and the occasional use of stimulants when indicated†.

When the cramps in the limbs are severe, immediate relief is obtained by firmly tying a handkerchief round the part affected. This was tried by Dr. Macann, in a case of severe cramps of the leg, with very good effect.

Your obedient servant,

OWEN LINDSEY, M. D.

Deputy Inspector Gen. of Hospitals.

* I consider tartarized antimony objectionable as an emetic in this disease, in consequence of the sedative effects which sometimes follow its exhibition.

† Bleeding is here recommended to be *generally* adopted in this disease, subject, however, to certain exceptions, such as the age, constitution of the patient, duration of the attack, &c. &c. &c. which can alone be judged of by the practitioner in attendance.

* I am of opinion, the application of heat, however ingeniously employed, can be viewed only as an adjuvant to more powerful remedies.

† In my former report, I stated that the mustard emetic had not been used in Sunderland previous to my making a trial of it: I wish now to correct that observation, by stating that such was then the impression on my mind, but which I freely admit may have been erroneous, from having been misinformed on this point.

MUSTARD EMETICS IN CHOLERA.

To the Editor of the London Medical Gazette.

Sunderland, Jan. 14, 1832.

SIR,

ALLOW me to request the correction of an error which appears in your Supplementary Number, in the report of the case of Mrs. —, to which my name is attached; it is the substitution of 3 for 5, in the statement of the quantities of laudanum and æther administered.

I beg at the same time to remark, in reference to the communication of Dr. Lindsey, that emetics were employed in this town so early as the 13th November, by Mr. Mordey, surgeon, and with precisely the same view with which the mustard has been employed. For this remedy we are indebted to Dr. Gibson, who proposed it on the 7th December, at a medical *soirée* at Dr. Brown's.

I believe that, in point of fact, the remedy employed by Mr. Mordey is the safer of the two, and quite as effectual; both, however, have failed in many instances. In reference to another statement in Dr. Lindsey's communication, I must, in justice to the profession, state, that Dr. Lindsey appears to have been misinformed when he was told that "the fact of diarrhœa occurring as a preliminary symptom of this appalling disease, was too generally overlooked." I believe that, except at the very commencement of the epidemic, this fact was known to all the profession, and that they used every exertion to make it generally known amongst the people. Trusting you will excuse my troubling you with these facts, I am, sir,

Your obedient servant,

W. HASLEWOOD, M.D.

Physician to the Sunderland Infirmary.

January 15.

P.S.—Since writing the above I have met Dr. Brown; he perfectly recollects the mention of mustard by Dr. Gibson on the 7th; and he empowers me to state, that the importance of attending early to diarrhœa was pointed out by him to Dr. Lindsey and others, on their first arrival in the town*. W. H.

* The suggestion respecting the contents of the Monthly Parts shall be attended to.—E. G.

NOTE FROM DR. GIBSON.

DR. GIBSON presents his compliments to the Editor of the Medical Gazette, and begs to inform him that mustard, as an emetic in the cure of cholera, was first suggested to him by Dr. Smith, an eminent physician of Newcastle, who, having many years ago practised in the West Indies, where he was subject to severe attacks of bilious cholera, unaccompanied with spasms of the muscles of the lower extremities, always experienced the greatest relief from it. Dr. Smith also informed Dr. Gibson that a mustard puke was a popular remedy in the coal districts for that state of asphyxia caused by choke damp. Dr. Smith had not at that time seen the malignant cholera, but certainly to him is due the suggestion that mustard, as an emetic, might be serviceable in the cure of it.

"Suum Cuique tribuito."

31, Brompton Square,
Jan. 18, 1832.

CHOLERA AT NORTH SHIELDS.

To the Editor of the London Medical Gazette.

North Shields, Jan. 16, 1832.

SIR,

I do not send the annexed letter for the purpose of insertion in the Gazette, it having appeared in some of the provincial newspapers, but merely to let you know what is doing in this quarter. I should infinitely have preferred throwing it into a different form, and sending it to the Gazette; but the object in view was to circulate it, with as little loss of time as possible, amongst the obscure villages in this and the adjoining county, where the disease had broken out, that the medical practitioners in them might know what was doing elsewhere. The mortality in some of these villages is very considerable; out of a population of two, three, or four hundred, the deaths in very many instances have far exceeded those of this town. I am not aware that any returns are made from those places. The evidence I have seen of the contagiousness of this disease is quite overwhelming; none but a predetermined sceptic could doubt it.

The proportion of deaths, in the reports from this town, appears great; but out of the first ten cases where opium and stimulants were trusted to, nine died, three have either refused medical aid, or trusted to quacks, and out of the remaining number who have died, several cases occurred where blood could not be obtained; indeed, where it was evident no means could avail.

I remain, Sir,

Yours faithfully,
EDWARD GREENHOW.

The following is an extract from the letter alluded to in the preceding, in which the method is described which has been found "beyond comparison the most successful:"—

The first indication in this disease seems to be, to relieve the heart from the mass of blood forced upon it, and therefore bleeding, wherever blood can be obtained, has seldom failed to produce the desired end, and bring on a speedy reaction:—the blood in the first instance is generally of the colour and consistence of tar, evidently highly carbonized; but as it continues to flow, it becomes both of its natural colour and consistence, proving that the heart is acting more vigorously, and propels the blood through the lungs, so as to acquire its due portion of oxygen. Should the reaction be feeble and incomplete, some stimulant may be carefully administered, to assist in promoting it; a little hot brandy and water seems to answer best; but should reaction become violent, it must be moderated by the abstraction of more blood. The second indication seems to be, to restore the action of the liver and kidneys, and this has been found to be best attained by the following plan:—administer a scruple of calomel and one grain of opium, and continue to give every hour from five to ten grains more calomel, either alone or joined with a minute dose of opium, in the intervals giving effervescent draughts, both to allay thirst and vomiting, and also to assist in determining to the skin, and as soon as the stomach will bear it, a full dose of castor oil, or some other laxative: as soon as the patient passes feculent evacuations, he generally experiences complete relief. External means are also necessary in this disease. The application of heat to the surface of the body, and diligent frictions with hot flannels;

also throwing three or four pints of some warm fluid (gruel) into the rectum with the forcing pump, has been found a useful auxiliary.

Where no blood can be obtained from the arm, an emetic in some instances has restored the arterial action, and thus enabled the attendant to bleed successfully.

Large doses of opium and the free use of stimulants, seem to be decidedly injurious, and where the vomiting and purging have been checked by such means, the death of the patient has been accelerated. In fact, the peculiar secretion thrown out by the stomach and bowels, seems to be an effort of nature to relieve the heart from the load of blood with which it is oppressed, and if the vomiting and purging be checked without bleeding in the first instance, death speedily ensues.

GALVANISM AND SODA IN CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

I OBSERVE, in your journal of the 7th instant, that galvanism has been recommended as a curative process in the treatment of the cholera. The expediency of resorting to every means of relief that science or experience may suggest, in the management of a disease which, under every mode of practice, has hitherto been dreadfully fatal, is so obvious, that I presume you will think no apology necessary when I crave a brief space to urge the use of a medicine I have elsewhere recommended; and also to reiterate the suggestions of your correspondents in recommending a trial of galvanism, supported as this is by the scientific researches and logical deductions of Dr. Wilson Philip, and the mature experience, in the treatment of Asiatic cholera, of Dr. Whitelaw Ainslie; who, in his "Observations on the Cholera Morbus of India," published in 1825, says, "if, in the first instance, so direct and overpowering was the morbid influence, that neither the vomiting nor spasms were present, I should have recourse at once to galvanism, to supply the deficiency of nervous influence in the sinking frame." p. 71.

I also observe that the carbonate of soda has been given, by a military surgeon, with the effect, in the case of Charlotte Wardroper, of producing convalescence in two days; and that "Mr. Torboeck, an intelligent surgeon," had given the carbonate of soda in some recent cases, and found it of great benefit; and in one case, where collapse had taken place before the patient was seen, the carbonate of soda, liberally administered, renovated the patient. This practice harmonizes, as far as its antacid properties extends, with the practice found to be successful by Dr. Whitelaw Ainslie, who, in speaking of the cholera morbus, as he saw it on the Coromandel coast, says, "on examining what is usually vomited on such occasions, I invariably found it of an acescent nature;" and, in consequence, he gave "subcarbonate of magnesia in full doses," and "found that, in very few instances indeed, he had occasion to repent it." This accords with my experience; and, in the *Times* of the 21st June, 1831, I gave a case of cholera of these climates, in which 5j. doses of carb. sodæ were given, and I alluded to several more in which I had detected acid, and successfully followed the same practice: it is therefore with much satisfaction that I see it faithfully followed by enlightened medical philosophers;—I say faithfully, because not only the medicine, but the dose also is the same.

In another place I have given cases of typhus fever treated with the carb. sodæ; and my experience of its efficacy induces me earnestly to recommend the use of this medicine in the fever that succeeds the spasmodic attacks of the cholera; a fever which I expect will not supervene when the carb. sod. has been previously and liberally used; but should I in this be mistaken, I doubt not it will be greatly modified, and prove much less fatal. The facts on which these opinions are founded have been many years accumulating, but it may not, in this paper, be deemed expedient to adduce them in support of the practice I have adopted and recommended.

I am, sir,

Your obedient servant,

PAUL SLADE KNIGHT.

Connaught Terrace,
Jan. 11, 1832.

CHOLERA NOT A NEW DISEASE.

To the Editor of the London Medical Gazette.

SIR,

SHOULD you consider the following remarks worthy of a place in your valuable journal, they are at your service. I have the honour to remain, sir,

One of your earliest subscribers,
And obedient servant,

P. LOVELL PHILLIPS, M.B. Oxon.

29, Half-Moon Street,
Dec. 23, 1831.

It has been confidently asserted, that the disease which has been the scourge of Europe for some time past, and more lately of our own shores, is not only not the same as the Asiatic cholera, but that it is altogether new, something *sui generis*, never before known. To the former of these opinions I would answer by referring to the judgment of those most competent, as I conceive, to determine the question—viz. those who have seen the disease both in Asia and in Europe, and who generally regard it as the same in both countries. If there was no modification of a disease such as this, by climate and habits, it would, indeed, be most extraordinary. To the latter opinion, I would reply by referring to the description of the disease by Aretæus, in his second book on the Symptoms of Acute Diseases, Chapter V. and the corresponding chapter on Treatment. I will briefly draw a parallel between this description and that of Drs. Russell and Barry, as recorded at page 693 of the last volume of your Gazette.

1. Drs. R. and B. say, diarrhœa, at first feculent, with slight cramps in the legs, nausea or heat about the pit of the stomach, give the longest warning. Indeed, purging or ordinary diarrhœa has been frequently known to continue for one, two, or even more days, without any other remarkable symptom.

1. Aretæus.—The disease begins with vomiting and purging, and what passes from the bowels is at first feculent and of a bad colour, and the complaint is at first entirely free from pain.

2. Drs. R. and B.—The vomiting or purging, or both these evacuations, are liquid, like rice-water.

2. Aretæus.—In appearance, the matter first vomited is like water, and the stools are also liquid.

3. Drs. R. and B.—The vomiting and purging are not the most important or worst symptoms.

3. Aretæus (on the Treatment).—The retention of those matters which ought to be evacuated is bad (*επισχεσις των φερομενων κακον*).

4. Drs. R. and B.—The patient, after a few days' diarrhœa, is frequently struck almost lifeless.

4. Aretæus.—If the disease increases, there is fainting (*λεποθυμια*).

5. Drs. R. and B.—The look is expressive of terror, wildness, and, as it were, a consciousness on the part of the sufferer that the hand of death is upon him.

5. Aretæus.—There is an anxiety, or a want of determination (*αποριη*).

6. Drs. R. and B.—Sometimes there are tetanic spasms of the muscles both in the legs, thighs, and loins.

6. Aretæus.—There are frequent spasms of the muscles both in the legs and arms.

7. Drs. R. and B.—The fingers and toes are reduced at least one-third in thickness, and cramps frequently begin in their extremities.

7. Aretæus.—The fingers are incurvated.

8. Drs. R. and B.—The nails put on a bluish pearl-white, and the skin is deadly cold and damp.

8. Aretæus.—The nails are livid (*πελιδνοι*), the extremities quite cold, and also the whole body; and in extreme cases the patient perspires.

9. Drs. R. and B.—Vertigo.

9. Aretæus.—Vertigo.

10. Drs. R. and B.—The pulse is either small as a thread, and scarcely vibrating, or else extinct.

10. Aretæus.—The pulse is extremely small and frequent.

11. Drs. R. and B.—The patient speaks in a plaintive whisper (the vox cholericæ).

11. Aretæus.—There is no voice (*αφωνια*).

12. Drs. R. and B.—The secretion of urine is almost totally suspended, and the bladder found contracted.

12. Aretæus.—The urine is retained on account of the spasm of the bladder, but it is not collected in any great quantity, on account of the derivation of the fluids to the bowels.

13. Drs. R. and B.—The lips, face, neck, and the whole surface, assume a leaden, blue, purple, black, or deep-

brown tint, according to the complexion of the individual and the intensity of the attack.

13. Aretæus, in describing the progress of the disease (towards the end of the chapter on Treatment), says that the patient becomes livid (*πελιος*).

14. Drs. R. and B.—Convalescence from cholera has been rapid and perfect.

14. Aretæus.—If all things go on improving, the patient may be dismissed to his ordinary occupations on the second or third day.

I do not see how it is possible, after reading these parallels, to deny the identity of the diseases which the respective authors intended to depict: had they been describing the same epidemic, they would hardly have done it in language more nearly alike. But we have still further grounds for coming to the same conclusion. *Mutatis mutandis*, the treatment recommended by Aretæus corresponds with that generally adopted in the present cholera—viz. warm cloths, and friction with stimulating unguents, to the feet, legs, belly, back, &c. &c.; hot drinks, with wine and aromatics, until the heat return (*εσ ανακλησιν θερμης*). In fact, amongst a host of stimulating remedies, he says that the same means are to be used which he had previously recommended in syncope. It would hardly be worth while going over the individual remedies, since the nomenclature would require a copious comment. I trust, however, that what I have adduced will be sufficient to convince every unprejudiced mind that the disease which has lately spread such terror and destruction through Europe, is, in fact, as old as Aretæus. But, with some, there is a grand stumbling-block in the way to this conclusion—viz. that Aretæus, more than once, mentions bilious evacuations as occurring in cholera; therefore, say they, the cholera of Aretæus must be the cholera which occurs commonly in England, and not that which has lately appeared in Europe, and has been lately imported into this country.

For my own part, so far from viewing this as an objection to the conclusion I have endeavoured to support, I am inclined to view it as a proof that Aretæus was acquainted with both forms of the disease, and, like a skilful author, described, under one head, both the milder and severer symptoms.

I am convinced I am following the example of this admirable author, in concluding that the cholera which is familiar to us in this country is but a variety of the cholera known for many years in India, and more lately in Europe, as the cholera morbus—the spasmodic or malignant cholera. Such a conclusion may perhaps appear, to some, heterodox and unreasonable. What connexion, they may ask, can possibly exist between a case of the blue cholera, as lately seen in Europe, destroying life almost in a moment, and the mild cases we are accustomed to see in this country, and cure with a few doses of medicine? I would submit, however, that this is by no means a philosophical method of treating the subject; for there is hardly any disease in which the extremes do not widely differ from each other: as well may we ask, what possible similarity is there between the mildest and most malignant forms of small pox, scarlatina, or common fever? or, what is still more to the point, between the mild cases of ague, which yield to a few grains of quinine, and the yellow fever, so fatal in warm climates? In all these instances, we must trace the connexion through cases of an intermediate character; and the same, I conceive, must be done in cholera. I have no doubt that a very little investigation will be sufficient to furnish us with connecting links for our chain: nay, so insensible is the gradation of the severity of the symptoms, that we see the most skilful of those who believe in the distinct nature of the diseases, unable to agree to which class particular cases should belong. Take an example of this on a large scale from the reports at Sunderland: for a long time after the appearance of the severer form of the disease in that place, a distinction was made between the cases of common and those of the new or malignant cholera; but after a time, it was found that the distinction did not really exist, and that in fact the mild insensibly merged into the more aggravated form, and therefore the classification was abandoned, and all the cases reported under the general head cholera. Again: it is not long since two cases occurred at Portsmouth in men belonging to H. M. S. Revenge; the attack in each was sudden, the symptoms aggravated, and I

am informed the physician to the Hasler Hospital, an old Indian practitioner, declared positively that it was the disease he had been in the habit of treating in India; but as both the men recovered, and as half Portsmouth did not catch the disease and die, therefore it is determined generally that these were *only* cases of severe English cholera, and not the *new* disease. It would be needless to multiply cases where the severity of the symptoms has entitled the disease to the name of malignant or spasmodic cholera, which, however, has been denied them, because it was possible to discover some minor points of difference, and because the patients have recovered under proper treatment. I may refer to the case detailed by Sir M. Tierney in the Medical Gazette for August 27th, 1831, and that by Dr. Forrest in the number for August 13th; also a third in the number for July 23d. These cases all recovered, notwithstanding their aggravated character; but others have proved fatal before the introduction of the *new* disease, and especially I would refer to the very interesting and important account of a cholera which broke out amongst a school of boys at Clapham on the 14th August, 1829, detailed in the fourth volume of the Medical Gazette, page 375. It will there be seen that the disease, in a very aggravated form, attacked twenty individuals almost simultaneously; that it destroyed two of these in twenty-three and twenty-five hours; and that the rest were with great difficulty saved by stimuli. Surely we are entitled to place this case as an intermediate and connecting link between those just quoted and the true malignant disease as now seen at Sunderland and Newcastle.

To avoid lengthening this paper, I have omitted the mention of the various descriptions of cholera by different older authors in all its grades of severity. It will be seen, however, that if there be any truth in the observations with which I have troubled you, we may and ought to receive the description by Sydenham as of the present disease, somewhat modified, indeed, but still intrinsically the same.

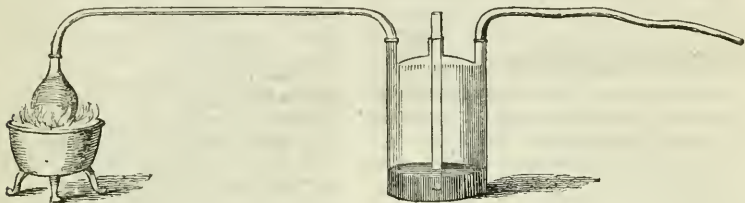
INHALATIONS IN CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

IN reference to a late communication, in which I pointed out the indications which called for the exhibition of mercury in the treatment of cholera, and in which I so strongly recommended it in practice, not upon speculative views alone, but from experience of its great benefit, I have to observe, that its occasional want of success I believe to be mainly or principally attributable to the insusceptibility of the system to its influence in the severe

asphyctic cases; or rather, I should have said, the difficulty or impossibility of introducing the remedy into the system through the ordinary channels of the stomach or skin. In such cases, I have elsewhere suggested the inhalation of mercurial vapour, experiments having proved that absorption goes on more actively from the lungs than from any other surface of the body. And here I would repeat the suggestion, and strongly recommend it to trial; with which intention I submit the following sketch of an apparatus which would appear to me well adapted to its production and administration:—



A drachm, or proper quantity of calomel, (or the nitric oxyde of mercury would perhaps be better, as its decomposition would be attended also with the production of oxygen gas,) being put into the iron bottle, and brought to a proper temperature by the fire in the chafing dish, its vapour would be conveyed by the connecting tube into the bottle, from which the patient could inhale it by the tube (which should be a flexible one) terminating in the latter. The centre tube arising from the bottle is for the admission of air, which should pass through mercury, sufficient being contained in the bottle for this purpose. It would be proper, I am of opinion, to immerse the bottle in a vessel of hot water, or heated sand, to prevent the condensation of the mercurial vapour. Could the patient respire a super-oxygenated atmosphere for some hours, I have no question but it would prove of much benefit, but should expect nothing from the inhalation of a few quarts, or even gallons of oxygen gas, administered during a short period. Should it be desirable to try it for a length of time, the above, I think, would prove a suitable apparatus for the purpose, substituting the black oxyde of manganese, or the nitrate of potass, for decomposition, instead of the mercurial. Or, perhaps,

what would be still better, the nitrate of ammonia might be subjected to the action of a spirit-lamp in a retort; the nitrous oxyde which would be thus evolved being more soluble in the blood, and therefore more efficient in its operation. I must, however, add, that no experiment of this kind should for one moment interfere with the employment of remedies of established repute, and of which I may mention calomel in particular.—I am, Mr. Editor,

Your obedient servant,
C. SEARLE.

Great Russel-Street, Jan. 10th.

MORTALITY OF CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

I SEND you, in conclusion, M. de Jönns' observations on the mortality from cholera in India, Western Asia, and Russia.—I am, sir,

Your most obedient servant,
C. W. CROWDY.

Jan. 17th, 1832.

1st. In Hindostan the number of infected persons, and the proportion of deaths, has varied considerably in dif-

ferent places, and at different irruptions of the disease.

2d. When the disease has been left to itself it has generally destroyed one-half of those attacked, or even two-thirds. It has been ascertained that, when met by medical treatment, the mortality is rarely one-third, sometimes one-fifth only of the sick.

3d. The population, taken in mass, gives the following proportions: one individual out of every ten has been attacked, and one out of every sixteen perished. This latter proportion raises to two millions and a half the annual mortality caused in India by the cholera.

4th. If we reduce this number one-half, on account of some intermissions, it may be calculated, that in fourteen years this pestilence has destroyed in India at least eighteen millions of inhabitants.

5th. In China its effects seem to have been still more destructive, doubtless from the greater density of population.

6th. In Arabia the mortality amounted, it is said, in the town of Muscat, and its suburbs, to one-third of the population.

7th. In Persia it was one-sixth, at Busheer, at Shiraz, and at Yerd; under a dry and pure atmosphere, and at a temperature of 36° centigrad.

8th. In Mesopotamia it amounted to a fourth, or even a third of the entire inhabitants, in the towns of Bassorah and Bagdad, which are situated on the Euphrates and Tigris, surrounded by an alluvial soil, and in an atmosphere saturated with humidity.

9th. It amounted to one-fifth of the population at Ereváu, and probably at Tauris, under a temperature of from 28 to 30°; but at Erzeroum and Kars, in the mountains of Armenia, it was considerably diminished.

10th. It has varied remarkably in the towns of Syria, although no adequate cause has been assigned for this, either from their situation, or any transient circumstances. It has not exceeded, on the whole, one-tenth of the population, but with so much diversity in the details that some places have lost one-half of their inhabitants, and others, for instance Tripoli, one only in 3000.

11th. This diversity cannot be attributed to any diminution of power in the contagious principle, since one-fourth of those who were attacked died in the

pachalik of Tripoli, and at Astrachan two-thirds.

12th. It seems rather to depend on a diminished facility for propagating the germ of the contagion in this part of the Levant, where the population is less numerous and condensed than in India, and the communications less frequent than in Mesopotamia and Persia, and where there have long been in use those sanitary precautions and remedies which the Franks oppose to the plague, and which alone can restrain the march of the cholera, or diminish its destructiveness.

13th. In all these countries the number of females who die from the disease scarcely amounts to a fourth of that of the men, which may be attributable to their constitution, their sedentary habits, and regimen.

14th. During the irruption of cholera in 1830, in Russia, the progress of the contagion, and the proportion of deaths, have differed with places and seasons. The southern provinces are those in which the malady spreads itself the most extensively and rapidly; and the towns which the infection did not reach till the end of autumn were those which suffered the least.

15th. At Teflis three-fourths of the sick died, and two-thirds at Astrachan and in the government of Caucasus. Almost everywhere the mortality has been one-half, and so low as one-fifth only among the wandering tribes, and in those places situated in the centre of the *Steppes*.

16th. The longest irruption lasted 114 days, the shortest 20. The latter arose in the end of autumn, the former in the summer.

17th. The greatest number of sick and of deaths occurred in the province of Caucasus: it has been computed that 16,000 persons were attacked by the disease, and that of these 10,000 perished.

18th. The numerical official returns which we have been able, up to the present time, to inspect, furnish when compared the following totals, which make up a minimum considerably within the truth. From the middle of June 1830, to the 15th of the following November, the public documents prove that there were 54,367 persons attacked by cholera, and that of these 31,236 perished.

19th. If we reckon the duration of

the irruption from the first appearance of the disease in the Russian territories until the period when it was stifled by the winter, we should compute it at 150 days, or 5 months; but we should extend it to 1071 days if we calculate its particular sojourn in each of the principal towns which it attacked.

20th. If we divide by this number that of the sick and of the deaths, we find that during a period equivalent to three years, there were 51 individuals attacked during every 24 hours by the contagion, and that of these 51 there died 30, or three-fifths.

21st. The numbers furnished by the official returns are certainly much below the truth, since, on the one hand, a multitude of cases must necessarily have been overlooked, and on the other, many, from various motives, have been concealed. One may admit, without any exaggeration, that during the irruption of the cholera in Russia, upwards of 100,000 persons were infected by the contagion, and that 60,000 perished.

22d. According to this computation, the sick have amounted to a 420th part of the entire population, and the deaths to a 700th. But the contagion having traversed half only of the provinces of the empire, we must conclude that it has attacked one individual in every 210, and destroyed 1 in every 350.

NATURE AND TREATMENT OF CHOLERA.

To the Editor of the London Medical Gazette.

Sir,

NOTWITHSTANDING the press has groaned for these last six months under the burthen of cholera, I hope you will excuse me sending a few remarks on the nature and treatment of this deadly malady. With regard to the nature of the disease, I think it is obvious, from the symptoms and pathology, that the organs of circulation and respiration, being either weakened or arrested by some *unknown cause*, the other symptoms follow as *effects*. This being the case, we should direct our sole and constant endeavours to resuscitate these organs, and thus control the morbid state of the natural functions. Now, in order to fulfil these intentions, it is our duty to compare those remedies which experience

has proved to be useful, with those which have been found either detrimental or inefficacious; and that, in acquiring this knowledge, we should examine those remedies used by practical physicians, and the results which followed their exhibition.

It appears, from the cases you have been kind enough to favour the public with, in a Supplement to the Medical Gazette of the 7th instant, and from various other accounts of the disease, that the most successful cures have been performed by those who have carefully and perseveringly used bleeding, emetics, fire, water, and external stimulants. I have not mentioned laudanum, because, with the exception of one case which you have recorded amongst the Sunderland reports, where bleeding had been previously used, and from other accounts*, this medicine seems to have been of no avail, and, as we know that opium, in whatever dose it may be exhibited, always ultimately depresses the vital powers, so I conceive, that in large doses, when these powers are collapsed, it may be positively injurious. But there are some who will say, do not the symptoms warrant us in giving antispasmodics and sedatives to control the morbid action of the stomach and bowels?—which question I answer in the negative; for I conceive that the vomiting† and purging (when the powers of life are not so far weakened as to create these efforts) being the effects of the congested state of the blood and diminished power of the nervous system, nature takes these methods of re-

* With regard to internal treatment, a great number of medical men declared themselves decidedly against opium in every form and stage of the disease. Some preferred large doses, given seldom, to small ones frequently repeated, and *vice versa*. The general impression was, that the invasion of the typhus or congested stage was accelerated by this drug. The great cause of the use and abuse of opium appears to have been the wish to stop the vomiting and diarrhoea, and moderate the spasms. All these symptoms, however, are by no means essential to the disease, *which has often proved fatal without any of them*. Extract from a letter of Sir W. Crichton.

† Vomiting is the means which nature takes of relieving herself of many agents which affect the mucous coat of the stomach and air-passages, consequently which act on parts supplied with nervous power by the eighth pair. It is a primary symptom in small-pox, measles, distemper, &c. and seems to have the double effect of relieving the parts affected, and of bringing out the eruption; for it is well known, that when the eruption in small-pox and measles declines, or is suddenly checked, or the discharge which flows from the mucous surfaces of distempered dogs dries up, symptoms of congestion follow, and death too often closes the scene.

lieving the vessels of the stomach and of easing the loaded system of the vena porta, by pouring out the *serous portion* of the blood. As no two cases of cholera asphyxia are exactly similar, so it follows as a consequence that the treatment must vary according to the circumstances of the case, although the general proposed plan must be pursued. I think, that in every case we should endeavour to abstract blood, till some effect is produced on the circulation, contemporaneously administering emetics, which, by exciting the pneumogastric and phrenic nerves, may rouse the heart into action, and thus relieve the turgid state of the internal vessels. Drs. Strebel and Lindsey have both succeeded in causing reaction by emetics: it appears, however, by the cases recorded in your Gazette, and from other statements, that patients have finally become victims to the disease either from the subsequent debility or from some sedative power continuing to act on the system. I therefore venture to propose an emetic which I am not aware has been given by any individual in this disease, possessing powerful stimulating properties, and which may have the effect of subsequently modifying or destroying the influence of any sedative poison which may still remain in the system. The emetic I recommend, I have frequently given to dogs labouring under the distemper; and I have seen the poor brutes, with their whole system so weakened as to be unable to stand, after having taken this emetic, gradually recover.

The yellow subsulphate of mercury*, the proposed remedy, may be given to an adult human being in mucilage or treacle (I give it to dogs in butter) in the dose of grs. vj. repeated till it causes full vomiting; subsequently in small doses every half hour, so as to maintain an alterative and stimulant action in the system, heated salt and mustard poultices being applied externally at the same time. If by the united means of emetics and bleeding, reaction has been fairly established, and the pulse continues pretty firm; should the vomiting and purging not have subsided, laudanum and castor oil, given as in cases of common cholera, will most likely prove beneficial. If the stage of

collapse is followed by fever, carbonate of ammonia and quinine may be given in small repeated doses, and wine at intervals. There is another remedy in treating this disease, which from its great efficacy in arousing the dormant vital powers, and of which I have myself been eye-witness, I shall now mention. I mean cold water *dashed* on the head, as recommended by Mr. Smith*, and I think we are farther warranted in using this agent from the favourable accounts given of it by Dr. Seidlitz, who always succeeded in causing re-action by directing a *powerful stream* of cold water on the spine. I am aware that in the most severe forms of the disease, heated irons and moxa have been applied to the spine with great success by Dr. Lange, but as I would far sooner die than suffer this treatment, I could not conscientiously recommend this as a general plan. As a dernier resort, galvanism applied to the exposed eighth pair, as advised by Dr. Ure, would certainly be less painful, and probably still more efficacious than the actual cautery.

I shall leave it entirely to your better judgment to insert or not these remarks in your impartial and extensively-circulated Gazette, and I remain, Sir,

Your obedient servant,

JOHN KNAPP, M.D.

Duncan Street, Edinburgh.

N.B.—The cholera asphyxia has not yet made its appearance in Edinburgh.

I had an idea that phosphorus might, from its supposed stimulating powers, be useful in cholera. I took, by way of trial, one grain, dissolved in oil, for a dose; it produced no effect on the pulse whatever, neither did it affect the urine; but it caused frequent eructations of what I suppose was phosphuretted hydrogen gas, though I was not daring enough to ascertain this point by means of lighted taper.

* I am happy to confirm Mr. Smith's account of the efficacy of dashing cold water on the head in cases of simple apoplexy and drunken coma. On New Year's Day, 1831, I was called to see a drunken female, with a pulse scarcely to be felt, insensible on being pinched, closed jaws, and purple face. She had taken five or six glasses of undiluted whiskey, walked home, and fell in the kitchen as if struck by lightning. I had no stomach-pump at hand, so I opened a vein, and then dashed several pitchers of cold water on her head with *great violence*; she heaved a sigh, the blood began to flow, and she gradually recovered. A man fell down in the street in a fit of apoplexy: I had him conveyed to a house, and bled him to a pint and a half; the stupor continuing, I used cold water as in the above case, which aroused him.

* I strongly recommend this medicine in hydrophobia.

LIABILITY TO CHOLERA LESSENE BY HAVING HAD THE DISEASE.

To the Editor of the Medical Gazette.

SIR,

AMONG the arguments that have been adduced by the non-contagionists, to prove that the cholera is not an infectious disease, peculiar stress has been laid on the one, that Mr. Searle failed to be affected, though exposed, under every degree of exhaustion and fatigue, to the breath and effluvia of patients labouring under the disease in his hospital at Warsaw—having even laid himself down to rest on the beds of those who had just died of it.

Now, sir, it is, I believe, admitted by most, if not all, authorities on the subject, that second and third attacks of cholera in the same individual are exceedingly rare; it would, I think, therefore, be somewhat interesting to learn whether or not Mr. Searle has had the disease already in India, as in that case his exemption at Warsaw, under otherwise favourable circumstances, will be explained under the acknowledged laws that seem to regulate this very singular malady.

Will you permit me, sir, through the medium of your journal, to solicit from Mr. Searle an answer to the inquiry that is contained in the preceding sentence?

Your obedient servant,

ALEX. TWEEDIE.

Borough, Dec. 20, 1831.

[We are able to answer Mr. Tweedie's question by informing him that Mr. Searle did suffer an attack of Cholera in India, though probably little stress can be laid on the circumstance, considering the great number of medical men who have been exposed with impunity, although they had not previously had the disease.—Ed. Gaz.]

QUARANTINE IN CHOLERA.

THE following document, addressed by the Central Board of Health to the Privy Council, has been published by authority in the Cholera Gazette:—

Central Board of Health, Council Office,
Whitehall, 4th January, 1832.

Reasons founded on authentic facts in the History of Spasmodic Cholera, for Establishing a Specific Code of Sanitary Restrictions for that Disease, considered independently of Plague, Yellow Fever, and other Infectious Maladies.

1. If the sole object of sanitary police were to protect communities, at all risks, from be-

ing infected by their neighbours, medical science need not be consulted; as an absolute cessation of intercourse with the suspected would be the only measure necessary.

2. But as such a measure would be nearly, if not altogether, impracticable, under the present circumstances of society, it is essential to endeavour to determine the point of time at which the danger of infection by any particular disease ceases, and consequently the period at which free intercourse may be resumed with those who have been thought capable of communicating that disease to others.

3. When sanitary police was first established, spasmodic cholera was unknown, and medical science in Europe was but little advanced. The very word *quarantine*, and the 40 days' restraint which it indicates, do not seem to have been derived from a well-authenticated knowledge of the individual sanitary histories of the diseases against which the restriction was first directed, and has since been kept up, partly from ill-defined apprehension, and partly from reverence for old institutions. It must, however, be allowed, that a much longer separation from suspected persons is called for in diseases in which infective matter is proved to be generated, and thrown out upon the surface of the body, and upon the clothes, as in plague and small-pox, than where no such matter is generated.

4. Yet it is certain, that even in these last diseases the maximum of the period of incubation, or interval between the reception into the system, of the infective germ, and the manifestation of the symptoms, does not exceed one-third of the quarantine founded on the doctrines of Fracastorius, and the sanitary laws of the 16th century.

5. The utmost length of time during which the safety of the public health absolutely requires sanitary precautions as to persons, or effects, supposed capable of communicating the infective germs of any given disease, ought to be determined by what experience in that disease may have established on the following questions, viz.:—

First. What is the longest interval of time between the reception of the infective germ into the constitution, and the manifestation of the first symptoms of the disease?

Second. The period during which an individual may retain the power of infecting others with the disease from which he is himself convalescent?

Lastly. The capability of certain classes of merchandize to retain, and afterwards to communicate, the germs of the malady?

6. Numerous and authentic data, tending to elucidate these three questions, so far as they regard spasmodic cholera, have been furnished by the extensive prevalence of that disease, since 1817, in our East India possessions; by our commercial intercourse

with Russia, Prussia, and other infected countries on the continent of Europe; by the recent investigations of the medical commissioners sent to these countries from different governments; and finally, by the laborious and accurate observations of the most enlightened physicians of the countries where the disease has prevailed, or is now prevailing.

FIRST QUESTION.—Period of Incubation.

7. The following are a few of the many facts which seem to settle this period with tolerable accuracy:—

“The subsidiary force under Colonel Adams, which arrived in *perfect health* in the neighbourhood of a village in India, infected with cholera, had 70 cases of the disease the night of its arrival, and 20 deaths the next day.”

8. “His Majesty’s 54th regiment landed at Madras on the 10th of May, in a remarkably healthy state, after a voyage of forty-eight days from the Cape of Good Hope, and marched into quarters in Fort St. George. Cholera appeared amongst the men within three days after their landing.”

9. “In eighteen vessels which arrived in England, between 26th May and 24th September, from infected ports in the Baltic (each vessel having had one or more cases of cholera on the passage), the greater number of attacks took place previously to the *fourth*, and only one attack so late as the *sixth* day of sailing.”

10. Dr. Becker, of Berlin, gives the subjoined statement in his Report:—

“From August 29th to September 26th, there have been reported cases of cholera in Berlin, 770.

“During that period, a second case has happened in the same house where one case had been reported.

“After one day, 65 times; two days, 34; three days, 23; four days, 16; five days, 21; six days, 7; seven days, 3; eight days, 2; nine days, 0.”

11. The British Medical Commission lately returned from St. Petersburg, after detailing a series of cases upon this point, concludes thus:—

“That in the above cases, in all of which the time intervening between an only exposure to infection and the subsequent development of the disease was most accurately marked, the period of incubation ranged between one and five days.”

12. The Genoese Medical Commission sent to Hungary and Vienna, to study the nature and history of spasmodic cholera, state in two distinct reports to the Sardinian government, their decided conviction, derived from protracted observation and personal experience in cholera quarantine establishments, that those who have absorbed the germs of the disease, are generally at-

tacked before the *third*, and always before the *fourth* day.

13. The Board are aware that accounts have emanated from respectable sources, of persons having been seized with cholera many days after their departure from infected places; but as the history of these individuals during the interval between the supposed last exposure to infection, and their subsequent attack, does not appear to have been accurately noted, and as even these instances are very rare, the Board would not feel themselves justified in allowing their opinions to be influenced by such insulated statements.

14. It appears, then, with regard to the first question, to be clearly established, that the longest interval between a well authenticated latest, or only, exposure to the infection of spasmodic cholera, and the subsequent manifestation of that disease in a susceptible person, has been from *five to six days*.

SECOND QUESTION.—Length of Time during which persons convalescent from Cholera may retain the capability of infecting others.

15. In a sanitary point of view, persons recovering from any disease, cannot be contemplated independently of their clothes, bedding, and other susceptible personal effects.

Whenever persons unaffected themselves with cholera (whether convalescent or not) have been conductors of the germs of the disease from the sick to the healthy, the latter have always been attacked within the period of incubation already specified.

16. The Genoese physicians already quoted, state that in the cholera quarantine establishments, which they had such ample opportunities of observing, no one was ever attacked after the sixth day.

17. No ship has ever arrived in this country from India, since the first appearance of the disease there, nor within several thousand miles of our shores, with the disease or its germs on board, although 103,376 bales of cotton wool have been imported from that country within the last three years.

18. No individual has ever been attacked on board ship south of the Baltic, on the passage home, nor in any of the quarantine establishments in England, since cholera first broke out on the shores of that sea.

19. But as a single well-authenticated instance of cholera having been communicated to a healthy community, by persons recently recovered from the disease, or by their effects, would be enough to demand quarantine precaution to the amount, at least, of something above the longest interval between the recovery of the one and the first appearance of the disease amongst the other party; and as there is reason to believe that the first case of declared cholera at the Mau-

ritius, in 1819, did not occur before the 15th day from the arrival at that island, on the 29th October, of the "Topaze" frigate, and the landing of her sick, after having had several cases of cholera on the voyage from Trincomalee, which place she quitted on the 9th of the same month*; and as the data which tend to determine the period indicated in the second, are by no means so numerous nor so precise as those which bear upon the first and third questions; and, as that period must be considerably modified by the conditions under which the recovering or recovered person or persons are placed with reference to cleanliness, ventilation, food, &c.; and as precaution naturally increases with undefined apprehension; the Board, until more precise facts on this question can be obtained, must consider persons ascertained to be but just convalescent from cholera, as coming under the most aggravated circumstances of a foul bill of health.

THIRD QUESTION.—*Capability of Merchandize to convey, and afterwards communicate, the infectious germ of Cholera.*

20. There is perhaps no question in the whole range of sanitary police, on which so many and such irrefragable facts can be brought to bear as on this; derived, too, from the most authentic and recent sources.

Seven hundred and thirty ships loaded with hemp and flax from infected parts of the Baltic, arrived at the different quarantine stations in this country between the 1st of June and the 31st of December, 1831.

Many vessels also arrived laden with wool and hides, yet not a single case of cholera occurred on board any of these ships outside the Cattegat Sea, nor amongst the people employed in opening and airing their cargoes in the lazarets.

21. At the hemp and flax wharfs in St. Petersburg, where several thousand tons of these articles arrived during the spring and summer of this year, from places in the interior, where cholera existed at the time of their departure for the capital, the persons employed in bracking or sorting, and who generally passed the night among the bales, did not suffer so early in the season, nor so severely, as other classes of the general population.

The same observation holds good with respect to all the rope-walks of St. Petersburg, and the imperial manufactory of linen cloth at Alexandrofsky, where all the yarn is spun from flax bracked and hackled on the spot.

22. Struck with the importance of the above, and other similar and authentic facts connected with the sanitary history of cholera;

Holding in view also the unnecessary embarrassments to every kind of intercourse caused by the adoption of plague precautions against individuals, communities, and merchandize, affected with or suspected of cholera only;

The strong inducements to elude sanitary restrictions furnished by their own severity;

The inefficiency of cordons by land, from the impossibility of their being made perfect, except by a system of coercion, entailing greater evils than the disease itself;

The panic, and other dangerous moral, as well as physical effects, caused by vexatious insulations of families and communities;—

Some of the most commercial nations of Europe, as well those still exempt from, as those already infected by spasmodic cholera, have lately reduced, very considerably indeed, the quarantine restrictions which they had hitherto directed against that particular disease.

23. Lubeck, a territory perfectly exempt from cholera, reduced its quarantine upon persons and merchandize arriving from infected ports, first from forty-one to twenty-one, and then to ten days.

24. A similar reduction has taken place at Copenhagen.

25. In Prussia, persons and merchandize from infected places are subjected to a detention of only five days.

26. The Board of Health at Genoa have modified their quarantine code, taking as the basis of their new arrangements the maximum of the period of incubation of cholera, as determined by their own medical commission already quoted.

27. Guided by what experience has already established as to the laws which seem to regulate the propagation of cholera, and having in view the enlightened decisions which the sanitary authorities of other countries have come to on this subject, the Central Board of Health feel themselves justified in giving it as their opinion:—

1st. That the maximum of sanitary restriction, or quarantine of observation, for an individual in health, but suspected of carrying the infective germs of spasmodic cholera, as yet latent in his organization, need not exceed ten days.

2d. That the period of separation from the healthy, of an individual ascertained to be but just convalescent from cholera, need not exceed twenty days.

3d. That ordinary diarrhoea, continuing one or more days, being often the first symptom of cholera; persons arriving from infected places, labouring under even the mildest degree of purging, should not be admitted to free pratique before the eighth day after perfect recovery from the same.

4th. That the clothes, bedding, effects, and sleeping places of all persons on board vessels from infected ports, ought to be opened, aired, and purified, during three

* See the journal of the surgeon of the ship (Mr. Foy), Medical Gazette, November 19, 1831, p. 226.

days after their arrival, although the length of the voyage may have exceeded the period of quarantine adjudged in such cases to healthy ships and unsusceptible cargoes.

5th. That the longest period of detention for airing and purifying merchandize of the most susceptible class, and arriving under the most suspicious circumstances, need not exceed fifteen days, to be counted from the day on which the airing may *bonâ fide* have commenced.

Finally. The Board see no reason to believe that the above suggestions, directed against spasmodic cholera alone, require any modification in reference to climate.

(Signed by the Members of the Board.)

MEDICAL GAZETTE.

Saturday, January 21, 1832.

“*Licet omnibus, licet etiam mihi, dignitatem Artis Medice tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso.*”—CICERO.

PROGRESS OF OPINION RESPECTING ANATOMY.

THAT old avowed enemy of the medical art—Cobbett—the railer at every thing that is right and rational—the political Proteus of his day—has been latterly once more meddling with the anatomical question, and throwing out certain extravagantly foolish dogmas on the subject not wholly peculiar to himself. He gloats over the havoc lately achieved at Aberdeen—he knew it would come to that; and to *that* it ought everywhere to come. Down with the dead-body-butchers! The people of Aberdeen are a brave people, and they have redeemed—in *his* eyes—the character of the whole Scottish nation: their example should be followed throughout the country, and those Philistines and vampyres, the anatomists, should have their houses pulled down about their ears, and themselves sent adrift with the disgrace and defeat which they deserve. Such is the strain of elegant invective

which adorns the pages of the last Register—such the wisdom of the exhumator of Payne. But what do we see in another page of this same work? With all delicacy be it spoken, the amiable author tells us that he has “a rupture”—a rupture of we know not how many years standing—in which he found a certain sort of remedy beneficial—one that he can recommend to his readers, and thinks it his duty to recommend, influenced solely by his tender love towards his neighbour. The quackeries of John Wesley are well-known—a dabbler, not ill-disposed, we believe, to physic—but charitably doling out certain nostrums for the sake of the poor. But here is Cobbett baying at the profession—preaching its very extermination—and with the same breath, confessing to a rupture under which he labours, and recommending a nostrum to “the afflicted!”

We have been forcibly struck with this fact—which we probably should not otherwise have deemed worthy of notice—in consequence of meeting with the following passage in a document to which we have often had occasion to refer. The words are those of the late Mr. Abernethy, who seems to rise from his grave to give a wholesome warning to the “reviler:” we know not, indeed, but the very individual in question may have been in the “mind’s eye” of that shrewd surgeon when he was uttering what follows:—

“A man, having that common infirmity, a rupture, may revile those who dissect the dead body; but when the protruded bowel shall be strangulated, his rupture, if left to itself, must bring him to a certain and most painful death; yet he might be relieved from agony and destruction by a single and secure operation, when performed by a person conversant with anatomy, though dangerous in the extreme when attempted by

hands not sufficiently practised in dissection.*" Cobbett may have been the patient of Mr. Abernethy, for aught we know: at all events, the coincidence is very remarkable: and we should be happy were we, in thus bringing before our respectable opponent so seasonable a hint, the humble instruments of averting an irreparable calamity from the political world.

But what though we were to conciliate the favour of one antagonist by a practical proof of the excellence of our art—should we then have peace, and no more impediments thrown in the way of science? There are, we are ashamed to say, enemies in our very camp—betrayers within the gates: in short, there are those who seek their reward in stirring up the passions, and pandering to the already but too inveterate prejudices of the people. We shall more plainly speak to one example of this sort which has come to our cognizance. At the Mechanics' Institute of this great metropolis, in presence of a large audience—composed as it is known the audiences in those places usually are—it is not long since a well-known knight famous for his popular views in medicine, and his patronage of the practice of elderly personages of the other sex, volunteered to hold forth on the subject of anatomy. We only regret that we were not apprised in time of the considerate and erudite knight's intentions; for we should most assuredly have done ourselves the honor of being present by proxy, and have enriched our pages with a report of his pregnant oration—and probably have added a few words of comment of our own; as it is, we can only say, on the authority of informants on whom we can place every reliance, that a more mischievous, absurd, and unworthy diatribe about dissection never before

issued from the lips of any medical man. We hope the enlightened author will have the courage to publish the production; nay, it should be remembered that if it was suited to the purposes of the audience to which it was addressed, it ought to form a valuable tract for the Society for the Diffusion of Useful Knowledge. This hint, we trust, will be attended to.

It is certainly one of the most singular facts with which we are acquainted, that with the medical history of the last thirty or forty years before their eyes, *professional* men can be found weak or wicked enough to advocate the not-absolutely-indispensable nature of anatomy, and to attempt to suggest expedients for its possible abolition. We will not show ourselves equally weak as they by proceeding to reason such creatures out of their ill-gotten notions: we leave it to the time-serving flexibility of their nature to reconcile them speedily to other views hereafter to be adopted in accordance with public opinion. But we will waste a word or two on some of those silly people, who, in newspapers and other ephemeral vehicles of "instruction," write and talk about the possibility of *putting down anatomy* altogether; we would have them understand that they might as well speak of altering the abstract physical nature of man, (if they can comprehend what that is): the pursuit of the grand catholicon would be an infinitely more rational project: when they have discovered the elixir of life, and rendered man unsusceptible of decay, and proof against the casualties and corrosions of time, then may they securely dispense with the practice of dissection;—*but* not till then. "Dissection," as it is well expressed by an able writer, "*must* be practised: the wants of the community imperiously demand it, and always *will* demand it. Society can no more go on without the

* Report of the Committee of Anatomy, 1828. Appendix.

supply of subjects for the anatomist, than it can go on without the supply of corn for food,—because people will no more lie down and die quietly under the influence of diseases which are remediable by surgery, than they will sit down quietly and starve*.”

This, however, we are glad to see, begins latterly to be pretty widely acknowledged: conversions have been effected in influential quarters: the press, with one or two worthless exceptions, is pleased to recognize the necessity of legislation on the subject; but not till most of our continental contemporaries have actually had the start of us in *perfecting* their anatomical arrangements. If, as it has been said, the state of the medical art in any country be the test of that country's civilization—the art being the main source of the health, comfort, and enjoyment of mankind—it must be confessed that this native land of ours is considerably savage. In Italy, in France, Austria, Germany, and even in America, fitting measures have been adopted for the supply of the schools; while here *we* are still arguing, and endeavouring to remove prejudices, and proposing bit-by-bit improvements, goaded even to the consideration of these by the occurrence of crimes amongst us, such as no other country in the world ever presented an example of. The excellent manner in which they order these things in France, we have had occasion very frequently to notice; and incidentally the arrangements of the Italian and German schools have been made the subject of our applause. In another page will be seen the most recent legislative measure, in behalf of anatomy, which has been enacted in the United States: it is the deliberate and well-advised provision of the Massachusetts Assembly, to secure to the profession those helps which are so ut-

terly indispensable wherever the study of medicine is cultivated; and it is founded upon the labours of an enlightened Committee, who, in addition to the sources of information which their own country afforded, availed themselves of the rich materials supplied by the research of *our* parliamentary investigators. What a contrast does this present to the whole course of proceeding which has been pursued in this country! After a lapse of four years—during which the most valuable and satisfactory evidence on the subject of the importance of anatomy ever collected, has been lying before our legislators—*nothing has been done*; and even now, as we write, the intelligence reaches us that an abortive attempt has just been made to procure a second reading for the Bill! We have no hesitation in saying, that this mode of conducting *the business of the people* reflects disgrace upon the whole country. There may be, and we have little doubt there is, much remissness on the part of the managers of this ill-fated Bill; but most assuredly it is scandalous, that, upon the motion for discussing a *national measure*, not only do none of the ministerial members (with the exception of one of the law officers of the crown) attend to afford it their countenance, if not their support; but all the members of any influence absent themselves, or slink away—and when it comes to be considered whether the measure is to stand or fall, it falls—because there is *no House!* In the brief notice which was taken of the subject while it was passing rapidly before them, there were some remarks made by honourable members, on which we could not unwillingly make a few comments, were it worth the while. One gentleman thought that practical anatomy should only be studied in time of *war*, and that *the field of battle* should be the theatre! And another sage senator announced it

* Westminster Review, vol. x.

as his opinion, that, in order to put down burking, dissection should be made *felony*; at least, that it should be made so for *two years*, during which time the practice of anatomy should cease altogether—and, we suppose, the practice of burking be altogether *forgotten*! Such is the sample of the legislative wisdom displayed on this occasion. Hear it not in Massachusetts, nor be it told in the streets of Boston! It is curious, though not less true, that had there been but a few more of those enlightened senators present, the Bill would most infallibly have been thrown out—for *this year* at least. It remains, however, luckily *in statu quo*; and it may not be too late to offer a few additional remarks upon its provisions.

A correspondent, of whose legal attainments we entertain a high opinion, has sent us some practical observations on the subject, which we think eminently deserving notice. "All that the proposed bill attempts," says this gentleman, "as a provision for the supply of subjects, is to give liberty to certain persons to deliver over to the schools such bodies as they may by law have the custody of; and this liberty is accompanied by a restriction of such a nature as will most assuredly give rise to codicils to wills already made, and a declaratory clause in every will hereafter to be made, to be called by lawyers, technically, 'the anatomical clause.' False swearing will be another consequence, on the part of the friends and relatives of the deceased—which also will, most probably, be called 'anatomical or justifiable perjury.' Nor will this false swearing create any moral revulsion in the prejudiced state of mind of the people of this kingdom. Another point in the proposed bill on which I would offer a remark is, the total omission of all reference to the mode in which registries of deaths are to be kept, in the event of

this bill passing into a law. The *legal* proof of a man's death in this country is the certificate of his *burial*; and on the existence of this proof the rights to property mainly depend. Here, then, is an important omission; but it might, I think, be easily remedied in this way:—the law of the land at present is, that no person shall be buried until the cause of death be known; and for the ascertainment of this fact, parish searchers attend, to examine the body of the deceased. Now there should be an act procured, rendering it imperative on the churchwardens, in future, to register the *deaths* instead of the *burials*; and this they could readily do through the agency of the searchers. It should be no longer necessary, in proof of a man's death, to prove that he has been buried; and I would just beg leave to add one observation more: I am firmly persuaded that, if the law simply took no cognizance of the disposal of the body, farther than requiring anatomists, under a penalty, not to receive any dead body unaccompanied by a certificate of the registry of the death, signed by the clergyman and churchwardens, much of the present solicitude about legislation for the supply of the anatomical schools would be rendered superfluous, and burking would be altogether suppressed."

There is much reason in this last remark. If dissection were simply legalized, and the stigma of the *punishment* of dissection taken away, and anatomists left to shift for themselves—to get bodies in the best way they could, always taking care that they had the requisite certificate of the death, we have not a doubt but that they would be far better provided for than by the very questionable supply expected from the proposed bill. It is, indeed, the prime objection to the bill recently laid before Parliament, that it pretends to afford a source of supply, while in reality it only points out a source of se-

rious vexation and disappointment; on the one hand, owing to the want of regulation by licences, the teachers of anatomy will be exposed to extravagant charges for subjects set up to the highest bidder, while, on the other, they may be totally deprived of all chance of a supply, by the ready interposition of "one or more witnesses;" for a single witness, be it observed, deposing that the deceased had, on some occasion during his life-time, expressed a desire not to be "examined," can overturn the claims of one and all, the anatomist, administrator, executor, and party legally possessed of the said body. And the assigned source of supply being thus so very limited and precarious, the natural consequence, we fear, will be, a recurrence to the old system of exhumation, if not to its diabolical counterpart—burking.

As far as we can judge, the simple suggestion which has just been offered would seem most likely to answer every end in view. But it is undoubtedly difficult, in the business of legislation, to please all parties, where so many have an interest to promote, and so many entertain notions of their own capacity to legislate. Among the number of documents with which it was our lot to be favoured since we first published an abstract of the bill, but of which we could not avail ourselves for want of room, we observe one which, for its singularity, we cannot pass over, in conclusion, without some sort of notice or remark. We allude to the printed paper of Mr. Stephen, recommending a *mortuary fund*—in other words, a tax to be levied on every individual who dies throughout the kingdom; this mortuary fund to be given in trust to the College of Surgeons, to be by them divided in premiums to persons bequeathing themselves for dissection!—but we suppose most of our readers have seen this curious Circular—we need not give

more of it. What strikes us as the most original part of the plan is, the setting a tax upon dying. An Edinburgh reviewer, we recollect, taxed his ingenuity in drawing up a well-known catalogue of taxes—taxes which pursue us from the cradle to the grave, "where we are taxed no more." Mr. Stephen manifestly improves upon him, and gives us a *coup de grace*.

DUKE OF BOURBON.

THE death of the last representative of the house of Condé continues to be involved in mystery. None of the explanations offered are satisfactory, and the medical jurist has hitherto been baffled in all his attempts to reconcile the circumstances to the idea either of murder or suicide. Another view of the case has been suggested to us, to which, indeed, for obvious reasons, we can do little more than allude; yet the mere allusion will be acknowledged to be of much importance, as a caution against hasty decisions under analogous circumstances. It is more than possible that the deceased neither was assassinated nor destroyed himself; but that he met his death in consequence of having voluntarily put himself in the situation in which he was found, insensibility having stolen upon him unawares, thus depriving him of the consciousness essential to his safety. One of the effects of hanging, the physiological reader will call to mind, is of a nature to have led to well-authenticated instances of exhausted voluptuaries endeavouring to imitate it by temporary suspension. In a case which occurred at no very distant period, an Italian *singer* used to hang himself up by the neck for the purpose alluded to, with injunctions to his landlady, to whom he always gave notice of his intention, to take him down after a certain very limited time. On one occasion, how-

ever, some one having knocked at the door, she thought she might answer the call before releasing her lodger from his voluntary elevation: when she returned he was dead. M. Kotzwarra, the composer of the well-known piece of music, "The Battle of Prague," is said to have lost his life from a similar practice, and in a somewhat similar manner; nor, incredible as it may at first sight appear, is such practice so uncommon, more especially in some parts of the continent, as to render its existence matter of doubt. But insensibility occasionally takes the parties so entirely by surprise, that they are unable to extricate themselves; and thus in the case from Lord Bacon, alluded to by Dr. Watson, of King's College, in his observations on the body of Bishop, it is stated that an acquaintance of his having hung himself up in sport, became so speedily unconscious, that he would have perished had he not been cut down. Now, if we suppose that the Duke of Bourbon—either in consequence of being, as Bacon terms it, "*ludibundus*," or from any other kind of wantonness which the reader's imagination may suggest—had purposefully fastened the noose with the intention of undoing it when his experiment was at an end, all the contradictions and apparent inconsistencies of the case will be at once reconciled.

SUPPOSED CASES OF CHOLERA IN LONDON.

THE case of Webb, the soldier, has excited a good deal of discussion, and a considerable share of apprehension in the public mind. We shall content ourselves with stating that the symptoms were extremely suspicious; but that they were speedily overcome by means of bleeding, a mustard emetic, external heat, and frictions. With regard to the sailor at Shadwell, whose case appeared in the papers of yesterday, it is sufficient to remark, that if the account given by the medical men be correct, the disease of which he died was certainly *not* cholera.

ON SOME MORBID APPEARANCES OF THE ABSORBENT GLANDS AND SPLEEN.

BY DR. HODGKIN.

[THE following is the abstract of this valuable paper, which we promised in our last report of the Medico-Chirurgical Society.]

If there be any accounts in books relative to morbid alterations of structure like those about to be described, they can be but little known; the author knows of none, and there is the more room and apology for the present observations.

1. The first case deserving of notice is that of Joseph Sinnott, nine years of age, a patient in Guy's Hospital, admitted labouring under ascites and effusion into the prepuce and scrotum.

Inspection.—Serous effusion under the arachnoid and within the ventricles. Substance of the brain soft and flabby. Pleura much affected with adhesions; fluid in the cavity. A few tubercles in the lungs. Heart healthy. Peritoneum had been recently and extensively inflamed: a sero-purulent effusion in the cavity. Viscera overlaid generally with a soft light-yellow coagulum. The mesenteric glands enlarged—one or two considerably so, equalling in size a pigeon's egg, of semicartilaginous hardness, and streaked with black matter. Liver pretty natural. Spleen large, and containing numerous tubercles. Kidneys mottled, of a light colour. A continuous chain of much-enlarged, indurated, absorbent glands, accompanied the aorta throughout its course, closely adherent to the bodies of the vertebrae, and extending along the sides of the iliac vessels, so far as they could be traced in the pelvis; none of these vessels had been sufficiently compressed to occasion a coagulation of the contained fluids.

2. The next case is that of Ellenborough King, aged ten years, a patient of Dr. Bright's. Until thirteen months ago this child had been healthy; a tumor was then observed in the left hypochondrium, which, under treatment, was very considerably reduced; the glands on both sides of the neck were swollen, the abdomen was somewhat distended, and there was considerable œdema of the scrotum.

Inspection.—The glands in the neck, when cut into, exhibited a firm cartilaginous structure, without any appearance of softening or suppuration. The glands of the vessels in the chest were in the same state. The lungs generally healthy; there was a considerable quantity of fluid in the peritoneal cavity. The glands accompanying the aorta, the splenic artery, and the iliacs, were as those of the neck. The spleen was enlarged to four times its natural size, studded with tubercles, and presenting the same structure as the glands just mentioned.

3. William Burrows, aged 30, admitted for scrofulous ulcer in the axilla and neck; had been previously in the hospital for secondary symptoms, which were supposed to have been treated with large quantities of mercury. Before he died, abdominal dropsy had made its appearance.

Inspection.—The cavity of the chest contained about a pint of serum. Heart small and flabby; the liver of a shrunken, irregular shape, indurated, pale, and pervaded with a substance of a white, hard, tuberculous character. It appeared to Dr. Hodgkin, that the liver was in that state which is almost peculiar to those who have laboured under a cachectic condition from mercury. The spleen twice its usual size, unusually firm, and, when cut into, exhibited a dense, dark-red, homogeneous structure. Some of the mesenteric glands much enlarged, and filled with white deposit; almost all the other glands of the trunk were similarly affected.

4. Thomas Westcott, aged 50, presented a great enlargement of nearly all the absorbent glands within reach of examination, but more especially of those in the axilla and groins.

Inspection.—The glands of the axilla proved the larger the deeper they were seated; when cut into, they appeared of a light white colour, with very few vessels; in consistence, they much resembled fungoid tumors. The glands in the groin of a similar character; the alteration in this case seeming to consist rather in an interstitial deposit, from a morbid hypertrophy of the glandular structure itself, than on a new or adventitious growth. The glands about the abdominal aorta and iliac arteries were as those in the preceding cases. Liver very large. Spleen at least nine inches long, five broad, and proportionately thick; its structure consisting of an almost infinite number of small white globules, appearing to arise from deposits in the cellular tissue of the organ.

5. In the next case, which was that of a middle-aged man, the last urgent symptoms were referable to the chest.

Inspection.—The glands of the neck, and those near the upper part of the thyroid cartilage, were very much enlarged. The lungs exhibited traces of recent inflammation. A large quantity of serum in the abdomen. The liver remarkably large, weighing upwards of seven pounds; the acini somewhat enlarged, but not fatty, as it was at first supposed. Spleen four or five times its natural size, but it contained no tubercles; the cellular tissue, however, was more conspicuous than usual in the intestines of the parenchyma. The absorbent glands about the aorta were all enlarged; some of them the size of a pullet's egg.

6. Thomas Black, aged 50, was affected with swellings of the neck and axilla; his abdomen was greatly distended, and his

breathing difficult. On examining his body, in both chest and abdomen the absorbent glands about the great vessels were enormously enlarged.—[Preparations exhibited.]

The enlargement of the glands, both in this and the preceding cases, appears to have been a primitive affection of these bodies, rather than the result of an irritation propagated to them from some other inflamed structure. Their enlargement is unattended with pain, heat, or any of the other symptoms of inflammatory action: nor is it accompanied by any alteration in the surrounding tissues, or by a disposition to the production of pus. Notwithstanding the different characters which the enlargement in question may present, it appears nearly in all cases to consist of an uniform texture throughout, rather the consequence of a general increase of the whole gland than of a new structure displacing the original one, as is the case when ordinary tuberculous matter is deposited in these bodies.

The state of the spleen, too, is very remarkable, inasmuch as in all the preceding cases but one, it was found diseased, and in some of them thickly pervaded with bodies in structure resembling the diseased glands. Now, although in human spleens no glandular tissue is distinguishable, in those of some inferior animals a multitude of minute bodies exist, which appear to be of that nature. Malpighi, indeed, considered the acini of the spleen to be glands. And Dr. Hodgkin conceives, that if there be, as there appears to be, a close connexion between the derangement of the glands and that of the spleen, the latter is a posterior effect, and thus may not always have been produced before the patient is carried off. In further confirmation of this view, he appeals to the pathological collection of Dr. Carswell, among which, having accidentally found a drawing of an immense spleen, loaded with tubercles, like those above-described, he was agreeably surprised by the sight of another drawing, of greatly enlarged glands of the neck, axilla, and groins, from the same subject. Dr. Carswell called it a case of cancer cerebiformis of the glands and spleen, and his account of it was to this effect:—The man in whom the morbid appearances were found, had been a patient at the Hôpital St. Louis, Paris. He was between 30 and 40 years of age, and had these glandular swellings apparently for three or four months, without pain, and with little inconvenience. But a short time before his admission he felt a difficulty in swallowing, which had at length come to that height that he could take no food whatever. He lived only two days, in great suffering. Here an account of the examination of the body was given from Dr. C.'s notes, and intended to be illustrated by his unrivalled drawings; but owing to an accident, the latter had not come to the

society. The account tallied most exactly with Dr. Hodgkin's observations, already given; but the Doctor did not agree with his friend Dr. C. in applying to these enlargements the appellation of "ceribriform cancer." In one case, indeed, Dr. Hodgkin had likened the growths to fungoid structures, but a closer examination detected abundant difference.

Besides the preceding instances from the dead, the author has met with similar examples of diseased or altered glands among the living. A Jew, between 40 and 50 years of age, came under his observation with the glands of the neck prodigiously enlarged, forming smooth ovoid masses, unaccompanied by inflammatory symptoms or thickening of the surrounding cellular structure. Those of the axillæ and groin were in the same state. There was no appearance of dropsical effusion about him. In another case, that of an emaciated child, an out-patient of Guy's hospital, similar appearances were observed.

In conclusion, Dr. Hodgkin confessed that he had nothing to offer with regard to treatment, either curative or palliative. Most of the patients had only sought relief when the disease had reached an advanced and hopeless stage; and in the case of the Jew, the cascarrilla and soda, and the iodine, appeared to be productive of no advantage. At an earlier period of the complaint, the indications would seem to be, to increase the general vigour of the system, to protect them from exposure to cold and inclement weather, to employ iodine externally, and internally to push the use of the caustic potash as far as circumstances might render it allowable. This last part of the treatment Dr. H. approves in consequence of Brandrish's recommendation, fully corresponding, as it does, with the views of the functions of the absorbent vessels, given in the present paper.

WESTMINSTER SOCIETY.

Saturday, January 7, 1831.

GEORGE SIGMOND, M.D. IN THE CHAIR.

Conclusion of the debate on Cholera.

DR. WHITE opened the adjourned discussion on the nature and treatment of cholera, by reading a paper, of which the following is an abstract.

DR. WHITE began by observing, that, in compliance with the request made to him on the last night of meeting, he should now explain briefly his views of the nature of cholera, and the general rules of treatment of which he approved. He believed the disease to be a form of epidemic fever, presenting different phases and varieties, according to

circumstances. Such is the variety on some occasions, as to render it difficult to tell whether it is cholera or an ordinary attack of indigestion, under which the patient labours; and again, whether the cold fit of an intermittent be not present. It is in the extreme forms, where there is a sudden and universal torpor, that the disease cannot be mistaken. Pathology justifies the comparison between cholera and fever to a very high degree: not only is this to be inferred from the morbid appearances presented by the intestines in the two diseases, but the identity of the morbid changes, which occur in the centre of the nervous system, and the great congestion of all the vessels within the spinal canal, render the analogy almost perfect. In fever, the veins within the spinal canal, and the vessels ramifying on the spinal cord, and those of the sheath even, are highly injected: in cholera, Dr. W. has observed similar appearances, and his views are confirmed by the reports of the Polish and Russian practitioners. The spasmodic form of cholera, Dr. W. thinks, is less dangerous than the paralytic, because in it there is still red blood to excite the vessels; and venesection may be practised with much hope of success. In one of his patients labouring under the disease in its most violent spasmodic form, Dr. W. opened a vein, and drew blood from a large orifice, until the spasms ceased; the quantity drawn was about three pounds; but in twelve hours the patient was convalescent. In paralytic cholera, the blood being vitiated the vessels can scarcely recover their contractile power, and bleeding of course must be looked upon as a very doubtful remedy. The brain does not receive its due impulse under these circumstances, and consequently fails in communicating the requisite *nervous energy*, so that the arterialisation of the blood by the atmospheric influence cannot go on. Hence the blood, especially in the veins, accumulates, the central trunks, and vessels of the thorax and cranium, are engorged, and the colourless fluids are forced through the pores of the skin as water from a sponge. And to these phenomena may be traced all the other external symptoms indicative of general paralysis, and of the worst form of the disease. With reference to *treatment*, the cholera may be divided into three stages—1. The preliminary stage. 2. The stage of collapse. And 3. That of reaction. After quoting a passage from a letter of Dr. Sanders, of Edinburgh, confirmatory of his views regarding the proper treatment to be observed in the first stage, Dr. White informed the Society that he too had seen the disease arrested *in limine* by a dose of opium and tartarized antimony, aided by warm drinks and confinement to bed, in order to encourage perspiration. Calomel and antimonials are then truly useful, as acting at once both on the bowels and the skin.

When coldness of the extremities has already appeared, the indications are to rally the circulation, and to relieve the oppressed viscera by abstraction of blood. Dr. W.'s plan is to administer for the first of these objects, pills of opium two grains, camphor two grains, and one of black pepper: one or two of these pills is given, and repeated, if necessary, in an hour afterwards, with two or three drachms of an aromatic tincture of ginger. Of emetics, Dr. White cannot speak from experience; nor of bleeding, as his practice was principally among natives of India, with whom it was seldom admissible. In the stage of collapse, we should be aware that it is the paralytic form of the disease with which we have to contend. Stimulants are to be vigorously applied to the surface; sinapisms, rendered more pungent by the admixture of turpentine or capsicum, to the whole spine and lower extremities; friction or sinapisms to the epigastrium; draughts of laudanum, ammonia, or ether, and some of the essential oils, mixed with an aromatic tincture; and aided, if possible, by drinks of weak spirits and water, and also by heated stimulating enemata. The system may be thus rallied, so as to allow a trial of bleeding, local or general. Dry heat meantime is to be applied in every particular form, without disturbing the patient—a point of vital importance in the management of this disease. The actual cautery, as recommended by Drs. Russell and Barry, may be very fairly tried, if it be found that sinapisms will not be sufficiently efficacious; and boiling water may be ordered to the epigastrium. Cupping the nape of the neck will frequently relieve both head and spine. If the patient be thus fortunately reanimated, the bowels are next to be attended to. Repeated doses of calomel and ipecacuanha are to be followed by more active cathartics, combined with bark and bitters. Should a feverish accession supervene after this treatment (as it rarely did among Dr. W.'s patients in India) camphorated mercurial frictions on the spine would seem to promise advantage; but we should be chiefly guided by circumstances. In conclusion, Dr. White begged the society to bear in mind, that he stated nothing that he did not deem himself warranted to do, by the experience which he had had of the disease. He deprecated the absurd speculations of those who would search after a specific for a complaint so various as to bid defiance in some instances to our most powerful remedies, and in others to yield to a draught of warm water; and he earnestly recommended that cholera should be studied in its mild as well as in its severe forms.

Dr. GILCHRIST denied the parallel drawn by the author of the paper between cholera and fever to exist.

Dr. GREGORY complained of the very

meagre accounts of the treatment of cholera which have been made public, and of the want of information as to the practice which has been, and now is being, pursued in the north of England.

Dr. JOHNSON thought the strictures of Dr. Gregory uncalled for, as, when the severe duties and anxieties of the medical practitioners at Sunderland and elsewhere are considered, it must be admitted that it is the wisest plan not to publish in the daily prints partial and imperfect statements. He had learned, both 'orally and literally,' that, with scarcely an exception, the northern cases had been preceded by diarrhoea for four, five, six, or even seven days; and that, when early brought under treatment, they were, for the most part, very tractable, and that the severe and fatal cases were those alone in which these premonitory symptoms had been neglected. As to treatment, he learned that no remedy had been followed by such beneficial results as full free vomiting.

Dr. GORDON SMITH declared, that he had read all the books, and reports, and essays, that had been published on this all-absorbing question; that he had spelt the labouring columns of the newspapers; that he had consulted philosophic men in the profession, and philosophic men out of the profession,—nay, he had consulted philosophic *women* also; that he had thought upon the subject by day, and had dreamt of it by night; and he had arrived at a conclusion, *for which he was himself alone responsible*; of which neither the credit nor the discredit would be impart to another, viz. that, after all his reading, his talking, his thinking, and his dreaming, he knew nothing more about the matter than he did before he began.

Mr. BURNETT differed widely from the honourable gentleman who had just sat down as to the results of the discussions, and trusted that his conclusions were indeed peculiar to himself alone: of the credit he would not endeavour to deprive him. "If," said he, "that learned physician has really read all that has been written—all that has been published weekly, daily—nay, almost hourly, upon cholera, I truly pity him; and if, after such labour, he has reaped no fruits, I pity him still more. Others who have read much less; who have talked much less; have consulted philosophers much less, whether in or out of the profession, whether of the firmer or the softer sex; who have thought of the subject much less by day, and have dreamt of it much less by night, than the learned doctor's report of himself, would seem to intimate hope that, by taking a more moderate dose, they have experienced better effects; hope that, by not having fed to repletion, they have the better digested their more moderate meal. He therefore differed widely from the statement that we knew nothing more of

the disease called spasmodic or blue, or Indian cholera now, than we did before the discussions on it commenced; he denied that we knew as much about it a quarter of a century ago as was known now, when in fact the disease itself was then hardly, if at all, known in the spot whence it has spread to us; for it was not until 1817 that it attracted notice, when it raged on the Delta of the Ganges. On the contrary, he contended that the debates of the Westminster Society alone would prove that much knowledge had been gained by the profession at large; and many most important facts had been communicated by those who had had the disease under treatment, to those who as yet had never seen it. Mr. B. then went over the various points in the history, symptoms, treatment, &c. of the disease, upon each of which he contended much light had been thrown.

After a few remarks from Dr. Stewart, Mr. Field, and Mr. Davis, who had suffered in his own person from cholera, and who greatly extolled diluents, Dr. White replied very shortly to the casual remarks and objections which had been made upon his paper, which being very few, did not require any elaborate comment; indeed, the whole tenor of the discussion fully shewed that the topic was exhausted, and we therefore heard with pleasure that the debate was closed, and that cholera is to have a respite.

MASSACHUSETTS ANATOMY BILL.

COMMONWEALTH OF MASSACHUSETTS.

In the Year of our Lord One Thousand Eight Hundred and Thirty-One.

An Act more effectually to protect the Sepulchres of the Dead, and to legalize the Study of Anatomy in certain cases.*

SECT. 1. Be it enacted by the Senate and House of Representatives in General Court assembled, and by the authority of the same, That if any person, not being authorized by the Board of Health, Overseers of the Poor, or Selectmen in any town of this commonwealth, or by the Directors of the House of Industry, Overseers of the Poor, or Mayor and Aldermen of the city of Boston, in said commonwealth, shall knowingly or wilfully dig up, remove or convey away, or aid and assist in digging up, removing or conveying away, any human body, or the remains thereof, such person or persons so offending, on conviction of such offence in the Supreme

Judicial Court of this commonwealth, shall be adjudged guilty of felony, and shall be punished by solitary imprisonment for a term not exceeding ten days, and by confinement afterwards to hard labour for a term not exceeding one year; or shall be punished by a fine not exceeding two thousand dollars to enure to the benefit of the commonwealth, and by imprisonment in the common jail for a term not exceeding two years at the discretion of the court, according to the nature and aggravation of the offence.

SECT. 2. Be it further enacted, That if any person shall be in any way, either before or after the fact, accessory to the commission, by any person or persons of the offence described in the first section of this Act, such person or persons shall be adjudged and taken to be principals, and shall be, on conviction in the court aforesaid, subject to the same punishments and forfeitures as are in said first section provided.

SECT. 3. Be it further enacted, That from and after the passing of this Act, it shall be lawful for the Board of Health, Overseers of the Poor, and Selectmen of any town in this commonwealth, and for the Directors of the House of Industry, Overseers of the Poor, and Mayor and Aldermen of the city of Boston in said commonwealth, to surrender the dead bodies of such persons, *except town paupers**, as may be required to be buried at the public expense to any regular physician, duly licensed according to the laws of this commonwealth, to be by said physician used for the advancement of anatomical science; preference being always given to the medical schools that now are or hereafter may be by law established in this commonwealth, during such portions of the year as such schools or either of them may require subjects for the instruction of medical students: Provided always, That no such dead body shall in any case be so surrendered, if within thirty-six hours from the time of its death any one or more persons claiming to be kin, friend, or acquaintance to the deceased, shall require to have said dead body inhumed; or, if it be made to appear to the Selectmen or Overseers of the Poor of any town in this commonwealth, or to the Mayor and Aldermen or Overseers of the Poor of the city of Boston, that such dead body is the remains of a stranger or traveller, who suddenly died before making known who or whence he was; but said dead body shall be inhumed, and, when so inhumed, any person disinterring the same for purposes of dissection, or being accessory, as is described in the second section of this Act to such exhumation, shall be liable to the punishments and forfeitures in this Act respectively provided: And provided further, That every

* The Amendments of the House of Representatives are printed in italics, and were all moved by the Chairman, J. B. Davis, Esq. to meet the views of the House.

* "Those who receive charity from the overseers of the poor, and die out of the alms-house, or House of Industry, as we term it."

physician so receiving any such dead body, before it be lawful to deliver him, the same shall in each case give to the Mayor and Aldermen of the city of Boston, or to the Selectmen of any town of this commonwealth, as each case may require, good and sufficient bond or bonds, that each body by him so received shall be used only for the promotion of anatomical science; that it shall be used for such purposes only in this commonwealth, and so as in no event to outrage the public feeling; and that, after having been so used, the remains thereof shall be decently inhumed.

SECT. 4. Be it further enacted, That, from and after the passing of this Act, it shall be lawful for any physician duly licensed according to the laws of this commonwealth, or for any medical student under the authority of any such physician, to have in his possession, to use and employ human dead bodies, or the parts thereof for purposes of anatomical inquiry or instruction.

SECT. 5. Be it further enacted, That nothing in this Act shall be so construed as to give to the Board of Health, Overseers of the Poor, or Selectmen of any town in this commonwealth, or to the Directors of the House of Industry, Overseers of the Poor, or Mayor and Aldermen of the city of Boston, in said commonwealth, any power to license the digging up of any dead human body, or the remains thereof, other than was possessed by them before the passing of this Act, or is given them by the third section of this Act.

SECT. 6. Be it further enacted, That the Act passed March 2, 1815, entitled 'An Act to protect the Sepulchres of the Dead,' and also all other Acts, or parts of Acts, contravening the provisions of this Act, be, and the same hereby are repealed.

House of Representatives, Feb. 11, 1831.

Read three times, and passed, to be engrossed.

Sent up for concurrence,
P. W. WARREN, Clerk. 4.

REPORTS OF CASES OCCURRING AT PUBLIC INSTITUTIONS.

MEATH HOSPITAL AND COUNTY OF DUBLIN INFIRMARY.

Case treated by Mr. Bernard, under the superintendence of the Clinical Physicians.

Emphysema of the Lungs, with Hypertrophy of Right Ventricle.

JOHN CARTY, ætatis 24, admitted February 5th. This man's occupation, in a glass manufactory, had constantly subjected him to great and sudden alternations of tempera-

ture, in consequence of which he was attacked about eight years ago with a very severe cough. Some temporary relief was afforded by repeated bleedings and blisters, but the cough never entirely left him; any unusual exposure aggravated it, and his expectation was always scanty. These attacks were very frequent, although he had given up his former business. He had some dyspnœa from the commencement, and palpitation, both which have continued to increase, particularly within the last few months. He now presents the following appearances:—

His countenance is distressed; face swollen, and of an earthy cast; lips turgid, and somewhat of a violet colour: great dyspnœa, and orthopœa, particularly at night; decubitus most easy on the right side; cough not very frequent, but hard and short, at times almost convulsive, occasioning great oppression both in the heart and head; respiration thoracic, laborious, and unequal, inspiration being longer than expiration; chest evidently more convex and prominent than natural; complains of great pain and throbbing in the temples, with tinnitus aurium.

Percussion yields a remarkably clear sound over the whole chest; and even in the præcordial region there is but little dullness; the sound of the respiratory murmur is not in proportion with the efforts the patient makes; it wants the full character of expansion, though somewhat louder, under the clavicles, and mixed throughout with loud sonorous and sibilous râles; posteriorly the respiration is also feeble, with some "dry crepitating râle" on the right side, but no "friction of ascent or descent;" in the left lung there is distinct sibilous râle; heart's pulsation scarcely felt between the sixth and seventh ribs, but very violent under the lower edge of the sternum; the sounds of the left side, as heard by the stethoscope, are natural, but those of the right are preternaturally loud and distinct: some pulsation of the jugular veins is perceptible; in the umbilical and right iliac regions, the abdomen sounds tympanitic, and pressure occasions pain; pulse 108, small; tongue dark-coloured and dry.

A blister to the chest. A bolus of Jalap.

6th.—His sleep was entirely prevented by constant throbbing in his head. Bowels opened once; no pulsation of the jugulars to-day.

V. S. ad 3viii.

Habt. Calomelanos, gr. i.

Pulv. Digitalis, gr. ss. Scillæ, gr. ij.
ter in die.

7th.—Relieved by the bleeding; less dyspnœa; action of the heart diminished; bowels open.

Rep. Pilulæ.

9th.—Pain in the head still severe. He was yesterday ordered a blister to the back of his neck, and to-day twelve leeches to the temples.

Hab. Potum Tartari.

10th.—The leeches gave no relief. His breathing less difficult, and cough diminished. Action of the heart less violent; pulse 86, soft; mouth sore.

Omit Pil.

15th.—He has had a blister to the neck, but very little improvement has taken place. The bronchitic râles have diminished, and action of heart is tranquil. Abdomen less tympanitic. Bowels confined for some days.

Ordered some purgative medicine.

16th.—Bowels not opened.

Hab. Olei Crotonis guttam.
Ricini, ζ ss.

Cough and breathing more difficult to-day.

20th.—Since he took the croton oil his bowels have been very irritable, with some griping. His cough and dyspnœa, however, have diminished as the bowels became affected. For the last three days he has been taking Infus. Digitalis, ζ ss. ter in die. The circulation is tranquil, but still the pain in his head continues obstinate, situated principally towards the sides and back part, so as entirely to prevent his sleeping.

Infricetur Unguent Tart. Antim. Abdomini. Cont. Infus. Digitalis.

27th.—Bowels now quite natural. The dyspnœa, which has increased, forms his principal complaint. It amounts at night to orthopnœa. Head very little relieved; in the recumbent posture, the pulsating pain is intense, while on sitting up, or attempting to walk, he is seized with slight vertigo; pulse tranquil; action of the left ventricle not greater than natural—that of the right is much diminished; some pain and tenderness on the left side of the abdomen, very little in the region of the liver; urine scanty.

Appl. Vesicat. parti abdominis dolenti.

R Pulv. Scillæ, gr. ii.
Digitalis, gr. ss.

Camphoræ, gr. i. fiat Pil. i. ter in die sumend. Ordered to have a vapour bath.

March 2d.—Having complained yesterday of acute pain in the epigastrium and right hypochondrium, 12 leeches were applied, and afforded great relief.

The bronchitis has nearly disappeared, and the dyspnœa and cough are diminished.

The pain, which he complained of in the left side, still continues, although a grain of morphia was applied on the blistered surface. He describes it to be dull and deep-

seated, and refers it to a spot of small extent, between the last false rib and crest of the ilium on the left side, about three inches from the spinous processes of the vertebræ. No tumor can be detected, nor is any pulsation discovered by the stethoscope. Pain is not increased on any particular occasion, except when coughing, by which it is aggravated.

The head seems less troublesome; pulse not accelerated; urine copious; bowels regular.

Rept. Pilulæ Scillæ et Digit.

5th.—No perceptible change since the last report. The pain in the left lumbar region still continues unaltered, and the man occasionally complains of a similar one on the opposite side in the same spot, though less severe. Upon the whole, he has been much relieved since his admission. The cough and dyspnœa are materially less, and the bronchitis, as appears by the stethoscope, has disappeared. Some lightness in his head still continues, but the turgescence and lividness of his countenance are quite removed.

Ordered to be discharged.

REMARKS.—This case suggests some useful and interesting hints for reflection. The importance of the stethoscope and percussion, indeed the impossibility of making an accurate diagnosis in such cases without them, is clearly shewn. This form of disease of the lungs, though first accurately described, and its diagnostic characters pointed out by Laennec, was not unknown to Baillie, though he confesses his inability to point out any decided diagnostics by which it might be recognized. He says, "when the air-cells of the lungs are *much enlarged*, persons have been long subject to difficulty of breathing, especially on motion; but, I believe, no symptom is at present known, by which this disease can be *discriminated from some other affections of the chest*."—*Morbid Anat.*

The cough attending this affection appeared peculiar, being rather forced and solitary than in fits like common bronchitis, and resembling very accurately the cough of a "broken-winded horse." This peculiarity, connected with the unnatural expansion of the thorax, and the lividness and turgescence of the countenance, would almost suffice, at the first glance, to point out the nature of the affection.

Then, as to the train of accompanying phenomena: the pulmonary obstruction evidently occasioning hypertrophy of the right ventricle, the same cause would also give rise to a congestion of unduly-aerated blood, producing the lividness and turgescence of the face. The enlargement, or what Andral denominates the mechanical hyperemia, of the liver, would also result from the obstruction to the course of the blood in the vena cava, and would account for the œdematous

swellings of the legs. Thus the whole series of symptoms may be ultimately referred to the mechanical obstruction in the lungs. One point in the case to attract attention, was the very frequent irregularity of the determination of blood to different parts, and the alternation of its effects on different organs, affording a fine illustration of the pathological law, that when the capillary circulation in one part is deranged, it has a strong tendency to lose its healthy balance of action in all. Thus we found at one time the severity of symptoms predominating in the chest; at another in the intestinal mucous membrane, as shewn by the diarrhoea; and then again, in proportion as this was checked, the bronchial membrane again became congested; while, at another time, the symptoms would be most severe in the head. With respect to the latter, however, there seems some difficulty in explaining the constant pain and throbbing of which he complained, in spite of all the various means which were tried to give relief; and not this alone, but also the feeling of lightness and vertigo which he experienced in the erect posture, remain unexplained. At first we might have thought that it depended on the supply of imperfectly aerated blood to the head, or to the obstruction afforded to its return; but the symptoms continued unabated even after the return of the natural colour to the countenance, and the diminished action of the right ventricle shewed that the general mass of the circulation was reduced. Nor was there any perceptible increase of action in the left side of the heart, to which we might attribute a portion of the phenomena.

Another point in this case may be deserving of notice, viz. the gradual manner in which the lungs will accommodate themselves, and the system generally become habituated, to the obstructed circulation. Although the dilatation of the air cells might tend to increase, and thus the obstruction become also greater, still there can be no doubt, that if the same degree of obstruction were to take place suddenly, as might happen from rupture of the cells in over-exertion of the voice, and the escape of air into the parenchymatous texture (which would seem to have been the case in one instance related by Magendie), the dyspnoea would instantly become so intense as to threaten immediate suffocation; but when the obstruction takes place gradually, the whole system recedes itself, as it were, to the state of the lungs; the dyspnoea is less distressing; and, instead of increasing, might possibly at length become diminished, provided the obstruction did not itself tend also to increase. This would necessarily be facilitated by reducing the whole circulating mass, which was accordingly done in this instance, both by bleeding and diuretics; the pulse became lessened, the action of the right ventricle reduced, and the dyspnoea was in consequence very much relieved.

NEW STYPTIC.

In the sheep upon which the experiments were made with the new styptic at the London Hospital, the hæmorrhage returned on Monday last, the seventh day after the carotid artery had been opened; the bleeding continued during the following day, and the animal died on Wednesday morning. Upon examining the neck, about four ounces of coagulated blood were found surrounding the artery, which contained no coagulum, nor had any process taken place for closing the wound in it. Had the external wound healed, there would have been a spurious aneurism. The experiments, therefore, with the new styptic, at the London Hospital, have proved complete failures.

We understand that Mr. Cæsar Hawkins is engaged in some experiments on this subject, which, so far as they have gone, are calculated to lessen, or destroy, any faith which may have been reposed in the efficacy of the styptic of MM. Talrich and Halma-Grand. We shall give the results as soon as the inquiry is completed.

METEOROLOGICAL JOURNAL,

*Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.*

Jan. 1832.	THERMOMETER.	BAROMETER.
12	from 33 to 46	from 29.72 to 29.56
13	35 41	29.50 29.74
14	27 37	30.03 30.23
15	21 36	30.34 30.38
16	21 35	30.36 30.32
17	30 41	30.26 30.23
18	42 28	30.25 30.26

Wind variable, S.W. prevailing.

Except the 12th, 13th, and 17th, generally clear; rain on the 13th, and in the evenings of the 12th and 14th.

Rain fallen, .575 of an inch.

CHARLES HENRY ADAMS.

NOTICES.

"Anatomicus" will have seen that his sensible suggestions were anticipated.

We have not had room for the letter of "A Constant Reader." It would be very absurd, indeed, if what he alludes to should happen to be overlooked; but it is not likely.

The papers of several correspondents, which are unavoidably postponed, shall be inserted next week.

We regret that we cannot insert Dr. Badeley's epigram regarding cholera; it is clever, but scarcely fair towards those who do their utmost to elucidate a difficult subject.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF
Medicine and the Collateral Sciences.

SATURDAY, JANUARY 28, 1832.

LECTURES
ON
THE THEORY AND PRACTICE OF
MEDICINE;

Delivered at the London University,

By DR. ELLIOTSON.

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PART I.—LECTURE XVII.

DROPSY.

I WAS in the last lecture, gentlemen, speaking of dropsy, and endeavouring to shew that very frequently it is of a highly inflammatory nature, that there is excessive secretion depending altogether upon inflammation of an active kind, and to be subdued simply by the remedies of inflammation. I mentioned that on the other hand it was sometimes devoid of all inflammatory symptoms, or at least, if inflammatory symptoms had existed, they have entirely ceased.

Dropsy united with internal disease.

It frequently happens that inflammatory anasarca is united with inflammation of certain internal organs—with an inflammatory state of the head, not a decided phrenitis, but heaviness, giddiness, and pain—an inflammatory state of the head—sometimes with an inflammatory state of the abdomen—but as far as I have observed, much more frequently with an inflammatory state of the lungs, either in the form of bronchitis, which is by far the most common, or peripneumonia. And the other kind of dropsy which appears independent of inflammation, is sometimes united with, and is one of the last symptoms of, structural disease of an internal organ. It is very common in disease of the heart for a person to become dropsical; in a large number of instances of phthisis, there is œdema of the legs towards the conclusion of the affection; and when the liver, the uterus, or the spleen is diseased, dropsy is exceedingly common.

Coincidence between Dropsy, Hæmorrhage, and Fluxes.

Now all these particular dropsies closely resemble hæmorrhage and the fluxes. You will recollect that hæmorrhage and that fluxes are frequently of an active inflammatory kind, and to be treated not by remedies calculated to arrest the flow of fluid, but by the remedies of inflammation—not by astringents, but by antiphlogistic measures. So inflammatory dropsy is to be treated, not by remedies calculated to carry off the fluid, or stop up the mouths of the vessels which pour forth the secretion, but by remedies calculated to remove inflammation. You will find in hæmorrhage, that frequently there is no inflammation at all, but merely such a debilitated state of the vessels as allows the blood to escape from their extremities, and dropsy and fluxes frequently appear in that form. You will recollect that hæmorrhage, too, frequently arises under visceral disease. It is very common for persons with diseased liver to have hæmorrhage from the nose; and in organic disease of the liver and spleen, persons will vomit blood and discharge a great quantity from the alimentary canal downwards. I need not say that when various organic diseases produce ulceration, then you may have the most frightful hæmorrhage. This is seen every day in the case of the intestines and of the uterus. So with respect to dropsy; you will recollect that when organic disease takes place in various organs, that the body at large, or one part in particular, may become dropsical, and the more you debilitate the patient in this case, as in the case of passive hæmorrhage, the greater will be the increase of the dropsy, just as would be the increase of the hæmorrhage. In many cases of hæmorrhage the flow of blood proceeds from mechanical obstruction, and the same occurrence takes place in dropsy. If great obstruction take place in any of the veins, dropsy must be the consequence. Local and more or less extensive plethora takes place in all the vessels on the other side of the

obstruction, and as a mere physical effect, the watery parts of the blood ooze forth from their extremities, and the part becomes more or less turgid, from the great congestion. You know that there is every reason to believe that the swelling of the leg after parturition, called *phlegmasia dolens*, depends upon mere mechanical obstruction. There is more or less dropsy of the leg, and it usually arises, as is proved by Dr. Davis and others, from obstruction—there is an obstruction of the veins at the upper part of the extremity, and perhaps also in the veins still higher up. The pressure of a very enlarged womb will often in this way induce dropsy of the lower extremities. In cancer affecting the axillary glands, or when the breast is attacked by cancer or scirrhus, you see an upper extremity become œdematous, simply from mechanical obstruction. It is not necessary that the absorbents should be affected: for if the veins be obstructed the same thing is produced. A local plethora takes place—both of veins and capillary vessels, and these pour forth a watery secretion, and you are sure to see dropsy at last, simply as a mechanical effect.

Dropsy independent of Inflammation.

In those cases of dropsy which are independent of inflammation, there may be organic disease, or there may not; very frequently you have evidence of organic disease in the system affecting particular parts. If phthisis it is sufficiently clear; if disease of the heart it is sufficiently clear; if disease of the liver, you may generally ascertain the state of the parts; you find the liver enlarged or indurated, or both—and so with regard to the spleen. If, however, you can find no organic disease whatever, and yet there are no signs of inflammation, if the patient be evidently sinking under his dropsy, and the urine is albuminous, I believe in the greater number of cases—provided the urine is *constantly* albuminous—the kidney is diseased, and it is generally found granulated, mottled, or labouring under great congestion of blood. You will recollect that I mentioned yesterday, as my own opinion, that an albuminous state of the urine does not *necessarily* indicate organic disease of the kidney: I am satisfied that the patient may be labouring under mere functional derangement of that organ of a temporary nature. But when you see a person sinking under dropsy, and the urine is albuminous, you may in almost every case expect to find more or less organic affection of the kidney. This is a curious circumstance, for the knowledge of which we are indebted to Dr. Bright.

With respect to the symptoms of these various dropsies, I shall mention them when speaking of diseases of different organs of the body, but with respect to anasarca, as that is a general affection, and therefore will

not come under diseases of particular organs, I mentioned the symptoms of it, and likewise the treatment, as well as of dropsy in general, in the last lecture.

Anasarca will often exist alone—it frequently is an inflammatory dropsy, and when not, it will often exist without any evidence of organic disease, though after death we perhaps find the kidney diseased; but anasarca as only one form of dropsy often follows others. When a person has water in the chest, it is usual for the legs soon to become œdematous, and perhaps the whole body. When there is effusion into the abdomen, it is very common for *ascites*, as it is called, sooner or later to be followed by swollen legs, and at last the œdema may pervade the whole body, and become anasarca. It may exist alone, or it may follow others; sometimes it is the first that begins, and then the other dropsies follow.

Prognosis.—I did not mention any particulars for forming a prognosis in cases of hæmorrhage, nor in the case of the various fluxes, because the prognosis is only to be made from observing the degree of disturbance on the one hand, and the strength of the patient on the other. In dropsy, we must of course follow the same rule; but as dropsy, when it is not inflammatory, so frequently depends upon organic disease, our prognosis will depend in a great number of cases of a chronic character upon the evidence which there is of organic affection: and we have also carefully to note, in giving a prognosis in regard to dropsy, whether, although the dropsy improves, some other symptom does not get worse or continue. Dropsy will sometimes diminish rather suddenly, and the patient so far from being better, although that is apparently the case, will die within a very short time. Sometimes a short period before death, all the external symptoms of dropsy have diminished; sometimes ascites will nearly disappear, or greatly improve at last, and œdema of the legs and the whole body diminish very considerably, and yet the patient speedily die, perhaps of apoplexy, perhaps of effusion into the chest. It is necessary not to be satisfied with the disappearance of any of the symptoms of dropsy. You must also carefully observe whether there are still not sufficient symptoms of mischief within, to make you fear the result of the case. Very frequently the severest symptoms may exist, and soon exhibit the most dangerous character, after a dropsy has spontaneously diminished.

Quality of the Fluid.—In regard to the fluid of these various dropsies in the chest,—the pleuræ and the pericardium, in the peritoneum, and in inflammatory dropsy of the head—it is sometimes perfectly clear; but more frequently it is at least turbid, not transparent, though translucent; sometimes it is very turbid indeed, and sometimes

bloody. The fluid which forms the tumor in anasarca is perfectly clear, and this is proved by the operation of acupuncture. If you make a minute puncture of the skin with the point of a needle, which is acupuncture, a bead of fluid follows, which is beautifully transparent. In the case of chronic dropsy within the head—chronic hydrocephalus—the collection of fluid is almost like rock water. In chronic hydrocephalus, you more frequently than otherwise, find the fluid collected within the ventricles of the brain, and this is very little different from pure water; it contains the least possible quantity of animal matter and salts. I believe the fluid of chronic hydrocephalus comes nearest to pure water of any fluid that is morbidly collected in the body.

DEFICIENT SECRETION.

We have some other diseases exactly the reverse of hæmorrhage, profluvia, and dropsy—we have a deficiency of secretion. In the height of inflammation the secretion may become scanty from the part itself; although it is increased at first, it will decrease afterwards, but still remain usually beyond the natural quantity. Sometimes, however, in inflammation, the secretion is at a stand; the secretions of the part itself, as well as of the rest of the body, are diminished, but sometimes the part affected secretes hardly at all. This circumstance may occur, however, independently of inflammation. As with respect to violent discharges, so we sometimes have, independent of inflammation, a perfect deficiency of secretion. This happens in a most remarkable way in the case of the kidney. Old men will sometimes cease to make water, not from any retention of urine, but from a suppression of it—the kidney ceasing to secrete, or nearly ceasing. Ischuria renalis, as it is called, is an instance of a case exactly opposite to that which occurs in profluvia. The treatment of this particular disease I shall, of course, speak of hereafter: I am now only speaking of these affections in general. We sometimes observe, on opening bodies, a similar state of the serous membranes. Sometimes the pericardium, and sometimes the pleura, is perfectly dry, almost as dry as a piece of dried bladder. You will find this mentioned in many works on morbid anatomy. I do not know that this state induces any symptoms during life; but that which is found after death is precisely opposite to what occurs in the affections of which I have just been speaking.

CHANGES OF STRUCTURE.

These are all mere phenomena or occurrences—functional matters—but inflammation produces changes of structure—and as I have spoken of those which affect merely secretion or the escape of different fluids, I now proceed to those circumstances which affect the substance itself of

the body. Now, the changes of structure, which are frequently produced by inflammation, frequently connected with inflammation, and sometimes like hæmorrhages, dropsies, and profluvia, unconnected with inflammation, are, in the first place, changes of consistence, and changes of size; in the next place, transformations, in which the substance is changed in its nature; and thirdly, new formations, in which something new appears, totally different from what we see in the body naturally. Two or all three of these may occur together.

Changes of Consistence.

The first of these of which I shall speak, and which are more frequently the effect of inflammation than not, are changes of consistence and changes of size. With respect to changes of consistence, I need not say that they can only be two, hardening on the one hand, and softening on the other; and of these changes of consistence, I shall first speak of induration.

Induration.

It is to be remembered, however, in the first place, that induration may be only apparent. An organ may feel much harder than natural, may be really harder, and yet we are justified in saying that the hardness is only apparent. I will explain this. If there be extreme congestion in a part that can expand, it will become larger, and feel much harder than natural. If there be an obstruction to the flow of blood from the liver, though the liver itself will only be of its natural hardness, yet, in consequence of the obstruction, it will feel exceedingly hard; but on liberating the blood, or making an incision and letting the blood escape, it will come down to its natural consistence. By an obstruction during life this effect may be induced, and I believe that by a violent injection of fluid after death the same thing may be caused. We must therefore consider, when we see a part indurated, whether it is organic, induration, or merely a state of great turgescence, or repletion, in consequence of an excess of fluid. When a part is really hardened, it generally is pale; of course, if the hardness be merely dependent upon the collection of blood in the part, it will be of a deeper colour than usual, but if the part be organically hardened, if it have undergone a hardening process, it is generally paler. Being more compact, you must suppose that, in general, the diameter of the blood-vessels will be less, and therefore there will not be such a proportion of blood as there was before. Sometimes the hardened part acquires a new colour; it will be grey, yellow, brown, or even black—but, for the most part, a portion of the body which has become indurated, is paler than usual. It does not follow because a part has become indurated that the size should be al-

tered. You may have a part hardened, and it may be larger or smaller than before, or it may retain its natural size. We have every day instances of induration, in the case of a fistula along the side of the rectum, and in fistula in perineo. You may find here, that the parts are all exceedingly hard. The heart is not unfrequently in this condition: this organ is more frequently affected in size, but not unfrequently we find the heart hard. Even the general cellular membrane is frequently seen hardened, and especially in young children.

This process of hardening is for the most part slow, and for the most part the effect of chronic inflammation. Sometimes it does not occur till the inflammation is entirely over, till the part has become pale and there is no reason to suppose that inflammation exists, and then that process will take place by which the part becomes hardened.

Treatment.—In the latter instance the affection is certainly best removed by irritating the part. If the hardening be evidently attended by inflammation, antiphlogistic remedies are the best that can be adopted. Bleeding, both local and general, together with the exhibition of mercury, iodine, fomentations, poultices, friction, and pressure, are particularly proper. But sometimes there is no inflammation to be discovered, and then stimulating the part answers much better than any thing else. It is also of great use to excite a discharge from the part itself, or close to it; that will frequently cause an absorption of the excessive matter which has been deposited. Occasionally, when there is no inflammation to be discovered, the treatment of induration of a part becomes so irritating, that inflammation is set up; so that inflammation, although it may at first have laid the foundation of the disease, and produced it, yet afterwards has ceased, and then again a third stage come on.

Softening.

The opposite to this change of parts is softening, called by the French *ramollissement*, and many persons would not for the world use the word softening, but must ever employ the term *ramollissement*: however, it can mean nothing more than softening in plain English. Formerly this was very vaguely described. Even induration was commonly described as a termination of inflammation by the name of *scirrhus*, *scirrhus* being confounded with simple induration. And as to softening, the subject has not been understood till of late years. It is to the French that we are particularly indebted for our knowledge of that change of parts which consists in softening. This process is remarkably seen in the nervous system, and also in the alimentary canal. The mucous membrane of the stomach and intestines is frequently reduced to a mere pulp, which you may brush off with a slight touch

of the finger, and sometimes it appears to be brushed off even before death. In the case of the brain, we every day see that organ reduced, in certain portions, to a mere soft paste. It occurs, also, in the skin, sometimes in the epidermis, in the cutis itself, and in the nails. We must all have seen the nails so softened as to appear more like wet card than any thing else. This softening will affect the cellular membrane, and then it will allow the serous membrane without the intestines—for example—or the mucous membrane within, to be peeled off. If you can peel off the peritoneum from a portion of the stomach and intestines, it must be from the cellular membrane between having become so softened, that the serous membrane is no longer attached. So, if you can peel off portions of the mucous membrane, it must be from the cellular membrane beneath becoming so softened that it is no longer properly attached. The cellular membrane will sometimes remain solid, while all the other parts are excessively softened. This is seen in the case of the brain, for example, and the spinal marrow; they shall become soft, while the pia-mater, which corresponds with cellular membrane, immediately upon the brain, and immediately upon the spinal marrow, will be perfectly firm. A serous membrane, such as the peritoneum, will sometimes soften, as also arteries, even cartilage and bone itself. Bones may shew such a disposition to become softened that one species of disease has acquired the particular name of *mollities ossium*; and in rickets, another disease of the bones, they are much below their natural consistence. The muscles, too, will sometimes become soft. After a person has been long exposed to the operation of lead in painting, I have seen the muscles become much softer than they should be, and so pale as to look like any thing but muscles. The heart itself, like other muscles, will frequently become soft; you sometimes may push your finger through the parietes of the heart, so excessively softened do they become. But every structure of every portion of the body may become softened. When parts are softened they frequently retain their natural colour, but sometimes they are paler than they should be, sometimes they are redder, and sometimes they become brown and dark. You see there are great varieties with regard to colour, both when a part is indurated and when it is softened. The colour may be retained, or it may be paler than natural, which I mentioned is generally the case, or it may be deeper than usual.

And as with regard to induration, so with respect to softening, a part may sometimes be larger than it should be, or smaller, and sometimes its bulk remains the same. You would not suppose that a part could be very rapidly softened, and yet it is certain that

a part may be softened in a very short time, at least I myself feel satisfied, together with many French writers, and perhaps with others, that that is the case. It is mentioned by the French that a few days, if not a shorter time, are sufficient to produce a pulpiness of an organ which before was in a state of apparent health. I think I have seen sufficient to make me believe that an organ, apparently sound before, may lose its natural firmness and become a mere pulp, in an acute disease, in a very short space of time.

Induration is generally the effect of chronic inflammation, and softening frequently arises from the same source. But I have seen a person in tolerable health suddenly, without any evident reason, fall into a state of the greatest debility, with most complete prostration of strength, with no pulse, and in two or three days some organ or other has been found reduced to a pulp. I had great doubts when I first noticed this, of the accuracy of my observation, but on reading the French writers I found that they detailed cases exactly similar, and that they came to the conclusion which I felt inclined to do, but was really almost afraid to make up my mind. When very rapid, I do not believe it always the result of inflammation, or even inflammatory; and when a slow process, I am sure it is not always inflammatory, for the parts are often exceedingly pale. In rickets, or mollities ossium, what inflammation is there of the bones?

Induration, I have stated, may be recovered from by the remedies of inflammation, in certain cases, while stimulants in others will frequently remove it, especially if aided by mercury and iodine; but it is not known whether a softened state of a part can be recovered from. It is very doubtful whether this is the case. One would think it possible if the portions be reduced to pulp.

I mentioned that you are not to consider every case of apparent induration as real; that it may depend upon great congestion; from the presence of fluid a part may acquire increased bulk and firmness. But softening cannot be apparent; if a part be soft there can be no deception in it. You have, however, to guard against an error on this point. Softening may occur after death, and, merely because on a *post-mortem* examination you find a part excessively softened, you are not to conclude that it was in that state during life. Parts from decomposition will become softened, they will melt down to a reddish pulp. I mentioned, when speaking of redness, that sometimes a red appearance is mistaken for inflammation, whereas it may be only the effect of a red fluid which transudes and encircles and dyes the part. Parts will become softened from decomposition. The brain, without any morbid state, very speedily becomes soft. Every body

knows that if he wishes to dissect a brain well, he must have it fresh; that if the person have been dead any time, or the brain has been exposed to the air, it becomes too soft for accurate dissection. But the fluids themselves within the body will sometimes dissolve parts. There can be no doubt that the gastric juice will produce a softening of the interior of the organ, and even do more than that, soften the whole coats of the organ, escape into the cavity of the peritoneum, and soften whatever it comes near. This was discovered by John Hunter, and I think we have sufficient proof of the accuracy of his observation. The appearances arising from gastric juice have sometimes led persons who were ignorant of the fact, to imagine that a patient had been poisoned.

I need not say that when softening occurs during life, it must produce some effect. A large number of cases of paralysis depend upon a softening of the brain or spinal marrow. Some cases of fatuity likewise depend upon a softened state of the anterior portion of the brain. Sometimes you have extreme debility when any essential organ, such as the liver, the lungs, or the heart, becomes softened.

Except in bones and parts at or near the surface, the existence of this softened state cannot of course be well known during life: we only infer it as probable. If the part show signs of inflammation, the only treatment you can adopt is that for inflammation. You do not know that the part is softened, you only see that it is inflamed. In cases of paralysis you cannot tell that the part is softened, but when you have symptoms of violent pain at one part of the head, and great heat, and the patient gradually becomes paralytic, there is a probability that the part is softened, but still you have no proof of it. If you see signs of inflammation, you must treat them without regard to softening or induration. If you see prostration of strength, you must endeavour to remove it, not knowing that the parts are softened. In the case of the heart, you may expect extreme feebleness of the stroke of the heart when examined at the chest.

Changes of Size.—Hypertrophy.

These are changes with respect to consistence; we have others with respect to size. If a part obtain really an addition of substance, not dependent on transformation or new formation, it is said to be *hypertrophied*. This is a new word, but it is a very convenient one. The part suffers an excess of nourishment; it is therefore hypertrophied, over-nourished. Now hypertrophy will affect one elementary tissue of an organ only, or it will affect the whole of an organ together,—every tissue of which it is composed. It will sometimes affect the cellular membrane, and then this becomes thicker and perhaps denser than it should be.

You may have a part over-nourished without increasing in size—the excess may be such as merely to harden it, so that in one sense induration may be an hypertrophy: but very generally, when a part is over-nourished, it acquires a considerable excess of bulk.

If the cellular membrane be over-nourished, and at the same time indurated, you will see dead white or semi-translucent streaks along it, — white lines, and frequently portions of it only fall into this state of excessive nourishment. Tumors may be produced which will cut like a turnip, and in which white and greyish fibres are seen. This is the character of scirrhous, and there is a doubt among those who cultivate morbid anatomy, as to whether this ought to be called a new formation, or is merely an excessive nourishment, hypertrophy, and induration, of the cellular membrane. But however this may be, in this state of the parts, when the cellular membrane is thus affected, you see dead white, or semi-translucent lines, —white streaks or white layers. It may form a tumor in one particular spot, which will cut like a turnip, and in which you see white fibres. Sometimes the cellular membrane in this state will cut like lard, or like imperfect cartilage.

Mucous membranes are frequently hypertrophied; you will see the villi of the intestines very much enlarged; you will see the mucous follicles enlarged; and you will see the whole together much increased. The skin frequently suffers this state, becomes excessively indurated and thickened. The brain will sometimes be hypertrophied: I never saw but one instance of this affection. I have seen many instances of partial induration, and cases where all the brain was very firm, but there was nothing except a hardening of the organ. In this instance, however, the brain had become larger than it should be; it caused the skull to be very much beyond the usual dimensions, and looked, on opening the skull, as if it had been ready to burst it asunder. The convulsions were all very large. This is a state described by many authors, and for the most part fatal disease of the brain is at length produced. This patient, a lad, who was exceedingly precocious, had a head larger than a man's, his character corresponded with that of an adult, and he suddenly became apoplectic, hemiplegic, and died. Nothing was to be found but an excessive size of the organ, the brain in other respects being healthy.

The spinal marrow has been seen hypertrophied. Nerves will fall into this state after amputation. The nervous ends will become very large, and frequently there is a morbid sensibility. Muscles are sometimes seen hypertrophied, but principally the heart. The most common disease of the heart is excessive nourishment of some portion, and that portion is by far the most fre-

quently the left ventricle. Blood-vessels as well as the heart will become hypertrophied, so that you have vascular tumors, of course filled with blood. On examining these tumors you find nothing but a congeries of blood-vessels. Some tumors of this description have been called *fungus hamatodes*, but it leads to a confusion of terms. By that disease we more particularly understand a combination of this state with a deposition of encephaloid, brain like, substance. Blood-vessels are often hypertrophied accessorially.

When a part is over-nourished, there usually is, I said, an increase of bulk, but an excess of nourishment may go only to condensation, and then you may not have a change of size, but only of consistence, and sometimes an organ which is over-nourished partly may be smaller than usual. It will sometimes happen when an organ is composed of more tissues than one, that one tissue will entirely waste, and the wasting of the one may more than counterbalance the over-nourishment of another. With respect to the cavity of an organ so hypertrophied, you may sometimes see it diminished, sometimes increased, and sometimes unaltered. That occurs in the case of the heart. Sometimes, if the heart be hypertrophied, the cavities enlarge; but sometimes the deposition of matter takes place outwards, and the heart really is enlarged, but the cavity remains much the same; occasionally, however, the deposition takes place inwards, and the cavity becomes greatly reduced. When a part is thus hypertrophied, sometimes the consistence remains the same, sometimes it is increased, sometimes it is diminished; and there is just the same variation in colour. Sometimes the colour remains natural, sometimes it is paler, sometimes it is heightened.

I believe that hypertrophy of a part is generally more or less of an inflammatory nature: certainly that is the case with the heart. Hypertrophy of the heart is in most instances entirely of an inflammatory nature, and indeed you must suppose that an excess of nourishment must be the result of an excessive action in the part itself—you must suppose that all the natural processes of the heart are going on with tenfold vigour. If a certain operation be necessary to the continuance of nourishment, and it is much increased, you will have an excess of nourishment. It is impossible to consider hypertrophy as any thing more than a sort of inflammatory state in a great number of cases; excessive exercise, however, of a part, will sometimes cause it to be over-nourished. We know that muscles will increase by exercise; a great excess of action of a part will sometimes cause them to acquire a bulk much beyond their natural dimensions. Still this is excessive excitement. It is chronic inflammation which causes hy-

perthrophy; *acute inflammation* rarely does, except so far as it lays the foundation for hypertrophy. Andral mentions that hypertrophy may sometimes arise from a defect of absorption—that the absorbents do not carry away the materials that they ought. An accumulation of nourishment takes place without undue nourishment being deposited. This, however, is but a mere hypothesis, and he calls it a want of de-assimilation—the want of removal of those particles which are already deposited, and have served their purpose. However, when a part falls into this condition of hypertrophy it is necessarily in a state of excessive activity, though perhaps without strength, and indeed no mark of inflammation may be discernible. Persons of the most sickly character frequently are subject to this in those unhealthy parts of the world where bronchocele exists, where the thyroid gland falls into a state of hypertrophy. Individuals, for the most part, in those situations are in a state of delicate health, sallow, shrivelled. So in rickets the ends of the bones enlarge very much; they are hypertrophied, but the case is one of debility. So in scrofula we frequently see parts become enlarged; you see the ends of the fingers in a case of decided scrofula much enlarged; the mesenteric glands, and also the under lip are much larger than they should be, and yet such persons are not instances of strong and increased vigour. Indeed, in the most unhealthy districts, the valleys of the Alps for example, you not only have the thyroid gland hypertrophied, but the liver, the tongue, the bones, and the hand, and yet the persons are pale and emaciated, and no sign of inflammation appears. Hypertrophy may induce very serious effects in the case of the brain; I believe that sooner or later apoplexy, paralysis, or epilepsy, ensue. In the case of the heart very great suffering is induced; you have palpitation from the slightest causes; you have a strong violent pulse generally, and at last dropsy.

Treatment.—The usual remedies for hypertrophy are the general remedies for inflammation; rest and low diet, moderate and repeated bleeding. But occasionally you are not justified in these measures, and you must support the strength. Of course, when this affection occurs in a cachectic habit of body, as rickets, or scrofula, or in state of cretinism in the Alps, any thing that depresses the powers of the body may do harm, and good air and good food are the best remedies.

Atrophy.

The opposite of hypertrophy is atrophy—wasting—and sometimes the part which wastes does not become smaller, but thinner than usual. Its bulk may be the same, but its texture may become lighter—more spongy than usual. Atrophy sometimes affects the whole of an organ, some-

times only a portion or a tissue, and sometimes the part will become thinner and thinner, till at last it is perforated. Parts that waste away are generally softer, and frequently paler than natural. Atrophy of parts is commonly induced by inflammation, or by violent irritation. The excessive nutrition of neighbouring organs will frequently cause one to waste; the excessive action going on in one causing the actions of another to decline. Wasting away of the testicle frequently supervenes on inflammation which followed the mumps. Andral mentions, that after suppuration near the liver, he saw the gall bladder waste away entirely.

Causes.—Atrophy is frequently an effect of bad air; of bad food; of depression of mind; of deficient exercise. As we must consider hypertrophy in general to be excessive function, so in atrophy there is a deficiency of circulation and action,—a deficiency of function.

Treatment.—As this is usually the very opposite state of things from hypertrophy, very opposite remedies are usually required. So far from the part being allowed to be at rest, it must be well used if possible, and also stimulated as far as may be.

[At the close of the lecture, the learned professor exhibited two recent specimens of diseased kidney. In the first the whole of the tissues were absorbed, except the outer and inner membrane of the pelvis and the intervening cellular membrane, and this had been all converted into bone. The second was an instance of greatly enlarged kidney, taken from a child six years of age, and contained scrofulous, cartilaginous, and bloody deposit.]

OBSERVATIONS

ON

PULMONARY HÆMORRHAGE;

Taken from a Clinical Lecture delivered at the Middlesex Hospital,

By DR. WATSON.

GENTLEMEN,—Two deaths have occurred among my patients during the last week. They both were the result of the same disease, and present some points worthy of notice.

Elizabeth L—n, aged 41, a cook-maid, was admitted on the 8th of Nov., for an eruptive eruption, of which I shall not detain you by saying any thing more than that it rapidly got better, and had, before her death, nearly disappeared, under the use of the zinc ointment.

It was soon discovered, however, that she laboured under disease of a much more formidable kind. She complained, on her admission, of some pain in the left side of the chest, of pain and tenderness at the pit of the stomach, and of cough; and stated that she expectorated daily a considerable quan-

tity of thick yellow phlegm, but that she had never spat blood. The cough had existed, more or less, for the last nine months—*i. e.* from the time when she had had an attack of “inflammation on the chest.” During this period she had been much more liable than before to “catch cold.”

Now these symptoms were by no means distinctive. They might all have been explained upon several different suppositions. She might have had pneumonia at the period she spoke of; or pleurisy; or inflammation; or some other morbid condition of the heart or pericardium; any of which might have led to changes in the organs of respiration calculated to produce the existing symptoms; or she might have been labouring all along under bronchitis; or the symptoms might be the result of tubercular disease of the lungs.

I may observe here, that she had not a phthisical look, and was not remarkably thin.

When I first had an opportunity of examining the matter expectorated, I saw much reason to fear that the last and least favourable of the suppositions just mentioned was, in fact, the true one. The expectoration consisted of globular, yellowish, separate masses, of peculiar appearance, floating in a thinner watery fluid. Sputa of this kind are strongly characteristic of phthisis pulmonalis; still they are not conclusive of its existence. The application of the stethoscope to the patient's chest removed all doubt about the nature and stage of the disease completely, and in less than a minute.

Below the left collar-bone, and close to the shoulder-joint, the air resired by the patient sounded as if it were blown through a hollow tube of some size, close to the ear of the listener. The voice also, loud and harsh, seemed to pass along the instrument. Neither of these peculiar sounds were heard, nor any morbid sound, in the corresponding portion of the right side of the chest. There was unquestionable *pectoriloquy* on the left side. I could no longer doubt that a tubercular cavity or cavities existed in the upper part of the left lung, and that consequently our patient (without at all suspecting it herself) bore about her a disease which must, in no long time, prove fatal to her.

She remained in the hospital till December the 3d. On that day, as the skin was nearly well, and the pectoral symptoms appeared stationary, it was arranged at the visit that she should leave the house on the following Tuesday. In the afternoon of the same day, during a violent fit of coughing, she brought up suddenly a large quantity of blood from the lungs, and presently expired.

The left lung, in its upper lobe especially, was found, as you know, full of tubercles. There were several cavities near its summit, communicating with each other and with the

larger air-tubes. A thin string of blood was traceable into one of these, the most remote from the trachea, where it ended in a small irregular clot. No vessels from which the blood might have proceeded were detectible. We once fancied, indeed, that we saw the orifice of a minute blood-vessel, but we could not satisfy ourselves that it was so; if it were, it must have been an extremely small one.

There were few or no marks of disease in the other lung.

In this case, you will do well to remark the facility with which the diagnosis was determined by auscultation, the occurrence of death during the first attack of hæmorrhage, and the absence of any rupture of a large vessel to explain that hæmorrhage.

The other patient to whom I have alluded was Daniel E.—n, a stableman, 57 years old. He came into the hospital on the 22d November, and died December 8th.

He complained of cough, which harassed him at night especially, or whenever he lay down. He said he had had a *bad* cough about three weeks; that he expectorated white phlegm; that, about a fortnight before, he had coughed up suddenly, and for the first time, nearly a pint of dark-coloured blood, and that he had spat blood in smaller quantities several times since.

He admitted that he had been subject to cough, every winter, for years; but it had never before “laid him up.” He stated that he had lately been obliged to sit up in bed for three nights successively. We always found him lying upon his right side, and he told us that any other position immediately increased his cough. As you saw these patients from day to day, I only remind you of the most prominent of their symptoms. You know that auscultation gave precisely the same results, the same unequivocal evidence of pectoriloquy, as in the former case; only here it was in the *right* lung.

Now this man was not emaciated, and bore in his countenance none of the characters of the disease of which he was dying.

You saw the result of the post-mortem examination. The right lung was loaded with tuberculous matter, and had a considerable cavity in its upper lobe. The left was comparatively sound. The cavity communicated with one of the principal bronchial divisions, by means of a large straight air-tube running obliquely from the cavity, downwards and inwards. This circumstance explains very satisfactorily the effect of position in emptying the cavity and exciting cough.

Here, again, you will notice the copious hæmoptysis occurring, for the first time, at an advanced period of the disease; and the decisive information furnished by a single application of the stethoscope, in regard to the real nature (of which we might otherwise have been uncertain) of our patient's

complaint. There are now four or five other instances of the very same thing amongst my own patients in our wards.

I shall take the opportunity afforded me by these two cases, and by another case which has also terminated fatally here during the last week, and of which I shall speak presently, to offer you some remarks upon pulmonary hæmorrhage; chiefly in respect to its pathology, and its general import as a prognostic symptom. You ought to know, that "spitting of blood" may depend upon several distinct morbid conditions: it will be my object to explain some of these conditions, and, if I can, to render your future observation of this symptom, whether here or elsewhere, more interesting and more instructive than it might otherwise be.

It is commonly said of a person who happens to spit blood, that he has broken a blood-vessel in his lungs. When blood flows profusely from an external wound, you may often see that it issues from a large vessel which has been laid open; and hence, by a very natural though deceptive analogy, you might be led to infer, that, in every copious hæmorrhage, whether from an external wound or from internal parts through some of the natural outlets of the body, the blood really escapes through an opening in the sides of some one (or more) considerable blood-vessel. This used to be held as good medical doctrine; but the progress of pathological knowledge has shewn it to be unsound. It is still the doctrine of the public, and it even continues to be sanctioned by the language and writings of many of the members of our own profession.

It is, indeed, quite true that some cases of pulmonary hæmorrhage do depend upon the rupture of vessels of a certain magnitude; but it is no less true that, in by far the greater number of instances, there is no lesion, detectable by dissection, of either veins or arteries; but the blood is poured out by (what is called) *exhalation*—i. e. it proceeds from those natural pores or channels which constitute, or are connected with, the ultimate ramifications of the minutest blood-vessels, and which are called *exhalants*. This is true, not only of bleeding from the lungs, but of internal hæmorrhage generally; with the exception, perhaps, of *cerebral hæmorrhage*.

The particular vessels injured in the first and smaller class of cases, and the nature and cause of the breach made in their sides, are matters subject to infinite variety. Sometimes the apertures through which the blood is extravasated are the results of a disorganizing process which has commenced in the coats of the vessels themselves; as when, for example, large aneurisms of the thoracic aorta, or of its primary branches, burst and pour their contents into the tracheal or bronchial tubes. Sometimes a large blood-vessel

is laid open by the gradual encroachment and extension of disease from contiguous parts.

It would be very natural for you to suppose, that where the hæmorrhage (as in the two cases before us) has been copious, and co-existent with tubercular excavations in the lung, the blood has proceeded from large vessels which had been laid open during the softening and expulsion of the tubercles, or by the subsequent extension of the ulcerating cavities. But, in fact, this is very rarely the case; and some modern writers have sought to explain the infrequency of hæmoptysis from this cause, by stating that the arterial tissue possesses a low degree of vitality, and thereby resists and avoids the destruction in which the surrounding tissues are involved. This explanation supposes that the bands which are not unfrequently seen stretching across tubercular vomica, are really blood-vessels that have escaped the disorganizing process. Such seems to have been the notion entertained by Bayle, and it has been more recently and more expressly advanced by Cruveilhier. But according to the more accurate observation of Laennec and Andral, the bands observed, in some cases, thus crossing the cavities, are composed of the natural tissue of the lung; condensed, indeed, and often loaded with tuberculous matter, but seldom containing blood-vessels. Laennec states expressly, that "he never found a vessel of any consequence included within the substance of these bands, which he resembles to the columnæ carneæ of the heart in appearance; that no such example occurs in Bayle's book, who had, however, mentioned to him, in conversation, *one instance only* in which fatal hæmorrhage took place from the rupture of a vessel that extended across a very large cavity." In the few cases in which Laennec had been able to detect blood-vessels at all in these bands, they formed but a small part of their thickness, were generally obliterated, and traceable for a very small distance only; the vascular appearance being soon lost in the pulmonary tissue of which the columns consisted. According to the same author, the tubercles press aside and separate the blood-vessels, which are often to be seen, of considerable size, lining, as it were, and forming a part of, the sides of a cavity; sometimes a little flattened, but seldom obliterated, while their smaller ramifications, which extend towards the excavations or towards crude tubercles, are become quite impervious. Dr. Baillie had in this country noticed previously the same condition of these vessels. "When blood-vessels (he says) are traced into an abscess of the lungs, I have found them, upon examination, very much contracted just before they reach the abscess, so that the opening of their extremities has been closed up entirely. On such occasions it will require a probe to be pushed with a

good deal of force, in order to open again their extremities. In these contracted vessels the blood is coagulated, as it is under similar circumstances in other parts of the body. This change in the blood-vessels is, no doubt, with a view to prevent large hæmorrhages from taking place, which would certainly be almost immediately fatal."

Andral, again, states that he had only once seen the orifice of a ruptured vessel from which the blood that filled a tubercular cavity had probably proceeded. This vessel was contained in one of the bands that had traversed the empty space, and had been broken. The mouth of the vessel was plugged by a small fibrinous coagulum, which was easily extracted, and then the pervious condition of the vessel became apparent.

Occasionally, but rarely, the cavities of which I have been speaking are found full of blood, though no lacerated vessel can be traced in them. More commonly the tuberculous or puriform matter which they contain, is more or less tinged of a red colour, from the admixture of blood. In both these cases, the blood seems to be the result of a sort of exhalation from the sides of the cavity.

All the examples of bleeding from the lungs that I have hitherto mentioned, may be looked upon as illustrations of the primary species of symptomatic hæmorrhage—of hæmorrhage, *i. e.* symptomatic of organic disease in the very part from which the blood proceeds.

But, in a far greater number of cases, the blood, in hæmoptysis, is *exhaled* from the mucous membrane that lines the air-passages; for, when this surface is examined in the dead body, and immediately after the occurrence of pulmonary hæmorrhage, it is often found to be perfectly entire, from the commencement of the trachea to the remotest divisions of the bronchial tubes; as far, at least, as minute dissection can follow them. The membrane in such cases is sometimes partially red—sometimes pale, or with scarcely any trace of vascularity. The former of these appearances results from the continued turgescence of the capillary vessels; the latter is the consequence of their having been completely emptied by the ultimate hæmorrhage. All this is in strict analogy with what is well known to occur in hæmorrhages from other mucous surfaces, and especially from that of the alimentary canal.

When blood is thus exhaled from the mucous membrane of the air-passages, the hæmorrhage may be purely *idiopathic*, *i. e.* it may be independent of any discoverable alteration of texture, either in the mucous surface itself, or in any other part which, by reason of some intelligible connexion of structure, or relation, seems capable of influencing the capillary circulation of that

membrane. But the occurrence of pulmonary hæmorrhage strictly idiopathic has probably been more frequently affirmed than proved. Active hæmorrhage from the lungs is stated by systematic writers to be the hæmorrhage of adolescence, as epistaxis is that of childhood. I believe, however, that idiopathic hæmoptysis is very rare indeed, unless we may consider as such certain cases of *vicarious* hæmorrhage, which are sufficiently common. Andral informs us, that, in one instance only, in which hæmorrhage from the surface of the air-passages had been the immediate and apparently the sole cause of death, had he ever found the substance of the lungs free from tubercles and perfectly healthy. He does not, however, state whether in this instance the *heart* also was in its natural condition—an important omission, as I shall presently shew you. He relates, indeed, as an example of idiopathic hæmoptysis, the case of a young man who suffered profuse hæmorrhage from the lungs on four several occasions, between the age of twelve and eighteen, without any apparent detriment to his health, which remained excellent. It is consistent, however, with much experience, to suppose that crude tubercles might have occupied the lungs in this person, and have sufficed, on the application of some exciting cause, to determine the hæmorrhage, although as yet their presence was not indicated by any other sign. Almost every systematic writer quotes, as an example of idiopathic hæmorrhage from the lungs, the story of a Roman governor (mentioned by Pliny) who lived to the age of ninety, though he was afflicted with habitual hæmoptysis. The frequent citation of this supposed instance is of itself a sufficient proof that spontaneous pulmonary hæmorrhage is far from being common.

Cæteris paribus, the disposition to pulmonary hæmorrhage is increased by whatever tends to diminish the capacity of the thorax, and to compress the lungs. And it is on this principle that some have attempted to account for the frequency of hæmoptysis in persons with crooked spines—in tailors who sit continually in a bending posture—in young women who apply tight lacing round the lower part of the chest—and in those who labour under dropsy of the belly. Hæmoptysis accompanying ascites has been known to cease at once on the performance of the operation of tapping, and to recur upon the re-accumulation of the dropsical fluid; and this not on one occasion only, but so often and regularly as to preclude all notion of accidental coincidence. There can be little doubt, however, that in this class of cases, or at least in a vast majority of them, the hæmoptysis is mainly to be ascribed to organic disease of the heart or of the lungs.

There is a form of pulmonary hæmorrhage

—to which I have already alluded as being common—which is sometimes called constitutional, and which is vicarious of some other natural or morbid discharge, and most frequently of all of the menstrual discharge in females. If this cannot strictly be regarded as idiopathic, it may be considered as forming a link of connexion, as lying midway, between symptomatic and idiopathic hæmorrhages. Some remarkable examples of this kind of hæmoptysis have fallen under my observation in this hospital. It is sometimes, but rarely, attended with danger to life. There are many very curious and well authenticated facts upon record concerning this singular form of *hæmorrhage by deviation*, as it is sometimes called. Women have been known to menstruate in this way, if the expression be allowable, through the lungs, throughout nearly the whole of that portion of life during which that function usually continues; and this without any impairment of their general health. You will find such cases detailed by Pinel and by Hoffman. At present I can only thus briefly allude to them.

It is, however, a melancholy fact, that the hæmorrhage which takes place by exhalation from the mucous membrane of the air-passages, is dependent, in a very large proportion of instances, upon incurable organic disease. Such cases belong to the secondary species of symptomatic hæmorrhage; the disease of which it is symptomatic being seated elsewhere than in the very membrane whence the blood escapes. The complaint of which hæmoptysis is by far the most frequently symptomatic, is undoubtedly tubercular phthisis. Where the tubercles are found upon dissection to be yet crude and entire, and no breach can be detected in the mucous membrane, no question can be entertained about the immediate seat of the bleeding; and even where cavities exist, especially if they contain no blood, it is probable that in most cases the hæmorrhage has had a similar origin.

When hæmoptysis is thus actually symptomatic of tubercular disease of the lungs, it is liable to considerable variety in regard to the period of its first occurrence, and the symptoms by which it is succeeded.

There are many persons in whom the first attack of hæmoptysis precedes, for months, and even for many years, the primary symptoms of unequivocal phthisis.

There are others in whom the first attack of hæmoptysis is immediately followed by all the well-known signs which declare the existence of tubercles in the lungs.

Many, again, do not spit blood until the tubercles have reached a considerable degree of advancement, and the phthisical symptoms have been for some time clearly marked; and occasionally in such cases, as in the instance of our female patient, the first hæmorrhage proves fatal.

Lastly, it is far from being an uncommon thing to see pulmonary consumption run its whole course, and terminate in death, without having been attended with any expectoration of blood.

Andral gives the following statement as the result of his own observation, in regard to the relative frequency of these several modes of connexion between hæmoptysis and consumption:—

Of the persons whom he had known to die of this disease, one in six never spat blood at all. Three in six, or one half of the whole number, did not spit blood until the existence of tubercles in the lungs was already made certain by unequivocal symptoms: in the remaining two-sixths the hæmoptysis preceded the other symptoms of tubercular disease, and seemed to mark the period of its commencement.

By this comparative statement you will see how very frequently hæmoptysis occurs as one of the symptoms connected with tubercular phthisis. Under this author's observation it happened in five cases out of six. In Louis's experience, however, the proportion, though very large, is not quite so great as Andral found it. Among 87 instances of consumption there were 57, or four in every six, in which hæmoptysis had been present.

You may perhaps wonder that I so frequently refer to these French authorities. I do so for two reasons; 1st, because I wish to excite your attention to their writings, which (those of Andral and Laennec especially) are full of valuable and interesting matter: and 2dly, because they supply me with what is much wanted among ourselves, viz. conclusions drawn from trustworthy numerical statements made on a large scale. The practical information to be derived from tabular accounts of this kind is perhaps more useful, as it is certainly more secure from fallacy and suspicion than almost any other.

It has been made a question whether the spitting of blood that occurs in connexion with tubercular phthisis, is always to be considered as indicative of the existence of tubercles in the lungs; or whether it may not sometimes precede and give occasion to their formation in those organs. This question has evidently been suggested by those cases (constituting, according to Andral, one-third of all that happen) in which the ordinary signs of phthisis begin to manifest themselves immediately upon the occurrence of the primary hæmoptysis, or a short time afterwards. Morton, who has noticed this kind of pulmonary hæmorrhage, includes among his species of phthisis, the "phthisis ab hæmoptoe;" and Cullen held that spitting of blood was often the cause of pulmonary consumption. It is a very important question, and though I cannot enter fully into its discussion now, I shall, if our time will let me,

mention certain facts that bear upon its determination, before I conclude this lecture.

[To be continued.]

THE PRACTICE OF MEDICINE, SURGERY, &c.

Among the New Zealanders and Natives of some of the Polynesian Islands.

By GEORGE BENNETT, Esq. M.R.C.S. F.L.S.
&c. &c.

[Concluded from page 439.]

NATIVES appear to have no compassion one for the other. When suffering under sickness, more particularly if they consider the disease irremediable, they would sometimes remove the sufferer to a separate hut, erected on purpose for him, where he usually perishes from neglect. Sometimes, if the sufferer lingers for some time, they dispatch him with their clubs and spears; and even would bury them alive, but this was only done when the patient was considered past recovery*. In India this indifference to a person suffering from disease also exists. In that country it has sometimes reference to caste, but more commonly to that apathy so usually found among natives. Bishop Heber mentions an instance of a friend of his "who some months ago found a miserable wretch, a groom out of employ, who had crept, sick of a dysentery, into his court-yard. He had there remained in a corner on the pavement two days and nights. Perhaps twenty servants had been eating their meals daily within six yards of him, yet none had relieved him, none had so much as carried him into the shelter of one of the out-houses, nor had any taken the trouble to tell their master. When reproved for this, their answer was, "He was not our kinsman." "Whose business was it?" "How did we know that the sahib would like to be troubled†?"

* When I visited the Bay of Islands in New Zealand in July 1829, I was requested to visit a native female labouring under illness, (occasioned by an injudicious use of mercury, given to her by an European). She had been removed from her usual dwelling by her relations, and I found her lying in the open air during the time a temporary hut was erecting for her reception, (and which, on the event of her dying, would have been burnt): this was usually done when the disease was expected to have a fatal termination.

† Heber's Journal on India, vol. 3d, pp. 262-63.

When I had a number of the natives of Rétinna suffering under intermittent fever on board the ship *Sophia*, those who had relations received every attention from them as long as they were not past recovery; those, however, so kind to their relations, would see the death of others with the most perfect indifference, and would not even administer to the wants of the helpless; and when I requested them to give their assistance to one of their suffering countrymen, if they did comply, it was with the most apparent reluctance; indeed, they would frequently require an exercise of authority to compel them; and how miserable was that benevolence which was forced! At this time (13th March, 1830) when we had so many sick on board, there was among them a number also of natives of Tongatabu. One named Tuutualei, who was past recovery, but lingered for some time, was thrown overboard by his countryman; he was picked up again by the boat of the schooner, *Minerva*, which was in company with us at the time. On remonstrating with the natives for this act of barbarity, they were unconscious of having done wrong. As he still lingered several days after this, he was observed by one of the officers carried into the chains by his countrymen, who intended again to throw him overboard, as they said he was past recovery. It was not permitted, but poor Tuutualei died shortly afterwards, and his body was then committed to the deep. Frequently I have seen a poor native tottering along the deck with his coco-nut shell for water, without any one of the natives in health attempting to render him the least assistance, but amused themselves by ridiculing his tottering gait and emaciated appearance. Our sailors, however, set them a better example, for they always expressed a readiness to assist the helpless natives.

Natives are apt to exaggerate accidents, and so are the lower class of people in this country, proceeding from a rage for the wonderful, or in order to excite attention. I was sent for in great haste one evening at New Zealand to attend the brother of a chief, who it was said by the messenger "had crushed both his legs to atoms by a spar falling across them." "When going on shore, I was considering of amputation, and if my patient died under the operation, what his countrymen would think of it. On arriving on shore, I was ushered into a

small hut, in which I found my patient, and his wife was assiduously attentive to her duties in attending on her unfortunate husband. The "crushed legs," however, did not exist. I found the injury he had sustained was a severe contusion from a rope getting across the thigh when he was employed in dragging down spars. This patient soon recovered.

At New Zealand not a single case of scrotal enlargement, or elephantiasis, came under my observation; but this disease prevails to an extensive degree at the Polynesian islands: its cause is difficult to be accounted for: Europeans become afflicted with it as well as natives after they have resided some time on the islands. At Tahiti I became acquainted with a worthy missionary, who has now attained a good age: he was unfortunately afflicted with this disease. He informed me that he suffers no pain until (at no certain periods) the paroxysms come on, the first indication of which was pain in the groin, passing to the knee and leg, but sometimes the pain in the groin and leg commenced at the same time; this was followed by a swelling of the inguinal glands, attended by febrile symptoms. Nausea, vomiting, and sometimes delirium, follow the attack. Active purgatives, taken on the first symptoms appearing, tend to mitigate the paroxysms. During the severe pain, opium is obliged to be taken to obtain relief. After the paroxysm has passed, a lingering but slight pain will remain for some days after, attended by much debility. Exposure to cold predisposes the person to a paroxysm. It attacks females as well as males; and a lady at Tahiti informed me that she was desirous of removing to Sydney, New South Wales, as she had for some time felt symptoms of an attack. It is remarked that persons living inland are not so liable to the disease as those resident on the coasts: this has been remarked in Ceylon and other parts of India*. At

Tahiti I saw a young man, of eighteen years of age, born of European parents, who had commenced suffering from this disease. The native name of this disease at Tahiti is *féfé*.

Change of air is considered beneficial to the disease, and frequently arrests its progress. Mr. Marshall observes, that "Persons labouring under elephantiasis generally obtain a suspension of the progress of the disease by a removal to a part of the island where it is not endemic. An inhabitant of Galle had, at regular intervals, the febrile paroxysm, and a concomitant effusion in one of his legs. He changed his place of abode from Galle to Jaffna, where he resided about a year. While at Jaffna he had not a single paroxysm of the disease. At the termination of the above period he returned to Galle, and had the usual febrile paroxysm in a few days after his arrival*."

Hydrocele is a very prevalent disease among the natives of Tahiti; bronchocele also exists, but is not common. I remarked also several young men afflicted with a partial paralysis of the lower extremities, which is not uncommon. During my stay at Tahiti, I attended several natives afflicted with abscesses of enormous extent. At Mata-vai district I visited a female who had an abscess extending from the ear to the nose, and almost closing the eye of the right side. I made an extensive incision with my lancet, and gave exit to about two pints of matter, after which my patient expressed herself much relieved: a yam or breadfruit poultice was directed to be applied. On entering another native dwelling, the patient I was to have seen no longer existed; she was a fine and beautiful young woman, with her head decorated with flowers, and had, the day previous, complained of a pain in the chest, for which a blister had been applied by one of the missionaries. It was difficult to account for her death, and the body was not permitted to be opened.

There can be no doubt but that an injudicious use of mercury, formerly administered to the natives by persons incompetent of judging whether the disease they were treating was or was not syphilitic, has been the cause of much injury to the constitutions of the natives. I saw, at Tahiti, several natives afflicted with ulcers, which were declared by

* Mr. Marshall observes, that "some classes of people are much more liable to the disease than others. The half-caste race and the pure Singalese are particularly obnoxious to it. In Galle, I suppose nearly one-half of the adults of the former class are more or less affected with morbid enlargements of some parts of their body. The descendants of unmixed Europeans are liable to be affected. Only one instance is known of a person born in Europe having had elephantiasis in Ceylon. This individual was a Savoyard, and had resided about forty years in Galle. Malays, Caffreys, and the natives of the peninsula of India, appear to be little liable to the disease."—*Diseases of Ceylon*, p. 225.

* Marshall on the Diseases of Ceylon, p. 226.

those who usually treated these cases (the missionaries) as proceeding from venereal causes, and the patient underwent, accordingly, a mercurial treatment; at the same time the ulcers had not that marked appearance which the venereal ulcer assumes. It is a remarkable fact, that, so much as we have heard of the spread of the venereal disease, and the devastation it has caused among the Polynesian natives, that in the ship to which I was attached, with a crew of nearly fifty, having an indiscriminate intercourse with the females of the Islands which we visited, and for a period of fifteen months at New Zealand and Polynesia, I had only five patients on my list for venereal, one gonorrhœa, and four syphilitic; and the syphilis was so mild in its character (which we may attribute to the cleanliness of the females where the disease was contracted) as to require the administration of but a very small quantity of mercury. The case of gonorrhœa occurred at New Zealand, the cases of syphilis at the Island of Tahiti.

I have observed a cutaneous disease which prevails extensively at Tongatabu, where it is named *tona*; at Tahiti, where it is named *bua*; and the Sandwich Islands, where it is named *pupu*; and it exists also, I am informed, at the Fidgi and Navigator's Group, &c. It is contagious; and at Tongatabu they state that every individual is subject to it, either at childhood or at attaining manhood: if they have it when young, it assumes a milder character, and is more readily cured than when it attacks them at a more advanced period of life. It first appears as a pimple at different parts of the body, more usually on the lower extremities. These pimples suppurate and form blotches, which discharge an offensive matter, and becomes encrusted over; the encrustation falls off and the discharge is renewed, forming a long, tedious, and disagreeable sore. At Tongatabu the method of cure adopted by the natives is by an application of herbs called *Kumu-kumu*, *Moriugu*, *Mohugu-apoa-apoa*, &c. the leaves of which are pounded and mixed with a kind of clayey earth named *Kéré*; the whole is boiled, and when cold applied to the sores.

At Oahu (Sandwich Islands) I had a case of this disease under my care, the subject of which was an Euro-

pean. The disease first appeared in small pimples, of a red colour, which, suppurating, discharged an ichorous matter, which soon became thicker and more offensive, spreading and forming large blotches, which dried up occasionally, and the scab falling off, the discharge was renewed. I treated the disease with the compound sulphur ointment, but it was a long time before it was cured; the scars from the eruptions always remain. This disease is named by the Europeans at the Sandwich Islands, the *itch*. It only itches, however, in a slight degree at the commencement of the disease.

Acute ophthalmia, and ophthalmia tarsi, are prevalent at some seasons of the year at New Zealand, and I have observed these diseases at most of the Polynesian islands. The purulent ophthalmia was very prevalent at the island of Rótuma among the children, increased, however, by the want of necessary cleanliness, from what cause I could not ascertain; but it was with the greatest difficulty that the mothers could be persuaded to use lotions for their children's eyes.

Among the females at the Sandwich Islands, from their licentious habits, leucorrhœa is very prevalent, as also ascites abdominalis. I attended, in conjunction with my friend, Mr. T. C. Hyde Rooke (now established at the Sandwich Islands), a case of ascites in a female of about twenty-two years of age. She stated (on the 14th of January, 1830) that, about three months since, she was attended by a native doctor*, who first applied steam baths to promote profuse perspirations, administering strong drastic purgatives at the same time, which were so violent as to occasion violent vomiting as well as purging. The swelling of the abdomen diminished under this mode of treatment, when the native doctor left her, being called away to attend a chief who was ill at a distant part of the Island. The water again accumulating, she placed herself under the care of my friend, Mr. Rooke, who performed the operation of paracentesis, and drew off about twelve quarts of

* Some of whom are very skilful in the administration of their native remedies, and treat cases successfully which have baffled European skill. Some of the natives apply to European surgeons, others prefer their own physicians.

water. She bore the operation well, and in a few days was enabled to resume her usual occupations, delighted with the relief the operation had afforded her.

Epidemic diseases occasionally prevail among the Polynesian islands; one is mentioned as a kind of influenza, which spread, and proved in numerous instances fatal, among the Society Islands; it affected the lungs and throat, and loss of voice often followed the attack. It is stated that "this kind of calamity has been frequently experienced in the islands since they have been the resort of foreign shipping, though we are not aware that it prevailed before." I can find no proof why it should be considered of foreign introduction, when epidemics are known to prevail without any attributable causes but those of atmospheric vicissitudes. In the same manner dysentery has been attributed to contagion from Vancouver's ship; but it is also stated, that "these diseases have generally passed through the islands from the east to the west, in the direction of the trade winds." With respect to dysentery, it prevails among most of the Polynesian islands to an extensive degree, and proves very fatal; but I am inclined to consider this fatality to be occasioned by the want of proper medicines to check the progress of the disease, as it prevailed at Tongatabu and the island of Rótuma during my visit in 1829 and 1830, and its progress was speedily checked by appropriate medical treatment.

At the Island of Rótuma, more particularly, I had numerous cases of this disease under my care; among others, a native named Kio (or, according to the English name bestowed upon me, Sampson), who came on board the ship *Sophia* on the 27th of February, and accompanied us to the Island of Erromanga. He applied to me on the 2d of March for medicine, as he had been suffering for the last week on shore, and since he had been on board, from an attack of dysentery. He observed to me, in his English, "Very bad, too much blood come; very bad disease at Rótuma—kill man, woman, and child." He did not attribute the disease to European origin, but said only that it was very prevalent and fatal at Rótuma. I treated his disease with calomel and opium, and on the 6th of March he was per-

fectly cured. "If at Rótuma," he said, "I should have been put into the ground, disease so very bad; now plenty well." It is to be regretted that this disease commits such ravages among the Polynesian natives, merely from the want of medical assistance*.

Among the native medicinal plants at the Island of Tahiti, was a parasitical plant called Mavi, by the natives *Dendrobium teretifolium*. The leaves are round, narrow, and fleshy. I found it bearing its white flowers in October 1829. This plant is used, as an external application, in acute pains in the head, or in any other part of the body, by bruising the leaves; and it is said to readily give relief†.

The Tahitians have also a plant named taa-taa-hiara, which they used medicinally. It is pounded up, mixed with a certain proportion of water, strained, and is sometimes also mixed with other herbs and oil, and is used as an external application in rheumatism, &c. It is applied on the fibres of the mou-haari (*cyperus* sp.), to keep it moist‡. They also use a species of *cyperus*, called by the natives mou-niu, for recent wounds. The plant is bruised, or chewed, and applied to the wound, and seems to have a stimulating property.

At Tongatabu, among their medicinal plants there is one called by the natives Uû, the *Fagara euodia* of Forster. The leaves have a powerful but not unpleasant smell, and are used by the natives both as an external and internal remedy in various complaints. When suffering from headaches they take it internally, and they apply it externally to swelled legs, ruptures, or any external wounds or bruises. The method of preparation for administration internally, is as follows:—The leaves are pounded, and water gradually added, and, after being well worked up for a few minutes, it is

* It was at the island of Rotuma only where a fee was tendered to me for medical assistance, which usually consisted of one of the mats of native manufacture. The most curious fee offered was by a chief, who seemed deeply to regret the loss of an only child from dysentery. "If I knew doctor on board," said he, "I would send for doctor to visit my child, and give him as present, if he make my child well, 'two hogs and two wives!'"

† A specimen of this plant, which I gathered on the 6th of October, 1829, was so tenacious of life, that, although hung in the sun, it changed from flower into fruit, and did not die until the 7th of November, 1829.

‡ The fibres of the stalk of this latter plant are used for straining kava, arrow-root, &c. among the Society Islands.

strained, and the remaining liquor is ready for use. As an external application, the leaves are simply bruised and applied as an embrocation, or poultice. From the powerful smell of this plant, the native females use it in the formation of their kakala, or necklaces.

Among the medicinal plants of the Sandwich Islands, I remarked a small plant which grew abundantly on the hills, and was called Moa by the natives, *Psilotum triquetrum* (allied to *Lycopodium nudum* of Linnæus). It is used medicinally, in the form of an infusion, in visceral disorders: a large tumblerful of the infusion is considered a dose.

They have also a shrub, an euphorbia (of which there are two species, one of which is remarkable), named Atoto, or Akoko*, by the natives; the milky viscid juice of which is used as an external application for ulcers. I saw it used in a case of a sailor belonging to the American whale-ship, the Dawn, by one of the native doctors at Honōruru (Oahu). The juice, with much trouble, was squeezed into a calabash, and then spread over the surface of the ulcer. After this application had been used for some time, the only benefit the ulcer appeared to have derived from it was the removal of the fœtid odour of the discharge; it did not in the slightest degree promote granulation. This milky juice had a pleasant sweetish taste, and was destitute of acidity.

We find charms, &c. considered as efficacious in the cure of diseases, whether among the African nations, Asiatics, or Polynesians, and it still exists, even in this enlightened age, in several parts of the united kingdom, more particularly in the north, where herbs, administered with suitable prayers, possess a wonderful power of curing the most inveterate diseases. Whether the individual is a Polynesian savage or an European, if the mind is uncultivated, we find the same credulity, the same barbarism existing.

Among the wounded that came under my care, after the attack made by the natives of the Island of Annatou, New Hebrides group, on a native gang, was a Tahitian, named Fatiau: he had received a spear wound in the lumbar region on the right side, passing in an oblique direction upwards; the head of the spear had broken into the wound,

and was extracted soon after: the wound healed speedily and well. When I saw him on the 21st of April, 1830, at the island of Tanna, the original wound made by the spear was perfectly healed, but an abscess of an enormous extent had formed about the ninth and tenth ribs: from its fluctuating feel, matter seemed to have formed, and I laid it open to its whole extent, so as to expedite the discharge of the matter: rather more than a pint of pus was discharged, and the wound displayed a formidable gap. After all the matter was pressed out from the lower part of the abscess, a quantity of which was lodged under the integuments, a yam poultice was applied, and my patient expressed himself as quite relieved. I ordered him a purgative draught. In the evening he felt perfectly easy; another poultice was applied, and, from the discharge of matter, was ordered to be frequently renewed.

On the 22d he came on deck in excellent spirits, and visited me in my cabin: he had slept well during the night, which, from the pain he suffered during the suppurative process; he had not been able to do for several nights previous. The wound looked well, and the discharge from it had greatly diminished: the bowels had been freely open.

24th.—There is but a trifling discharge of matter, and the wound is granulating well; appetite good, and bowels regularly open.

25th.—The wound was rapidly healing by healthy granulations; the lower part was nearly healed: this shows how rapidly the healing process takes place in a young native, which, in an European, would have been both tedious and dangerous. On this day we sailed for Manilla, and my patient returning in another vessel (the Alpha) to his native country, I was obliged to leave him under charge of Capt. Henry, with directions for his treatment. From the healthy appearance of the wound, and the rapidity with which the healing process had taken place, he was no doubt in a few days perfectly cured. The applications to wounds in natives require to be of the most simple kind, cleanliness being alone requisite to aid nature in her cure.

In conclusion it may be observed, that experience in the treatment of diseases, and attention to the constitution

* At the Sandwich Islands the natives use indiscriminately the t, k, r, and l.

of the patient, is the only method to ensure success. "All our knowledge," observes Lord Bacon, "is derived from experience:" natives, in their treatment of diseases, (judging, probably, of the rapidity with which organic disease goes on in a tropical country,) frequently adopt desperate and excessive doses, when such a mode of treatment among ourselves would be regarded as hazardous; and, I have no doubt, that many of our diseases are permitted to assume a severe character, from a timidity in the administration of remedies at the first onset. Again, the remedies used by the natives are indigenous to their country, and the medicinal virtues they possess are rendered inert by being kept for some length of time in a dried state. Many of the native remedies are well worthy of attention: at the Sandwich Islands, some of their medicines have been highly extolled by Europeans, "Whence," inquires Mr. Locke, "comes the mind by that vast store which the busy and boundless fancy of man has painted on it, with an almost endless variety? Whence has it all the materials of reason and knowledge? I answer, in a word, from experience. In this, all our knowledge is founded—from this, the whole emanates and issues."

London, Nov. 1, 1831.

LEUCORRHOEA, WITH AFFECTION OF THE KNEE SIMILATING RHEUMATISM.

By MR. SMART, OF CRANBOURN.

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To the Editor of the London Medical Gazette.

SIR,

Mrs. W. æt. 32, a delicate person, a keeper's wife, always subject to irregularity of menstruation and confinement of bowels,—mother of three children, with the interval of six years between each birth,—about two months ago, in consequence of fatigue and damp, was seized with menorrhagia and leucorrhœa; she kept about for a fortnight subsequently, and then was obliged, with an apparently inflammatory swelling of the right knee, to take to her bed, where she has been ever since.

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She was attended at first by a gentleman who treated her complaint as acute rheumatism, without relief. Five weeks ago she became my patient, and I fell into the same error. The joint was considerably swollen; very painful—constantly so, but more at night; very tender; *not* red. She said it was preceded by flying pains in various parts. She took colchicum, calomel with opium, steel, purgatives, but all without benefit. By this time I well began to doubt the correctness of my diagnosis, and my attention was directed to the situation and course of the pain she suffered in the whole extremity. A small blister was put on the inside of the knee, the surface dressed afterwards with morphine; but this, with leeches, lotions, and fomentations, was without avail. Her bowels were kept regular under the use of castor oil. She latterly informed me of a copious fetid discharge from the vagina, with great pain in the loins, which had existed a long time. I found the os uteri turgid and very tender. Then it struck me that the disease of the knee might be connected with this affection. Some femoral glands were enlarged and tender, and she pointed out the course of the pain from *Poupart's ligament, along the inside of the thigh, round the inner condyle of the femur* (where it was most acute,) *to the front of the tibia, down to the foot.*

October 2d.—R Argenti Nitratis \mathfrak{ij} solve in Aquæ \mathfrak{zvj} . to be applied to the cervix uteri several times a day, by means of a piece of sponge attached to the end of a bonge.

5th.—Remarkable change for the better. The pain, with its depressing influence, she had borne so long, being *now entirely removed*, her spirits and strength are regaining their natural elasticity. She is able to bend the joint, which she had not done for the last five weeks; she can change her posture in bed with facility; the slightest motion before caused acute pain. The knee feels stiff; less swollen; less tender. The lotion gave no pain.

Pergat in usu lotionis.

12th.—Is down stairs; sits up all day; complaining of great weakness in the loins; discharge "next to nothing;" not fetid; the knee of its natural size, but she is unable to extend the leg; the thigh still tender; occasional dart-

ing pains down the leg to the foot ; appetite good ; bowels regular.

Pergat.

24th. — Gaining strength ; walks lamely.

Nov. 17th. — After straining herself in lifting, &c. discharge is apt to gush from her. Lumbar weakness nearly gone.

29th. — No more discharge ; has had daily a "sign" of the catamenia ; has left off using the lotion.

Dec. 26th. — Quite recovered : has had a regular return of the catamenia. The limb sound.

Observations. — This is a case of irritable action of the lumbar nerves, occurring in a susceptible constitution, accompanied by profuse leucorrhœa and an inflammatory state of the neck of the womb, which in all probability re-acted on the original cause, exasperating it, prolonging it, and tending to propagate its morbid actions to parts remotely situated. The saphena nerve bordering on, if not actually in, an inflammatory condition, was the medium of this communication. In its course over the knee joint, being tied down by the tendinous fibres spread over the capsule, that restraint would prove an additional exciting cause of irritation to the neighbouring structures.

Hence a disease of the knee-joint closely resembling inflammation, which, however, I would regard more in the light of neuralgic irritation, differing from inflammation, in resisting every method of treatment, but that which was adapted to remove disease from a distant part.

This case, too, bears testimony to the good effects of the nitrate of silver, as recommended by Mr. Jewell.

December 27, 1831.

NITRATE OF SILVER IN LEUCORRHOEA.

To the Editor of the London Medical Gazette.

Sir,

IN the use of the nitrate of silver in solution for the cure of leucorrhœal discharge, it has often been remarked to me that some slight decomposition

takes place when the pewter syringe is employed. This, indeed, is so obvious as scarcely to require comment. In order to remedy this effect, I beg permission to state that I have suggested a modification of what is usually designated Clarke's syringe, which appears to answer the purpose extremely well.

After having given a trial to the nitrate of silver, in its various forms, and in almost every species of vaginal discharge, I cannot but reiterate the opinion given in my little work of last year, — that I consider it one of the most efficient remedies which can be employed, when the disease is not dependent on absolute structural change. Even, however, in the advanced stage of organic disease, its application is often attended with considerable relief and comfort to the patient. To prove that my anticipations, when I first introduced this remedy to the notice of the profession, have been in a great measure realized, permit me to quote a few words from the admirable *Clinical Lectures of Dr. Elliotson*, as reported in the different medical periodicals of last March : — " I am quite sure that the nitrate of silver forms one of the best injections you can employ in these cases (leucorrhœa). I will not say it is better than any, but I am quite sure that it is inferior to none, and better than many." Enough, sir, I trust, has now been said to induce my professional brethren to employ this remedial agent in such cases as those to which I have alluded, and I do not hesitate to express my firm belief, that in most instances their practice will be crowned with success. I could detail numerous cases in which it had effected a permanent cure, when almost every known remedy had been employed without benefit to the patient ; at the same time it is candid to observe, that I have experienced disappointment, which, indeed, must occasionally occur to every one in the use of the most valuable articles in the *materia medica*.

I remain, sir,

Your obedient humble servant,

GEO. JEWEL.

24, Sackville Street, Piccadilly,
January 23, 1832.

HYDROCEPHALUS IN AN ADULT.

To the Editor of the London Medical Gazette.

SIR,

THE following case of hydrocephalus internus in an adult, of unusual duration, seems to be worthy of record.

In October 1825, James Burchell, aged 24, a strong and healthy young man, was seized with violent headache, and all the usual symptoms of the inflammatory affection of the brain which usually terminates in hydrocephalus. He underwent the usual treatment of copious bleeding, leeching, cold applications to the head, and active purgation; but in spite of these, and of the subsequent use of mercury and diuretics, in about five or six weeks he became perfectly blind, and so he remained, without any other diminution of sensual, and no abatement of intellectual power, up to the time of his death.

His general health, after the acute stage of his disease, was gradually re-established, and he was able to walk abroad; but his headache, though alleviated, never left him. After this stage, also, his natural functions were never much disturbed, and he was rather full of flesh till within the last few months, when he became restless, declined in health and gradually sunk, and died on the fifteenth instant, six years and a quarter from the commencement of the disease, retaining his mental faculties to the last.

For a year or two after the commencement of the disease he had occasional epileptic fits, but they also left him.

The general aspect of the cranium externally after death, gave the idea of what would be called, in a child, a form predisposing to hydrocephalic disease, a slight expansion laterally, but certainly so slight that it might have passed unobserved if the subject had died of any other malady. The cranium was remarkably thin, but the sutures had not given way in any part.

The lateral ventricles were enlarged to nearly treble their natural size, and the third and fourth were also enlarged, but not in the same proportion. The foramen commune was three-quarters of an inch in diameter, and about eight ounces of limpid fluid was contained in these cavities. The membranous lining of the ventricles was almost colourless,

showing no signs of recent inflammation; the choroid plexus was also sunk and pale. The thalami were less prominent than usual, and the septum lucidum three times its usual breadth. A tumor about the size of a hazel-nut projected from the hind part of the right thalamus, covered by the membrane of the ventricle, and imbedded in the medullary substance; it presented a fungoid, granulated, cheese-like appearance, and was inclosed in a capsule of a remarkable pearly aspect. The other parts of the brain and cerebellum presented nothing very remarkable.

Absorption, or condensation, of the medullary substance of the cerebrum, seems to have made way for the enormous enlargement of the ventricles and the contained fluid. The posterior cornua of the lateral ventricles had been pushed so far backward, as to leave only about a quarter of an inch between them and the pia mater.

The only practical inference from what is here observed seems to be, the possibility of tapping such a dropsy through an opening in the superior part of the occipital bone. The early history of the case convinced the attending surgeon that it was one of simple arachnitis in the first instance, and that the growth of the fungus was incidental, and perhaps of recent date.

After blindness was established, the patient's health being in great measure restored, and the disease become chronic, it seems probable that a needle-shaped canula might have been guided into the posterior part of one of the ventricles without much risk, and not without some hope that, by relieving the tension of the brain, opportunity might have been afforded for the establishment of a natural sanitary process.

WM. WARDROPER.

Pulborough, Jan. 17, 1832.

LITHOTRITY.

To the Editor of the London Medical Gazette.

SIR,

THE operation of lithotritry, which has been introduced into this country, in a much improved form, by M. le Baron Heurteloup, has, in his hands, been

completely successful in relieving the sufferings of a patient in this neighbourhood *. The operation was every time performed in the presence of M. Candy, a surgeon at Little Hampton, and myself; and I have great pleasure in bearing testimony to the remarkable skill, coolness, and dexterity, manifested in the performance.

The patient is a man of about sixty years of age, who has endured the pain of two calculi of considerable size in his bladder for more than two years. In addition to the suffering necessarily connected with this complaint, he has been rheumatic for twenty years, and has a prostate nearly three times its natural size.

The application of the instruments was repeated nine times, each performance lasting from three to four minutes. The Baron Heurteloup did not think it safe to prolong each manipulation beyond this, on account of the irritable state of the bladder, which, even in this short time, began to contract violently. This, of course, occasioned the necessity for a more frequent repetition of the operation than would have been called for had the bladder been better capable of bearing the presence of the instruments.

After the first and second manipulations, there was some slight fever, and a considerable quantity of mucus in the urine, which (the urine) had a strong ammoniacal smell; but these symptoms lasted only two or three days, and became less and less after every operation. The patient never, through the course of his cure, kept his bed after the first day, and, latterly, sat up to his dinner in an hour or two after leaving the operating-room.

The calculi were of lithic acid, stratified with phosphate of lime, and, from the quantity of fragments brought away, must have been very large; one of them was probably twice the size of a walnut, and the other was something smaller.

The patient suffered several times, during the cure, from the irritation of a fragment of calculus in the urethra;

but this was always relieved by the hip-bath.

When we remember the age, rheumatic state of the patient, and the enlarged state of his prostate gland, with his comparatively trifling suffering, I think we must consider this case, in conjunction with those already published by the Baron Heurteloup, as going far to prove the value of the operation of lithotomy, and its great superiority, in point of safety, over that of lithotomy.

I am, sir,
Your obedient servant,
A. T. S. DODD,
Surgeon to the Chichester
Infirmary.

Chichester, Jan. 7, 1832.

TREATMENT OF ULCERS.

To the Editor of the London Medical Gazette.

SIR,

IF the few remarks which I take the liberty to submit on the treatment of ulcerated legs be worthy of insertion in your valuable journal, it would give me pleasure to have them laid before your readers. The treatment commonly pursued at hospitals and dispensaries, as also in private practice among the poorer classes, is manifestly tedious, and too frequently unsuccessful. I am convinced that almost every description of ulcerated legs may be cured, with the utmost expedition and comfort to the patient, by pursuing the very reverse of the plans generally adopted.

Patients who have long been in attendance at hospitals or dispensaries, usually complain that the remedies, when applied, are of an irritating nature, such, for instance, as sulphate of copper, destroying the granulations thrown up by nature to close the wound. Solutions of caustic, &c. used with a view to excite granulations, although they be present without any such auxiliaries. Are not hard work, walking, and the superincumbent weight of the body, sufficient, and, alas! too powerful stimulants to an ulcerated and inflamed surface? May we not add to this the frequent condition of a varicose state of the veins, in conjunction with the practice of drinking ardent spirits?

* This patient lived at a place 60 miles from London, and the Baron Heurteloup used to arrive there early in the afternoon. He immediately operated; spent the night there; repeated the operation the next morning, and then left for London, to return in three, four, or five days, according to the accounts he received concerning the patient.

—a practice whereby, for increasing the revenue of government, the health and prosperity of myriads of individuals are destroyed, and whole families fall inevitable victims to disease and infamy.

I need only suggest what to every surgeon cannot but be obvious, namely, that rest alone, in an horizontal posture, with the application of linen spread with any mild unctuous substance, will generally be productive of a perfect cure. But in very few cases can that simple plan be adopted by persons whose unfortunate lot it is to endure these diseases, together with a train of others incident to their poverty. When a surgeon is consulted, he will find in frequent instances that something irritating has been applied to the wound; he will perceive the surrounding skin inflamed, and the discharge thin and foetid; the ulceration gradually extending; the limb much swollen, and suffering from its own weight. All these symptoms are accompanied with continual pain, which increases during the night, in consequence of the exercise of the preceding day. In this state most surgeons would deem it indispensable to foment and poultice the part: to this there may be no objection, but I have seldom found it requisite. The first grand measure is, to attack and subdue the irritation and pain, by giving from two to five grains of opium daily, and about one grain of calomel night and morning, or every night, according as the constitution requires. As soon as the tenderness is diminished, let the dry suture be applied in the following manner:—

Having first defended the cicatrizing edges of the wound, and the surrounding inflamed and delicate integuments with a strip of lint or fine linen, spread with spermaceti ointment, palm oil, or any such mild preparation, by placing it on each side, the length of the wound; then apply a piece of adhesive plaister, cut tapering, about an inch in width at the broad end and two-thirds of an inch at the narrow end, and from eight to ten inches in length; the broad ends first, on the sound and uninfamed skin, beyond the lint, on each side of the ulcer; then draw the narrow ends as tight as can be comfortably borne, so as to approximate as much as possible the edges of the wound. In doing this the strips of plaister will cross each other, and afford the strongest support to the wound and the parts surrounding it.

Two or three pairs of these strips will of course be necessary to cover and support an extensive wound. Lastly, apply the bandage, which is the most important part of the operation. This should be done, so as to afford the due support without creating uneasiness.

I am, Sir,

Your obedient servant,

H. OKES BRADFORD, M.R.C.S.

216, Tottenham-Court-Road.

IDENTITY OF SMALL-POX AND COW-POX.

To the Editor of the London Medical Gazette.

SIR,

IF you can spare some corner of your valuable Gazette for the following remarks, I request you will insert them.

I am, Sir,

YOUR CONSTANT READER.

Jan. 13th, 1832.

On reading in your last number, (Jan. 7th) Dr. Gregory's paper on the identity of small and cow-pox, I could not suppress a wish that experiments better calculated to settle the matter than what we learn respecting Dr. Sonderland's, might be instituted in this country, by attaching some cows, unfit for any thing else, to the vaccine establishment, and trying to communicate to them natural small-pox by inoculation. Should it succeed, a supply of lymph would be secured far superior to that obtained from human individuals at the risk of rendering vaccination imperfect, by disturbing the pustule. Cows might then, likewise, be kept at some of the hospitals in the country, and thus depôts for genuine lymph be multiplied.

If, as some think, vaccination is on the wane, it may be attributed to the virus, in its present state, having lost much of its protecting virtue, by passing through so many individuals, and thus becoming more and more assimilated to the human constitution.

The powerful and singular effects of transfusion of blood from one animal to another of a different species, are known. Perhaps a transfusion of morbid matter may produce somewhat analogous effects, and undergoing the anomalous

affection consequent on such a transfer from the cow, may render man insusceptible of the more ordinary form of the disease, generated in his own species. If so, assimilation by continued transmigration cannot but weaken the prophylactic power of the vaccine virus.

— — —

In reference to Dr. Burke's question, at page 505 of the same number, as to the *modus operandi* of the oil of turpentine in the case he communicates, it is evident that the inflammation of the eyes was metastatic, and having come on as soon as the irritation in the urinary organs had ceased, was again transferred to those parts on irritation being reproduced there by the judicious employment of the oil, which here had a counter-irritant, while in the other eye the calomel and opium had a sedative effect.

DR. STEVENS'S REJOINDER TO DR. JOHNSON.

To the Editor of the London Medical Gazette.

SIR,

I OBSERVE by your last number that you are unwilling to lend your pages to a controversy which has now become personal, but as you have already published another attack upon me, I hope that in common fairness you will also insert my reply.

It is painful for me to be compelled to appear so often before the public, on purpose to resist the unfounded attacks of an individual who is no sooner fairly convicted of one misrepresentation, than he tries to evade it by inventing another.

The chief accusation contained in Dr. Johnson's last letter, is one which concerns himself fully as much as it does me; for in it he asserts that the *analysis* of my paper on the Blood, which appeared in his journal for July 1830, was written by myself. Now there is but one charitable construction which I can put on this, and that is, to consider it as a modest intimation to the public, that any author who wishes to review his own works will find a place for the same in the *Medico Chirurgical Review*,—of which this accommodating individual is the sole editor.

It is very natural that a certain class of authors might wish to be accommodated in this kind of way; and lately there has been but too much reason to fear that such accommodation is not uncommon. Be this, however, as it may, there can be no doubt that

the editor who permits such conduct, is just as culpable as the author who wishes to be the writer of his own review. I am sorry to observe that Dr. Johnson, in the present instance, has brought an accusation of this kind against himself; but, in as far as relates to this instance I will stand his friend, and try to prove that he is not guilty.

He asserts that, as he had not heard my paper read at the College of Physicians, I had called upon him, and requested that he would insert a *report* or *analysis* of the said paper, written by myself. "This," says he, "I did publish, and this is the unfair statement by which he (Dr. S.) says I wilfully endeavoured to mislead the public." As Dr. Johnson has fortunately furnished us with a great variety of polite expressions, I will borrow one of his own, and aver that there is scarcely one iota of truth in all this. I was not present myself, neither did I know whether Dr. J. was or was not present when that paper was read; nor did I care. Dr. J.'s journal may be, like others of the day, a useful medium for disseminating original facts; but those will be sorry to have the Doctor himself on their side, who have either read his own works, or who know the light in which his opinions are considered by the *intelligent* part of the medical profession. It is true that I did call upon him,—but not for the purpose which he has specified.

Some of your readers may recollect that in 1814 I published an account of an operation that I had performed in 1812, in which I had tied the internal iliac artery. The patient died in 1822, and the parts were preserved. Dr. Van Brackle, who had been present at the operation, and who was also present at the examination of the body after death, wrote a minute account of the preparation, and sent it to his friend, Dr. Chapman, (the then editor of the *Philadelphia Journal*.) which was published in that work in one of the numbers for 1822. Believing that this would find its way into the English journals, I did not write any account of it myself, but I kept the preparation in my possession, and brought it to England with me, when I returned to this country in 1829. And the truth is, I considered the preparation at that period as of little value; for I thought the circumstance of my having tied that artery was universally admitted. But in this I was deceived; for in May 1830, in looking over Mr. Lawrence's lectures, which had then just appeared, I found that he had expressed a doubt about the correctness of the statement which I had published. In consequence of this I waited upon Mr. L., told him that I had the preparation in my possession, and expressed a wish that he would examine it as early as possible. The preparation was immediately sent to the museum of the College of Surgeons, where it was examined by Sir Astley

Cooper, Mr. Travers, Dr. Hume, Mr. Lawrence, and many others, who expressed themselves perfectly satisfied with respect to the correctness of the original statement which I had made.

A few days after this I received a communication from the secretary of the Medico-Chirurgical Society, asking me to draw up a description of the preparation, and to send it to them for the next number of their Transactions. This, however, I could not do; for I was then on the very eve of embarking for America. In consequence of which I requested Mr. Owen to do it for me, and promised to procure him the description which had formerly been made by Dr. Van Brackle, who had seen the parts in their more recent state. On purpose to procure this, I applied to several of my acquaintances, but none of them could give me the volume which I wanted. Amongst others, I went to my friend, Dr. James Clark, who told me that Dr. Johnson was the most likely person to be able to give me the journal in question; and, as I did not know Dr. J. myself, Dr. C. gave me a few lines to him, stating who I was, and what I wanted. It was *for this purpose* that I called upon Dr. Johnson,—and not, as he avers, to request that he would insert in his journal an analysis of my paper written by myself.

The said paper had been read at the College of Physicians a short time previous to this period, and during our interview it became the subject of conversation: who introduced it I do not recollect, but I will allow, however, that it was probably myself, for I felt then, as I do now, an anxious desire that the facts contained in that paper should be made as public as possible. In the same conversation, Dr. J. expressed a wish to get a copy of the paper, on purpose to lay before his readers a correct account of the facts which I had stated. I told him that I would send him a copy, provided he would give me a positive promise not to publish it in its original form; for, as I believed, the College of Physicians retained to themselves the right of publishing any paper which had been read at their meetings. In consequence of this conversation, I afterwards sent or gave him a copy of the original paper; and, in as far as Dr. J. has given a statement of the facts contained in that document, they are perfectly correct. He states, however, what is not correct, when he asserts that he can prove that the analysis, and the only statement which he had ever given of my views, were written by myself. Indeed I have only to refer to the very first sentence of the said analysis, to convince any impartial person that I, at least, could not have been the writer of it. It begins by stating, that, “at a late meeting of the College of Physicians, a paper on the above subject was read by Dr.

Stevens, who has for many years been a practising physician in the West Indies.”

Now, in the first place, so far from having read the paper myself, I was not even present at that meeting; and, in the second place, you will observe that my name is misspelt, which is not likely to have been the case had I been the writer of the article.

It is customary, I believe, for an author who has written a work to send a copy of it to the editors of journals, and I have explained how and when Dr. J. got possession of a copy of my paper. There is, however, a wide difference between an author giving a copy of an original paper to the editor of a journal, and writing a review of his own production.

Dr. Johnson asserts, however, that he has a document in his possession which will *prove* that I had written the analysis myself. Now, if he has such a document, or any other of mine, except a correct copy of the original paper on the blood, let him bring it forward, and expose it to a fair inspection, within fifteen days from the publication of this, and then I will shew either that the said document is entirely a forgery, or confess that my memory has deceived me sadly in this affair. But if he does not produce it within the above period, I shall then consider his assertions about this document to be as untrue as his other misrepresentations, and so leave him to God and his own conscience.

Now, setting aside all his sophistry, the following is the fair statement of the case. In his journal for July 1830, Dr. J. published a correct outline of the facts contained in my paper. At that period he spoke favourably of my opinions, and represented the means which I had proposed “for combating a dreadful scourge of the human race, as both simple and practicable.” Secondly, when he found that some eminent individuals in the profession considered the proposal which I had made for the treatment of cholera* as better than his own, he then got up, at a public meeting, in a violent passion,

* I may observe, that though this treatment has not yet got a fair trial in cholera, still, in as far as it has been tried, it has been found to be decidedly the most successful. See Sir W. Crichton's letter, in the Supplement to the Medical Gazette; in which it is stated, that, in an hospital at St. Petersburg, only three died out of thirty patients who had been treated with large doses of muriate of soda: but of this hereafter. It is probable that the cholera, like the yellow fever, never would have been so fatal had it not been for improper treatment. Water, saturated with carbonic acid, an infusion of dried raspberries, mustard emetics, &c. &c. have lately been found to be successful in the treatment of this disease. Not that there is any thing specific in these remedies, for they are more useful merely because they are less pernicious than calomel in *scruple doses*, and the other destructive agents that have been recommended by Dr. Johnson, and used by most of his colleagues who have treated cholera in the East.

and, on the authority of certain papers, uttered a tirade about soda and sodafied blood which he himself must have known to be incorrect; for, as I have said, the whole of it was in direct opposition to my own facts, such as he himself had previously published in his journal. It was for this that I accused him of wilful misrepresentation; and having convicted him of this, even by his own evidence, how does he try to get rid of the proof?—why, simply by inventing one misrepresentation on purpose to conceal another, and endeavouring to mislead those who do not think for themselves, by asserting, “*that he had never made any statement except that which was contained in his own journal.*” And though I have admitted the said statement to be correct, yet he labours to prove that this is the ground on which I have accused him: but the truth is, that I never accused him on any such grounds; for I admitted that the facts, as stated in his journal, were perfectly correct. He asserts, however, that this is the only statement which he had ever given of my views. Now the memory of this gentleman must be very bad indeed; for, independent of what occurred in the Westminster Society, is it not true that, in his letter to you of the 5th December, he asserts, that he had in his possession documents, “*from which it appears that soda had nothing whatever to do with the success of the treatment in the above Island—i. e. Trinidad; that the blood of sodafied patients after death was found to be precisely similar to that of the unsodafied;*” and, lastly, he intimates that the success had been owing, not to *soda*, but to the reddening of the blood by the warm bath?

It was for this that I accused Dr. Johnson of unfair dealing, and wilfully endeavouring to mislead the public; for, as he himself had previously published a correct statement of my views, he must have known that the opinions imputed to me by the Trinidad physicians were directly the reverse of those which I had expressed. Under these circumstances, he had no right to bring forward such documents as affording any proof against either my facts or opinions; and if his having done so be not a wilful misrepresentation on his part, then it is madness; and the person who can be guilty of such conduct, without meaning to misrepresent, is fitter to become the inhabitant of a lunatic asylum, than to be, as he professes, an instructor of the public.

Dr. Johnson denies having had any previous communication with the writers of the Trinidad documents; and this, perhaps, is true. I did not, however, directly accuse him of corresponding with either of these gentlemen. I observed, “*that Mr. Greatrex, apparently in answer to a letter from Dr. Johnson, states,*” &c.; and I did so for

the following reason. If you will refer to page 296 of the last number of Dr. J.’s journal, you will find it there stated, that “*the following extract of a letter from Mr. Greatrex (after perusing a communication of mine on the foregoing subject, and essentially* the same), I beg leave to append,*” &c.

Mr. Greatrex’s letter, which is apparently written to Dr. J., is dated July 24th, 1831, and in this he says—“*I withheld, as you have seen by Dr. Hacket’s letter, all authority,*” &c. Dr. Johnson, however, observes, “*that he had never corresponded with Mr. Greatrex either before or since.*” Now, are we to consider this as an indirect admission, that he had corresponded with him at that period? and when we find it stated in a journal, “*that Mr. G. in answer to a communication of mine on the foregoing subject,*” &c., who are we to suppose to have been the correspondent in this case but the editor himself? It is evident from what follows that it was not written in answer to any communication of Dr. Hacket; and if it was not to Dr. J., to whom was it written? It is possible that some other unknown third person has been making himself busy in this affair; and if so, Dr. J. can only clear himself of the suspicion, by explaining to the public who this person is.

Dr. Johnson observes, that he had acted the part of a friend, and denies that he has made any attack upon me; for “*where,*” says he, “*did I attack him at all?*” Now Dr. J. is aware that, when my last paper was written, I was under an impression that he had asserted in the Westminster Society, “*that he had a document in his possession*” which would prove that what I had published was “*forgeries,*” and that my conduct “*was disgraceful to the whole profession.*” This statement, it is proper to observe, found its way into the Morning Post. It is, however, but fair to state, that when I requested a friend to wait on Dr. Johnson, and to require either a disavowal or an apology for such outrageous conduct, that he signed a document, in which he affirms “*that the above report is totally incorrect, and that he had never made any charge of the kind.*” Such being the case, and though the editor of the Morning Post still insists that the original report was correct, yet I will waive what occurred at that meeting, and simply refer to his own letter to you of the 5th December, 1831, in which he states, “*that he had documents in his possession ‘charging Dr. Stevens with misrepresentation of facts,’ also ‘of garbling, to suit his own purpose, a letter from*

* From this it would appear that the said document has undergone certain changes since Mr. Greatrex had seen it; and of this I am very glad, for I can scarcely believe that Mr. G. would have lent his sanction to the improper language which has since appeared in Dr. J.’s journal.

Mr. Greatrex;" and other grave charges that were, in his mind, "calculated to do injury, and even disgrace to the medical profession." Now, if Dr. J. has any sense of propriety yet left, let him read this; and then he may himself answer his own question, and inform the public "where he had made any attack against me."

You will thus see, by referring to his own letter of the 5th, that he did use language which he had no right to have used, and you know also that he did make an impression on the public mind which the Trinidad papers themselves never could have made had they merely appeared before the public in their original form, and without any previous comment. Yet, after all this, he has, to use his own expression, "the effrontery" to state that he never made any attack upon me, and that he took no part in the controversy, "either *pro* or *con*." Poor innocent man! he has really been ill-used. He took no part! he merely accused me of misrepresenting facts, garbling letters, and other conduct that was disgraceful to the whole profession, and this, like a good, simple, honest editor, he calls publishing "the two documents written by the contending parties themselves, without taking any part, *pro* or *con*, in the controversy." Yet he is perfectly aware that at this moment the Trinidad documents are circulating against me all over the world, and uncontradicted, at least in as far as his journal is concerned;—his next number, however, will shew whether he be or be not deserving of the good character which he has given of himself.

It is the duty of an editor, or of a professed critic, to lay before his readers a clear view of facts, and if necessary, to reason from these as fairly as he can. It is one, at least, of the essential characteristics of a gentleman not to attack the honour of another until he is fully convinced that he deserves such treatment. I have thus stated the facts, and whether Dr. J. has acted with wisdom towards himself, or fairness towards me, I shall now leave to the decision of the public.

By inserting the above, you will oblige
Your obedient servant,

WM. STEVENS, M.D.

Albany-Street, Regent's Park,
January 23, 1832.

[We have thus inserted Dr. Johnson's original charges against Dr. Stevens, the answer of the latter, Dr. Johnson's reply, and Dr. Stevens's rejoinder; and here we must close the controversy, so far as the Medical Gazette is concerned. If either gentleman have any thing farther to say, it must appear at the end of the number, *extra limites*, and at the expense of the parties.]

ANALYSES & NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abrégé."—D'ALEMBERT.

On the Phenomena of Dreams, and other transient illusions. By W. C. DENDY, M.R.C.S. &c. 18mo. pp. 154.

It would require no ghost to tell us, even if this little production had appeared anonymously, that its author was a medical man. How nosologically does he treat his subject! Here we have—

"1. The *Predisposing* Cause (of Dreaming).—Congestion of venous or carbonized blood in the vessels of the brain.

"2. *Exciting* cause.—External or internal impression on the extremities or origin of a nerve (of sense?) at the period of departing or returning consciousness.

"3. *Proximate* cause.—Recurrence of ideas (erroneously associated)."

Then there is allusion to the "*remote* or *sympathetic* causes." And in another place we are told, that "the *immediate* cause of sleep appears to consist in certain conditions of the blood in the vessels of the brain, depending on the reflux of a great portion of the vital fluid towards the heart, as in deliquium, or its congestion in the sinuses or veins"—"a condition which may result from depressed nervous energy, exhaustion, fatigue, cold, or the influence of powerful narcotics; and when *either* of these causes is in excess, death itself is the result." Now, in our humble judgment, there is no small mistake in this view of the matter. Sleep is certainly *not* to be considered as a disease; nor is dreaming either. Mr. D. would understand sleep too literally in the sense of a "non-natural." We fear he is too medical to be correctly metaphysical. But he has given us, without doubt, a very amusing little book, full of materials for the occupation of the inquisitive; how he contrived to persuade himself to be so brief, after so much curious reading as he has evidently indulged in, is to us a consideration which much redounds to his credit.

CELSUS AND GREGORY.

A. C. Celsi de Re Medica Libri octo.

With a Translation, by G. F. COLLIER, M.D. In 4 small volumes.

A. Cor. Celsi Medicinæ Libri octo.

With the words of the text in the order of construction, and a Translation by ALEX. LEE, A.M. Surg. In 2 vols. 8vo.

Conspectus Medicinæ Theoreticæ auctore JACOBO GREGORY, M.D. With an Ordo, Translation, and Notes, by J. STEGALL, M.D. 18mo. 2d Edit.

Lectiones Celsianæ et Gregorianæ; or Passages from Celsus and Gregory syntactically arranged, with observations explaining difficulties, &c.; to which is added a Grammar. By WILLIAM CROSS. 18mo.

ALL these productions are indebted for their origin to the regulations of the worshipful company of Apothecaries—they all, particularly the latter three, profess to help the tyro through his difficulties—to give him a *quantum suff.* of intimacy with Gregory and Celsus—and, in short, at a *minimum* of expense, both of time and money, to supply him with all the classical literature he will *absolutely* want in his profession. Woe to the luckless wight who puts his faith in such “blind guides!”

Dr. Collier's book is certainly the most respectable in the collection; for which reason we have put it first. The text is apparently correct enough; but we cannot say much for either the elegance or accuracy of the translation.

The second on the list is as impudent a piece of pretension as we ever remember to have met with. In a preface full of vapouring nonsense, the *author* informs us that he has been several years employed upon his *Ordo Verborum*, which he hopes will lessen the “tædium of study otherwise necessary;” and he puts forth a quantity of the most ridiculous trash relative to *his* labours in procuring a “most approved text:” on he goes then babbling about Targa, and Cæsarius, and Almelooven, and Linden—*his* predecessors in this *duty*, attended with such “great responsibility.” We have not time to waste upon a full exposure of this sort of quackery; but we shall lay before our readers a few specimens of the *author's* abilities in the translating, or rather traducing, way, that will amuse them. Among other preliminary matter with which Mr. Lee has adorned his Celsus,

is a life of him by a Dutchman, which our author has been tempted to translate. Aware that there have been various English versions of Celsus de Medicina, by which a new translator might profit, we were not so anxious to taste of his quality in the body of the work as in this volunteer specimen of the life, where we suspected he should have to “swim without his corks.” Let us take a few samples.

Rhodius (the author of the life), after saying he was satisfied that Celsus lived at Rome, adds,—“non desunt tamen causæ, cur addubitem, patria, an jure civitatis, ipsum Romanis accenseam.” This Mr. Lee translates,—“reasons are not wanting why I should hesitate to acknowledge him a Roman, *either* by birth, or right of citizenship!” in which he has not only missed the grammatical meaning, but the common sense of his author. The following passage, if he understood it, would have set him right. Rhodius thinks it probable that the ancient booksellers would be anxious to secure to Celsus the epithet of “Romanus,” at all events, whether he was entitled to it by privilege, or by birth,—“posterius tamen ut credam, *suadet* præter apertum auctoris (nostri) textum, epistola ejusdem ad Calistum, et *persuadet* ad Pullum Natalem altera;” which Mr. Lee translates,—“yet I am inclined to believe the latter, as it is evident from the text of *that author* (?), in a letter of his to Calistus, and in another to Pullus Natalis,”—missing both the sense and strength of the expression before him.

Again, where Rhodius is telling us that in some of the older editions “Romani” is added to “A. Corn. Celsi,” he takes occasion to add:—“Patriam sive auctor ipse, sive alius quispiam addiderit, id certe non caret vetustatis exemplo,” &c. Mr. Lee thinks proper to translate the latter phrase for us thus:—“As to the word ‘*patria*,’ whether the author himself added it, or some other person for him!” &c. &c.

One passage more from the translations of the new editor of Celsus; it is at the conclusion of the life:—

“His quotquot naturæ associavit benignitas, æquos adferre animos, eleganti favete linguæ. Ita vobis, et verum animi sermonisque cultum, et firma valetudinis præsidia parabit. Tantum est.

“Optandum est, ut sit mens sana in corpore sano.”

"Therefore," translates Mr. Lee, "let that *felicitous association in youth* be united to an *unbiased mind*, and *cultivate the elegance of his language*. So that by these means *they* (?) will ensure for themselves the most valuable improvement both of mind, language, and the safest remedies for the recovery of health. *I must finish, 'with the sincerest wish, that you may always possess a sound mind in a vigorous frame'*" !!!

Is the reader satisfied? We have taken the preceding specimens, almost at random, out of a number of others which struck us as equally engaging. But we cannot part from Mr. Lee without seriously informing him, that a boy of one of the lowest forms in our grammar schools would be whipped for such stupid and egregious blundering.

Dr. Steggall's *Gregory* is a very good little book; but we are sorry to see it disfigured with an *Ordo*. This business of ordo-making is one of the vilest liberties ever devised or practised upon a classical writer: all beauty and harmony of arrangement are exploded by it. Better a thousand times have recourse to the interlinear versions of the Hamiltonian system; for by them we make out the meaning of the author without torturing him to death: and we can infinitely better afford to have our own language disjointed and put out of tune, than have the rhythmical cadence of an ancient tongue *dis-ordered* and destroyed.

Of the *Lectiones Celsianæ*, &c. the less there is said the better. It is a humble performance of the humblest class.

Working Man's Companion. — The Physician. (1. *The Cholera.*) Published under the superintendence of the Society for the Diffusion of Useful Knowledge. Charles Knight, Pall-Mall East, 1832.

USEFUL knowledge! What will it profit the "working man" to have an elaborate history of the rise and progress of cholera laid before him? A few general directions for his guidance, as to habits of temperance and cleanliness, might indeed be of service, but a volume of two hundred and odd pages on the symptoms, pa-

thology, and treatment of cholera to a person of that class to which the work is addressed, becomes perfectly ridiculous. The only part of it which is likely to be thoroughly understood, and practically followed, is the recommendation to the working man, if he be cold and wet, to take a glass "of good brandy." We are sorry to perceive a series which commenced so happily with a most interesting essay on machinery, well calculated to convince the "operatives" of the benefits derived from its improvements, so soon break down into any thing so utterly unfit for its intended purpose as "the Physician."

MEDICAL GAZETTE.

Saturday, January 28, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Ar-
tis Medicæ* tueri; potestas modo veniendi in pub-
licum sit, dicendi periculum non recuso."—CICERO.

AMENDED ANATOMICAL BILL.

THE anatomical bill, as amended by the committee, now lies before us, and our hopes in consequence revive. Several important steps have been made since we addressed our readers last week. Then, indeed, we were disappointed, for we had before our eyes the manœuvre, apparently successful, of a meddling blockhead, and the mortifying spectacle of a measure involving some of the dearest interests of society, delayed, if not defeated, because there was no "House" to discuss or entertain it. But a better fate awaited the renewed attempt, and the bill, with several improvements, has now passed the second reading, and been committed. The changes in it are not numerous nor striking, but they are of essential importance in their effects. To say that even now it is in all its parts such as we, being professional men, should have desired, would be uncandid; but this we will say, that its provisions are cal-

culated, as regards the public, to protect science, and consequently to benefit the community in no small degree; while, viewed in reference to the anatomist, they separate him from his unnatural partnership with ruffians, and thus permit him to prosecute his most useful labours, if not entirely free from the revilings of the ignorant and the prejudiced, at least without degradation, and secure against the penalties of the law. There is yet another light in which the proposed measure may be viewed; we mean, as regards the legislature itself, which, if passed, it will free from the greatest blot by which any code can be stained— injustice;—owing to the existence in the statute-book of laws by which men are compelled to know, and forbidden to acquire knowledge—are enjoined to do, and to leave undone—by laws which defy obedience, and enactments which mock the understanding. It is subject of congratulation to find that Mr. Warburton has not, as we, it appears, prematurely apprehended, become indifferent or lukewarm in the cause for which he has done so much; and the majority in his favour, on the divisions which occurred, afford reason to hope that he will carry his bill successfully through the lower house. Devoutly do we hope that it may not, as before, meet with any impediment from the peers.

We have criticised the bill as to some of its minor points, but we will freely admit that possibly—nay, even probably—provisions which to medical men would have appeared more perfect, might, by the very circumstances in which we deemed their excellence to consist, have become less eligible in the eyes of our legislators; and however much we may argue and declaim upon the subject, to their view of the question we must bend. Our judgment, we may persuade ourselves, is the law of reason, but their decisions constitute the

law of the land. The principle—the general principle once admitted by the legislature, that anatomy should be protected, and some provision made for its being cultivated without violation of the law—details accommodated to the circumstances would afterwards follow; and therefore, though we have freely commented on some subordinate clauses, while there was yet opportunity for their improvement, now that some essential amendments have been made, and the measure fairly brought before the House, we are for cheerfully accepting what we can obtain, and most sincerely do we desire that we had in firm possession the benefits which Mr. Warburton's bill would confer. But in order to facilitate the attainment of this great object, it is to be desired that those, who think the measure not so absolutely perfect as they conceive it might possibly have been⁷ made, should, notwithstanding, waive their own favourite notions on less important points, in consideration of the great good which must result from the proposed enactment, constituted as it is. The appearance at the present moment of petty jealousies—the proposal of fine-spun distinctions—the insisting on corporate privileges—in a word, the appearance of any disunion in the respectable part of the profession, must needs be mischievous; and by being adduced in either House by some one hostile to the bill, might lead to its being thrown out, and with it every measure of relief postponed for a period, the duration of which, or the extent of intermediate evil, no one can foresee.

From the very temperate and judicious letter addressed by the Council of the College of Surgeons to Lord Melbourne, we had hoped that they at least were unanimous in the opinions we have above expressed. What, then, was our mortification on beholding an anecdote about a

gallant officer expressing, on his death-bed, an abhorrence of dissection, from the pen of one of these gentlemen, effecting its revolution through all the newspapers, whig, tory, and radical, as if it were something which appealed to all men alike, of whatever rank and of whatever party. The story itself, indeed, is so utterly destitute of any point, that the only feeling it can excite must be that of astonishment that "copy" should ever be so scanty as to lead the editor greediest of materials to transcribe it into his pages; but when by the reference it appears to come from a Professor to the College of Surgeons, and a member of the Council, the source whence the sentiment flows gives it an importance which it certainly would not have derived either from any depth in the argument, or force in the illustrations, by which it is accompanied. It is amusing to observe how frequently the secret feelings and weaknesses of men will escape them almost unconsciously; and thus it becomes apparent in the present instance, that the learned author has personally a horror at the idea of being dissected, for he says, in reference to the anecdote alluded to—"Sir W. Myers and *myself* know too much of the process to like it." Now the gist of all his argument is, that dissection, which the medical profession almost to a man desire to see freed from the stigma which is entailed upon it, by being made a part of the punishment inflicted on those guilty of the greatest crimes, should now and for ever be stamped as infamous, and as an indignity offered to the body of criminals alone. With this view of the question it is not surprising that the writer should strenuously oppose the first great step towards removing the general prejudice, by making the bodies of executed murderers no longer objects of popular horror, and by ceasing to exhibit them in all the revolting display so unwisely adopted on such occasions;

and which, in one recent instance, at least, gave rise to a scene of the most brutal description. Can any man, with ordinary powers of reasoning, contend that exhibitions like these, got up to gratify vulgar curiosity, in which the surgeon completes the sentence which the executioner began—can he, we ask, conceive but for a moment that the cause of anatomy can thus be forwarded; or that by any possibility it can fail to be injured, by such direct association, in the public mind, with crime, with punishment, and with horror? But it is said that the dissection of murderers is not the cause of that process being looked upon with aversion, and that the repeal of this part of the law would therefore be attended with no advantage. Indeed, so strong is the conviction of Mr. Guthrie on this point, that if any one supposed that his objections would be thus removed, he "should only be prevented by civility from laughing at him." Now there is something so excessively absurd in this mode of dealing with such a subject, that it really requires all our forbearance to avoid treating the learned Professor in the manner he threatens to adopt towards those who differ from him in opinion; as it is, we cannot but smile to think what a merry time he would have of it, if his "civility" did not stand in his way; for if the reader will turn to the examination before the Anatomical Committee of the House of Commons in 1828, he will find that, of a very large number of persons whose evidence was taken on that occasion—physicians, surgeons, magistrates, policemen, resurrectionists—men of all ranks and occupations—he will find, we say, that one, and one only, was of opinion that the dissection of murderers had no share in causing the prejudice against dissection*. The Professor next in-

* See also some judicious observations on this point contained in a Letter, by Dr. James Somerville, addressed to the Lord Chancellor. *Hatchard and Son, Piccadilly.*

dulges in a good deal of sentimental indignation about dissecting the poor man, merely "because he is poor," and of the necessity, in order to be consistent, that medical men should leave their own bodies to the anatomists. All this will do for the Morning Herald, whose articles savour very much of the Professor's pamphlet; but we shall content ourselves with saying, as the Baron de Grimm did of some sophistry of Rousseau's—*cela est très beau et très faux*. Doctrines more injurious to the cause which the writer professes to advocate, it would have been difficult at any time to propound; and the particular moment at which they are broached is calculated to render them doubly mischievous so far as their influence may extend. This, however, we imagine will prove extremely limited; for the bill, as we have said, holds on its course with every prospect of being triumphantly carried in the lower house.

We have stated that some improvements have been made since the original sketch, which we gave some weeks ago. They are as follow:—

Originally it was proposed to be enacted, that it should be legal for any party having lawfully the custody of a body, to permit such body to be dissected, *with the consent* of the nearest known relative of the deceased, "unless to the knowledge of such party such person shall have expressed his desire, either in writing, *at any time*, or orally, in the presence of one or more witnesses, that his body after death may not undergo such examination." This clause, it is obvious, would have been open to much abuse, and given rise to great and vexatious difficulties. Now, however, it stands that any one, "*not being an undertaker*," having lawful possession of the body, may permit it to be dissected; "unless to the knowledge of such party such person shall have express-

ed his desire, either in writing at any time during his life, or orally in the presence of two or more witnesses *during the illness whereof he died*, that his body after death might not undergo such examination, or unless the surviving husband, or wife, or any known relative of the deceased, shall require the body to be interred without such examination." The importance of this change is too obvious to require that we should point it out.

A provision is also added, in case of persons directing anatomical examinations after their death—namely, that if any person, either in writing at any time, or orally during his last illness, shall direct his body to be examined anatomically, and if, before the burial, such fact be made known to the parties having lawful possession of the body, they are to direct such anatomical examination to be made; unless it be contrary to the wishes of the husband, wife, or nearest known relative.

The nature of the certificate to be given by any medical man, who may be sent for after the death of the individual, is also amended, and with propriety. Instead of certifying positively as to circumstances of which he can often have but an indirect knowledge, he is only "to state the manner of his death to the best of his knowledge and belief."

A clause is proposed to be added in the Committee, regulating the amount of salaries to the Inspectors, which are not to exceed one hundred pounds annually.

And lastly, the operation of the bill is to commence from the 1st of July next.

We have thus laid before our readers the earliest information regarding this important measure in its various stages; and we shall continue to do so, till it either be passed, or share the fate of its predecessor. We have avoid-

ed inserting the bill itself at length, because the changes it has already undergone render the original version already obsolete; but as soon as it has become part of the law of the land, we shall transcribe it into our pages, and hail with gratitude a statute which will recognize anatomy, and place it on the same footing as the other liberal sciences.

ACTION OF AMMONIA ON THE VACCINE.

M. NAUCHE, of Paris, has given an account of some recent experiments of his, with regard to the power of ammonia over the vaccine virus. 1. Upon vaccinating with a lancet charged with the virus, but which had been exposed for some seconds to the vapour of ammonia, no development took place. 2. Inoculating one arm of the same patient with vaccine exposed to ammonia, and the other with pure vaccine, not only did no development take place in the first arm, but that in the second proved very imperfect. 3. After vaccinating a child as perfectly as might be, and in a few minutes afterwards making some slight punctures or incisions with lancets which had been exposed to the vapour of ammonia, the power of the vaccine was so far injured as to render the development very incomplete. These facts, M. Nauche thinks, taken in conjunction with the well-known effects of ammonia in various poisonous and contagious complaints, would warrant a fair trial for the volatile alkali in cholera, both as a preservative and a means of cure.

WESTMINSTER MEDICAL SOCIETY.

Saturday, January 14, 1832.

Phrenology and Insanity.

A PAPER by Mr. F. Winslow was read on the application of Phrenology to the elucidation of Mental Disorders.

The author began by observing, that the value of knowledge altogether depends on its susceptibility of being applied to practi-

cal purposes. Many who object to the abstract doctrines of phrenological science are continually inquiring, to what utility do they lead? Mr. Winslow thinks that a sufficient answer is given to the question by the fact, that one of the most important classes of diseases with which medical men have to combat, admits of being rationally elucidated by the aid of phrenology. Until Dr. Gall's time the phenomena of mind used to be explained by metaphysicians, without any reference to matter: he it was who "established, on an immutable basis, a system of mental philosophy, which, for its beauty and simplicity, stands unrivalled." "Reasoning *à priori*," observed Mr. W. "it certainly does appear absurd, that any person can have the power to form an estimate of the capacity of an individual, by examining the external conformation of his cranium; but there are many principles proved beyond all doubt, which an ignorant person would pronounce to be preposterous and absurd. Tell a man unacquainted with the rapid improvements of medical science, that you had just been eating your breakfast of bread made from sawdust, would he not pronounce you to be mad for making such an assertion?" And the author went on to prove, that of such sort of ignorant persons are those, who pronounce phrenology to be "a monstrous piece of quackery!" Dr. Spurzheim was the first who gave the public a satisfactory theory of mental derangement. Until his time, the definitions of insanity were vague and contradictory. Medical writers have erred in considering insanity as a specific disease; whereas the phrenologists, after Dr. Spurzheim, hold that mental derangement is *only a symptom* which may be produced by a variety of totally dissimilar affections. There are two hundred diseases, according to nosologists, to which the human eye is subject; and it would be just as futile to attempt to include all the symptoms of these diseases in *one* definition, as to frame one definition inclusive of every affection of the brain giving rise to insanity. Medical writers have attempted to draw a line of demarcation between insanity and those idiosyncrasies of thought and feeling which, in the eye of the law, do not amount to mental derangement; and hence many definitions have been suggested by Dr. Haslam and others. Mr. W. expressed himself dissatisfied with Dr. Haslam's suggestion, that the physician's mind should be the standard of the patient's sanity; and entered, at considerable length, into examples of hallucination of a remarkable kind, from Shakspeare's, Luther's, and Sir Walter Scott's works. The diseases of the brain were curiously exemplified by reference to Hamlet and King Lear; and extracts from Dr. Combe's work were read. Dr. Gooch and Dr. Conolly, D'Israeli and Hazlitt, were also cited; and

Mr. Winslow amply adduced instances of men, particularly physicians, "of one idea." From all the facts which he stated, the author was of opinion, that too much care could not be devoted by medical men to the ascertaining the healthy, as well as the deranged, manifestations of the individuals presented to them. Dr. Haslam's standard he thought absurd. "Insanity cannot be defined; it is ridiculous to expect that all the varieties and forms of mental derangement can be included in one definition." A medical man of enlarged understanding can, after an attentive examination of the present and past conduct of his patient, tell whether insanity does or does not belong to him. In medico-legal investigations, where we are called upon as witnesses regarding insanity, Mr. Winslow thinks that we are not obliged to decide on the abstract question of the disorder, but upon the degree of mental derangement; and the slightest departure from a healthy condition of the brain, giving rise to deranged manifestations, ought to be looked upon as insanity. But it would be more proper for the commissioners of lunacy, on these occasions, to ask the opinions of medical men as to the existence of insanity, accompanied with an inability on the part of the patient to control himself or his affairs. "A man," said Mr. Winslow, "may be insane, yet perfectly competent to go at large, and to will away his property." In conclusion, the paramount importance of a knowledge of phrenology to the medical student was once more set forth, and this passage from Dr. Thomas Brown, the metaphysician, was quoted, as applying to phrenologists *par excellence*:—"He is unquestionably the philosopher most worthy of the name, who unites to the most accurate knowledge of mind, the most accurate knowledge of all the physical objects amid which he is placed; who makes each science to each, reciprocally a source of additional illumination; and who learns, from both, the noblest of all the lessons which they can give—the knowledge and adoration of that divine Being, who has alike created and adapted to each other, with an order so harmonious, the universe of matter and the universe of thought."

ALDRSGATE-STREET SCHOOL.

To the Editor of the London Medical Gazette.

SIR,

I FEEL confident that, whilst one leading object of your publication is to communicate valuable professional information, another scarcely less important one is to protect the rights of students, and to enforce regularity and attention on the part of their instructors.

It is with great pain that I feel myself compelled to direct attention, through your publication, to neglect on the part of one of the anatomical teachers in the Medical School in Aldersgate-Street. The latter half of the course of demonstrations has been given with the greatest irregularity; and this morning, when the students assembled expecting to hear a demonstration (after the lapse of several days without one), notice was given that Dr. Roe was in the country with a sick friend, and could not leave. Discontent, on account of such repeated irregularity, has arisen to so great a height, that, by inserting this communication, you will do no less a service to the students at large than to the interests of the establishment itself.—I am, sir,

Your obedient servant,

A PUPIL.

Aldersgate-Street,
Thursday morning, Jan. 19, 1832.

[The above is authenticated by the writer, who has communicated his name. We think it to the interests of the gentlemen connected with the school, to let them have an opportunity of refuting the statement, if it be unfounded, or remedying the evil complained of, if our correspondent's account of the matter be correct.—*Ed. Gaz.*]

METEOROLOGICAL JOURNAL,

Kept at EDMONTON, Latitude $51^{\circ} 37' 32''$ N.
Longitude $0^{\circ} 3' 51''$ W. of Greenwich.

Jan. 1832.	THERMOMETER.	BAROMETER.
19	from 23 to 34	from 30.26 to 30.20
20	28 34	30.11 Stat.
21	29 41	30.07 30.16
22	32 43	30.16 30.15
23	33 42	30.14 30.24
24	24 46	30.14 30.09
25	37 47	29.74 29.76

Wind variable, S.W. prevailing.

Except the 24th, and afternoon of the 25th, cloudy; rain in the morning of the 25th.

Rain fallen, '1 of an inch.

CHARLES HENRY ADAMS.

NOTICES.

The communications of Dr. Allsop, Dr. Burrows, Mr. Ainsworth, and Mr. Dixon, were received too late for the present No.

The papers of Mr. George, Mr. Smith, Dr. Todd, Mr. Ashwin, Mr. Bateman, Mr. Davison, Mr. Stafford, and Dr. Copland, are in hand.

W. WILSON, Printer, 57, Skinner-Street, London.

THE
LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF
Medicine and the Collateral Sciences.

SATURDAY, FEBRUARY 4, 1832.

LECTURES
ON
THE THEORY AND PRACTICE OF
MEDICINE;

Delivered at the London University,

By DR. ELLIOTSON.

PART I.—LECTURE XVIII.

*Changes of Structure—Transformations—New
Formations.*

I HAVE spoken, gentlemen, of that change of structure which consists simply in an alteration as to consistence or as to size. I have spoken of induration and softening, and of hypertrophy, and atrophy, and I have likewise spoken of ulceration, which of course causes a diminution of substance. I now proceed to consider those changes of structure in which one structure is converted into another natural to the body. These are called *transformations*.

Transformations.

Now these *transformations* of a diseased character are all like those which naturally occur during the growth and decline of an individual in health, or they are to such a structure as the particular part is actually found to be naturally of in some of the series of animals. For example, cellular membrane is the frame-work, if I may so speak, of the other structures of the body; and in the natural condition of the body, in its natural progress, we observe that when a part is no longer wanted, it degenerates into cellular membrane, an instance of which is presented to us in the thyroid gland of the fetus. Now such a change will occur merely as a diseased process; a part may be resolved nearly into cellular membrane, and that is an instance of transformation. Cartilage will sometimes by disease become bone; but it is perfectly natural to the progress of the body that cartilage should be-

come bone: before bone we have cartilage in the fetus. The morbid changes which are transformations are thus nothing more than alterations precisely the same as what really do happen naturally in the progress of the human body, or are alterations of a part to a structure which it is seen to be of naturally in some other animals. They observe exactly the same law, although they are morbid processes. One structure is not changed into another unless that same structure is observed to be changed into that other at some period of the progress of the body, or that other is the natural structure of the part in some of the series of animals. Cartilage, I have said, will become bone through disease, and the same is observed as a natural process at certain ages; whereas it is never known that by any disease a mucous will become a serous membrane. As such a change is unknown in physiology, so it is unknown in pathology.

Mucous membranes may be converted into skin, and skin may be converted into mucous membranes; muscles will also degenerate into fibrous membranes. Now the very same part which is mucous membrane in one animal is skin in another; and what are muscles in some animals are merely fibro-ligamentous substances in another. These same changes, however, will occasionally take place out of their natural course, through disease, and they are then called transformations. Cartilage is never known, on the other hand, in physiology to become mucous membrane. What is cartilage in one animal is never mucous membrane in another; and in the transformations which take place in the progress of the human body naturally, cartilage never becomes mucous membrane; and so in disease cartilage is never known to be converted into a mucous membrane.

Transformation to Cellular Membrane.—Now as to these transformations, almost all the structures of the body will waste into cellular membrane. It is perhaps hardly

right to say they are transformations into it, because it certainly is the frame-work of all the other structures; and if all the various other substances were absorbed, and nothing but the cellular substance remained, it has been said that, provided it could be kept firm, it would maintain the entire form of the body.

Transformation to Fat.—Parts often appear transformed to fat. But Andral considers that this is merely an instance of inordinate secretion of fat, with or without an atrophy of the tissues.

Transformation to Serous Membranes.—We have frequently a transformation to serous membrane. When the extremity of a bone has been dislocated, you will find, after a considerable lapse of time, that a serous membrane is formed around it. If a coagulum of blood be effused into the brain, or any other firm substance deposited there, (even if it is a bullet) it will become surrounded by a serous membrane, a *capsule*, as it is called, which secretes serous fluid. Occasionally, when there is a loss of substance, which nature cannot entirely repair, the space is enveloped by a serous membrane. Even when a portion of substance is deficient, not by external violence, but by a natural defect, its place is frequently supplied by a serous membrane. In a defect of portions of brain, you will find in their room a bag, containing liquid, and which is to all intents and purposes a serous membrane. What are called *apoplectic cells* in the brain are nothing more than cavities formed by a coagulum of blood which has been let loose, and which coagulum has had formed around it a serous membrane. The coagulum will be absorbed, while the serous membrane may remain, and continue secreting fluid.

Serous Cysts.—Such serous cysts may be formed in almost any part of the body. You know that a serous membrane is a closed sac, with a smooth internal secreting surface, and is found in the body naturally. Precisely such serous sacs are formed by disease, and are formed of all sizes, from that of a pin's head, and even more minute still, from the size of a millet-seed to a very considerable bulk. We find these cysts sometimes placed in juxta-position, either attached or unattached to each other, and sometimes they are closely united to the surrounding parts. They may be single or they may be numerous, solitary or crowded. Generally there is no red vessel to be traced beneath the external surface, and the utmost vascularity is an arborisation upon this. The part which surrounds these cysts may be in all sorts of states. Usually it is perfectly healthy, sometimes it is wasted, and sometimes it is in other states of disease, as, for example, indurated. Occasionally we find these serous cysts surrounded by pus or other secretions. They are surrounded frequently by cellular substance, which gives them an additional

coat; sometimes they become indurated, even to cartilage or bone; and sometimes they become more or less fibrous. You will recollect that the cellular membrane becomes very firm around the fibrinous cup of an abscess, and gives it an additional support, and so it is with respect to these serous cysts. The cellular membrane around them is frequently condensed.

The internal part of these serous cysts presents very various appearances; occasionally, and for the most part, indeed, it is smooth, exactly as in a natural serous membrane; but it varies, exactly as in natural serous membranes under disease. When natural serous membranes are diseased, they are frequently rough, granulated, or flocculent within; and so the interior of these serous membranes of disease presents a great variety of appearances, being occasionally rough, with minute granules, and sometimes with large caruncles, as it were. Frequently the granules which adhere to the interior of these serous cysts are of an osseous character, or contain, at least, more or fewer particles of bone. Occasionally we observe on the interior of cysts false membranes, that is to say, fibrin has been poured out, giving them exactly the appearance which you have in inflamed serous membranes, and which I spoke of formerly. Frequently in these cysts there are partitions, dividing them into so many compartments. Frequently there are prolongations from the interior, running through the cavity of the cyst, and not completely dividing it, but causing it to have a number of compartments, communicating more or less with each other.

You will find the contents of these new serous membranes very various, as well as their internal surface. Occasionally they contain merely a serous fluid, occasionally they contain blood; they will contain something like mucus, various saline matters, fatty matters, tubercular matter, and some things which are peculiar to cysts. There is a far greater variety in the contents of these serous cysts than in the natural serous membranes of the body, when these happen to be diseased. You will find various contents in different portions of the same cyst. You will also find some growing from the inside of others, hanging in them by peduncles; and sometimes, instead of hanging in this manner, they are attached to the interior by a considerable flat surface.

It has been imagined that all the new formations of the body are nothing more than cysts originally, and these cysts have been called *hydatids*. The term *hydatid* should be confined to a certain animal, but it is used vaguely to embrace real animals and these serous cysts, which are nothing more than morbidly formed serous membranes. Now it has been imagined that all the new

formations of the body have been originally hydatids—that is to say, parasitic animals, or such new cysts as I have mentioned. One compartment of these cysts will certainly sometimes be filled with scirrhus substance, another with melanotic, and another with encephaloid substance; and we continually see minute serous cysts by the side of scirrhus, melanosis, &c., and hence it has been supposed that these are merely different stages and forms of the same disease; but I do not see any proof of it, because when a part has once fallen into a state of disease, all kinds of disease will frequently spring up around and in it. It has been supposed, that where we cannot detect any new serous membrane, but where we see the solid substance of scirrhus, for example, that it was nothing more originally than a serous cyst filled with indurated substance—that the transverse bands are nothing more than new serous cysts which grew on the inside, and hung by peduncles, which had been filled with an indurated substance until the whole formed one mass—that the fibrous bands we see in scirrhus are nothing more than different serous cysts all compacted in this way. You should consult on this subject the work of Dr. Baron, and a paper by Dr. Hodgkin, in the *Medico-Chirurgical Transactions*. Their views are ingenious, but whether altogether true, I will not pretend to say. Dr. Hodgkin followed Dr. Baron, and, as far as I can comprehend his paper, has only developed Dr. Baron's views.

You will understand, when you see minute sacs in various parts of the body, that they are usually nothing more than new serous cysts. You will see them continually in the choroid plexus in the brain, and you will see them from the most minute size till they become as large as the specimen I now shew you, and even a great deal larger.

Transformation to Mucous Membranes.—Now with respect to the transformation to mucous membranes. When a natural mucous membrane is destroyed, it is very common for nature to supply a new one. This is, of course, not always a disease, any more than inflammation is always a disease. When a fistula takes place by the side of the rectum, or in the perineum, it will acquire a lining membrane of a mucous character. Even in an old purulent cavity you will sometimes see a lining of this character. In the lungs, when an abscess takes place and does not heal, but remains for a considerable length of time, the internal secreting surface acquires the character of a mucous membrane.

When a new mucous membrane is formed in the lungs, you will have a cavity sometimes exist for years, and both the fistula and the cavity will be lined by something like mucous membranes. In all instances of this newly-formed mucous membrane—form-

ed through disease, although by a restorative process—you will find it to be one of the most simple character. You will never see a new mucous membrane so perfect as that of the intestines—it will never exhibit such villi; in its utmost state of perfection it is only like the mucous membrane of the urethra. The urethra presents one of the most simple specimens of a mucous membrane, while the mucous membrane of the intestines presents one of the most perfect description.

Transformation to Skin.—When the skin is destroyed it may be reinstated by fresh skin. If a mucous membrane be exposed to the atmosphere, it acquires all the character of skin. As I before stated, skin and mucous membrane may be always classed together; but if a mucous membrane be completely exposed to the air, so as to be dried, it becomes exactly like skin. You will have an opportunity of continually seeing this in the instance of prolapsus of the womb. When the womb falls through the vagina beyond the external opening, and there remains—is not kept up by art—you will find the vagina become dry, shrivelled, and pale, exactly like skin; and I presume that if it were allowed to remain there permanently, you would hardly be able to tell the difference between it and skin.

Transformation to Fibrous Membrane.—The conversion to fibrous membrane is very common, and it is particularly the cellular membrane which undergoes that process. The cellular membrane will become indurated, so as to form cords; it will be indurated so as to form a continuous membrane of a fibrous character; and frequently we have it accumulated and indurated into a mass. In all these cases fibres may be seen, and in the midst of these fibrous bands which you observe, there is cellular membrane to be discovered not yet perfectly changed.

You may have tumors consisting of fibrous membrane or of cellular membrane yet imperfectly converted into fibrous, and you may have them pretty uniform throughout, homogeneous; or again, you may have them in lobes; and again, in granules. Sometimes they will become a little vascular, and sometimes very vascular, exactly as we see various natural fibrous membranes differ. They are more or less firm according to the change which has taken place; sometimes you may tear them to pieces, and sometimes you may reduce them entirely to pulp; but occasionally you cannot do this, and they are exceedingly hard. The structure surrounding one of these tumors is often perfectly healthy; sometimes it will be actively inflamed, and sometimes it will waste away. These tumors will occasionally take place after accidental inflammation, and sometimes they will occur

without any obvious reason whatever. We sometimes see that, after the cellular has been converted into a fibrous membrane in this way, it becomes still harder, and forms cartilage, and even bone.

Transformation to Cartilage.—The transformation to cartilage is also a very common morbid process, and takes place exactly under the same circumstances as the conversion to fibrous membrane, and very often succeeds it, and is present with it. What becomes fibrous membrane at first, will often become cartilage afterwards. Now cartilage, if of diseased origin, is very often formed of cellular membrane in the substance of organs, and frequently between the different tissues, so that it is common to see it under a serous membrane. When you see the pleura cartilaginous or ossified, or the pericardium, it is the cellular membrane immediately underneath the serous membrane which is converted into bone. When cartilage is formed in this way, it is sometimes in mere little points, and sometimes in patches. This is a very common occurrence in arteries. You will see spots in the interior of an artery of a different colour from the rest, excessively firm, of a cartilaginous character; and these are formed under the lining membrane of the inner coat of the artery. Cartilage is frequently formed like a fibrous tumor, in lumps, so that you have hard knobs. It is very common after chronic inflammation of a serous membrane, when the whole is thickened together, to find a knob of cartilage here and there. Serous cysts frequently have knobs of cartilage in different parts, and frequently they are cartilaginous to a great extent. These knobs are found in the substance of different organs, but I believe they are for the most part cellular membrane, extending most frequently from the surface into the substance, and increasing in extent.

You will sometimes see new cartilage loose in a cavity. It is very common to see it in a joint, and it is not uncommon to see it in the veins of the pelvis. I believe they are formed under the synovial membrane in the one case, and under the lining membrane, or inner coat of the vein, in the other. They protrude till at length they have a peduncle, which peduncle becomes thinner and thinner till the whole is detached, and then you have a loose cartilage in the joint, or in the veins. I sent a specimen to the University last week, of the veins of the pelvis, in which there were a large number of cartilages which you could move about from one portion of the vein to another like peas.

The transformation to fibrous membrane and cartilage so frequently runs into each other, that you have fibro-cartilaginous transformations.

Transformation to Bone.—Induration, how-

ever, proceeds frequently farther than this. The natural change of cartilage into bone is continually observed to take place in disease. After cartilaginous transformation has existed for a length of time, you frequently find bone produced. It is said that the cellular membrane, fibrous membrane, and cartilage, strictly and positively are the only parts of the body that are ever converted into bone; but whether they are so or not, they are the parts which by far the most frequently are so converted. The loose cartilages which I mentioned as frequently existing in the veins of the pelvis, frequently become bone; so that you have hard bony substances loose in these veins, and these go by the name of *phlebolites*—vein-stones.

When bone is deposited, the deposition may take place in the form of minute granules, so that the part is rough; and in this way you frequently find it in the valves of the heart;—or it may be deposited in the form of scales, a character in which you frequently see it inside arteries. When the radial artery is slit up, you frequently find bone deposited in minute scales, which you may pick off from the interior; and sometimes it forms continuous plates. I shewed you, after lecture the other day, a kidney which had been converted into a bony cyst. There was a *continuous* deposition of bone. This is a proper word, because the bone is deposited in a continuous manner, like membrane; but it would be wrong to say *membranous*. Sometimes it is deposited irregularly—in an *amorphous* manner.

It sometimes is deposited abundantly, forming very considerable masses. You will occasionally, in encysted tumors—I have seen at least two or three instances of it—find a lump of bone larger than a walnut, or perhaps as big as two walnuts. Occasionally, bone thus deposited is homogeneous; if you cut it, it is without fibres, without any reticulated portion at all. Sometimes you have it harder than natural bone. Sometimes it has rays or cross fibres, like diploe, with external compact substance. You see an instance of nature's highest efforts and powers in forming new bone, in necrosis, where a deposition to a considerable extent takes place under the periosteum, and which will become a perfect bone surrounding the old bone. Diseased formations of bone are found to consist of phosphate and carbonate of lime, and of some animal matter, just like healthy bone; but the proportion of phosphate and carbonate of lime is exceedingly various, so that you rarely find two cases of bone formed by disease in which the proportion of the two is the same.

These are the chief observations which it was necessary for me to make on transformations. When structures are so converted, we say it is a *transformation*, and that it is an *analogous* structure—that is to say, the struc-

ture which is produced is analogous to natural structure,—to structure seen somewhere or other in the body during health.

NEW FORMATIONS.

But besides these, we have *new formations*—formations not newly made, that is not the meaning of the term, but formations of a new character—something which is new to the body, and which is altogether of a diseased character. These are called *non-analogous* or *heterologous* formations.

The chief of these, of a solid kind, are *tubercles*, such as are seen in scrofulous cases; *melanosis*, which is a formation of black substance; and sometimes a yellow substance, called *cirrhosis*. Besides these, we have certain saline substances produced, which are unknown to the healthy body—for example, when I am speaking of calculi, we shall find a substance called *xanthic oxide* and *cystic oxide*; substances unknown to the healthy body. Some of the liquid substances of inflammation (as, for example, pus) are unknown to the healthy body. The secretion of serum and of lymph, in inflammation, may be compared to analogous transformations; they are liquids subsisting in a healthy body, although their appearance in the part is unhealthy—at least their quantity. But we have a substance produced in inflammation which is unlike the natural fluids of the body, is a *new formation*, and of a liquid character—namely, pus; so that there is an analogy running through all these things. In inflammation we have the liquid products of the part increased—for example, from a mucous membrane we have more mucus secreted than natural, and of an unhealthy quality; and from a serous membrane we have serum produced in abundance; so that these may be compared to those changes of structure in which there is merely an alteration of size and consistence, and in which the part becomes hardened or enlarged. And again: I mentioned that there are some diseases in which there is a deficiency of secretion—as, for instance, ischuria renalis, and dryness of the skin; and these may be compared to softening and atrophy of substance, where there is a deficiency of matter. Then again, we have, in inflammation, liquids secreted which are foreign to the part—for example, we have lymph secreted continually; and though that is natural to the body, yet it is not natural to the part;—and these may be compared to those changes of structure in which one part of the body assumes the character of another part, such as the transformation to cartilage or bone. Then, in inflammation, we have pus produced—a new liquid; or we have, as I shall presently mention, a semifluid substance in some diseases, like jelly, which is called colloid, from being like jelly. Sugar sometimes is formed in the urine, and

the cystic and xanthic oxides just alluded to: cirrhosis, melanosis, tubercle, encephaloid, are new solid formations. All these are called non-analogous or heterologous, being heterodox matters. Now some of these formations may be organized; they are organizable, if such a word may be used. Scirrhus, for example, is of this description; fungus hæmatodes, as it is called, or encephaloid tumors, may be organized, but tubercular substance cannot be organized. Scrofulous tubercles may become bone, but more frequently they are softened down, and pus is seen; but they are not substances capable of being organized and forming part of the body.

Tubercles.—I shall begin the consideration of new formations with tubercles. The word *tubercle* has been employed in a vague sense; it is used by one person in one sense, and another in another; but there is now a great tendency to limit it to a scrofulous deposit. All small tumors in the substance of organs and serous membranes, or indeed upon the skin, have been called, and are still called, tubercles; but the French have adopted the custom of restricting the word to tubercles of a scrofulous character, and it is in that sense in which I wish now to speak of it.

A scrofulous tubercle is of a yellowish white colour, and usually round, varying from the size of a millet-seed to a nut, and even still larger. It is firm, but friable: although it is tolerably hard, yet if you take it between your finger and thumb, you find it will break down, unless it have been converted into bone, and then, indeed, it is no longer a tubercle. Its character is curdy or cheesy, and it softens down into a substance which is non-analogous to the rest of the body: it softens down into a sero-purulent fluid; but in this fluid you still have the friable tubercular substance, forming flakes within it. When the matter is let out altogether, you see a curdy substance floating in it. When a tubercle softens down, the liquid of course has a tendency to escape, like the matter of an abscess. An abscess is formed, which either increases or remains stationary for a time, and at length, when it is discharged, there is an attempt on the part of nature to heal the part up—to produce cicatrization. These tubercles not being an organized substance, and not being organizable, must increase by *juxta-position*, exactly as stones would do, or any thing else inanimate.

It is supposed by some that a tubercle is liquid at first, and becomes harder afterwards: I should suppose that every thing in the body, when first secreted, must be liquid. One cannot conceive that vessels will pour forth solids; they all appear destined to hold liquids, however short a time the matter may remain so after its escape: it may become hardened immediately. Neither do I

not suppose that any thing can be absorbed but liquids, and it is the opinion of Cruveilhier, an eminent morbid anatomist, that tubercles are liquid at first, and Andral, an equally industrious and sound morbid anatomist, entertains the same opinion. Although they become of a yellowish white, yet they are at first greyish and semi-transparent, and the semi-transparency gives a farther reason for supposing that they are originally formed liquid. It is clear that they do undergo changes, first having a transparency, and then they become yellow, and we may suppose that there are more minute changes still, but which occur at too early a period to render it possible for us to discern them.

They soften down, it is said, by a deposition and secretion around them, which penetrates them, and breaks them up; but I am not myself satisfied that this is a true account, because we find them continually softened in the very centre, without any liquid which could have got into them. We find them firm externally, and liquid within. I therefore should suppose that the softening of the tubercles was really a chemical change, and their not being organized forms an additional argument in favour of this opinion. If there be a deposition of external liquid matter, which goes into them, certainly the external part ought to be softened down, as soon, at least, as the interior; but they generally soften at the centre. However, it is not invariably that they are softened down at the centre; they will sometimes soften in other parts, especially upon the surface. When you see them softened down, an abscess is formed, and the tubercular matter escapes. It is very common for fresh tubercular matter to be deposited within the cavity after the discharge of the contents of the same character.

When tubercles are not softened down—when they are firm, they are found to consist chiefly of animal matter, with some muriate of soda, phosphate of lime, and carbonate of lime, and traces of oxide of iron. This, however, matters little. It is right to ascertain these points as far as possible, in the hope that it may lead to some important discovery hereafter; but still we do not yet understand their nature better by understanding of what they consist. Some have an exceedingly firm cretaceous earthy matter. Of course, the same substances are found in all of them, but in proportion as they are firm do the phosphate and carbonate of lime abound, compared with the other materials.

Now these tubercles are seen almost everywhere—you may find them in almost every part of the body; but they are more frequently found in free cellular membrane, or in the cellular membrane which is a component part of different organs. Besides these situations, they are found in the liver, in

the brain, in the kidneys, in the testicles, in the lymphatic glands. They are also found in the air-cells, and in the parietes of the air-cells of the lungs; and they are found also in the lymphatic vessels themselves.

In adults they are by far the most frequently found in the lungs, and next to the lungs, in the small intestines. In 350 adults with tubercles in various other parts of the body than the lungs, Louis, a French writer, found one only whose lungs were free from them. When tubercles exist therefore in adults, they are most frequently found in the lungs, and next to them come the small intestines. So great is the tendency to them in the lungs, that Louis has made the calculation I have just stated. Speaking still of the adult, after the small intestines comes the mesentery in point of frequency of this affection, so that all other parts stand at a very great distance with regard to the frequency of tubercles in them. The intestines and mesenteric glands stand at a certain distance from the lungs in point of liability to this disease, but all other parts stand even at a greater distance from them. The liver is a part in which scrofulous tubercles are less frequently found than any other; but they are by no means uncommon in the spleen, particularly in infants. In infants it is very common for other organs than the lungs to contain tubercles, while the lungs remain free, which is just the reverse of what is observed in adults. In infants you more frequently find many organs affected at once than in the adult, in whom nothing is more common than to find them only in the lungs. In infants, too, the proportionate frequency of tubercles in different organs, is not the same as in adults. The frequency does not pursue the order of lungs, intestines, and mesentery, but follows quite a different rule: for example, they are far more frequent in the spleen, in the mesenteric glands, or as they are called, the mesenteric ganglia, and in the bronchial ganglia. They more frequently affect those three parts than the lungs, and they are much more frequently found in the nervous centres, the brain, and the spinal marrow of infants, than of adults. They are very rare, however, in the fœtus during its first months, but about the fourth year they become far more frequent, though they are then not very numerous. From the fourth to the fifth year they are found to be very frequent, and to attack many organs at once. So frequent are they in children at this period of life, at least in Paris, that Lombard says three-fourths of them perish from tubercular disease, or at any rate there is a tubercular deposit found somewhere or other, after death. After this age they become less frequent, till the period of puberty, though they are still more numerous than before the fourth year. From the fourth to the fifth year they are

very frequent; then they decline for a time, but still they are more frequent than before the fourth year. It is found that children are more free from tubercles during the second year than at any other period before the fifth.

Here are some beautiful drawings executed by Dr. Carswell, representing tubercles in all their various stages. Here are numerous specimens of transformations and new formations. Here is a specimen of ossification of the trachea. Here is an ossified cyst. Here you observe ossification of the valves of the heart. Here is an instance of an ovarian tumor containing bone. Teeth, and even hair, have sometimes been found in tumors of this description. Here are, &c. &c.

OBSERVATIONS

ON

PULMONARY HÆMORRHAGE;

Taken from a Clinical Lecture delivered at the Middlesex Hospital,

By DR. WATSON.

[Concluded from page 628.]

NEXT to tubercular disorganization of the lungs, the most frequent source of pulmonary hæmorrhage is to be found in organic disease of the heart. It is stated by Chomel, another distinguished French pathologist, that the disease in these cases is most commonly situated in the right chambers of the heart. But this statement seems to be borne out neither by reason nor by general experience. The only alteration in the right cavities of the heart which we could suppose likely, *à priori*, to cause pulmonary congestion, and thereby hæmoptysis, would be increased strength and thickness of their muscular substance—a morbid condition which is comparatively rare on that side of the heart, and which, perhaps, would not suffice for the production of hæmoptysis, even if it did exist. The immediate effect of any obstacle to the free passage of the blood in the right chambers of the heart, would be to gorge the liver and the system of the vena portæ, and to prevent the lungs from receiving their due proportion. On the other hand, any material obstruction existing in the left auricle or ventricle will impede the return of the blood from the lungs—lead to its accumulation there—give rise to mechanical congestion—and so dispose to pulmonary hæmorrhage. And, in point of fact, we find that hæmoptysis is very frequently the result of disease in the left side of the heart. And this leads me to speak of one very remarkable morbid condition of the lungs, which is often directly connected both with pulmonary hæmorrhage and with cardiac disease, though it is not perhaps always

or necessarily associated with either. You have seen an example of it within the last day or two. The morbid state to which I allude, though far from being infrequent, was scarcely noticed by or known to pathologists, until Laennec described it under the title of *pulmonary apoplexy*. It consists of a variable number of dark, hard, compact masses, situated in the substance of the lungs, and for the most part very exactly circumscribed, their ordinary magnitude being (according to Laennec) from one to four cubic inches. Occasionally the pulmonary substance is broken down or torn by the extravasated blood, and in these cases, perhaps, the resemblance between the injury done to the lung, and that which is done to the substance of the brain in *cerebral hæmorrhage*, is tolerably close. In the greater number of cases, however, there is, I believe, no such laceration of the pulmonary substance; but one or more of the lobules of the lungs are gorged and crammed with blood, which has been poured out from the surface of the mucous membrane. These lobules, as you probably know, have no direct communication with each other, but are isolated (except where they severally open into the bronchial tubes from which they spring) by a distinct investment of cellular tissue; and it is to this peculiarity in their structure and disposition that the exact limitation of the dark red, indurated masses, is to be attributed.

Now, under their most ordinary occurrence, the principal symptom of the formation of these masses, is *hæmoptysis*, usually severe and abundant; and the principal cause is disease of the heart, usually of its left auriculo-ventricular orifice.

Laennec, indeed, speaks of this pulmonary apoplexy as if it were the cause of the hæmoptysis; but this surely is an incorrect view of the subject. The partial engorgements, and the hæmoptysis, are not mutually connected with each other, as cause and effect; but they are concurrent effects of the same cause—of that cause which gives rise to the extravasation of blood—part of the blood so extravasated passing upwards and escaping through the mouth, and part being driven in the contrary direction, into the ultimate divisions of the bronchi, and blocking up the whole tissue of a single lobule, or of a bunch of contiguous lobules. Andral conceives that the sanguine effusion takes place in the ultimate air-cells; and he applies to this form of disease the term *pneumo-hæmorrhage*, to distinguish it from ordinary hæmoptysis, which he calls *broncho-hæmorrhage*. But it seems to me more probable, that the seat of the effusion is (sometimes at least) in the larger branches of the air-tubes, and that the blood is forced into certain of the pulmonary lobules by the convulsive efforts to respire made by the patient.

when threatened with suffocation by the copious hæmorrhage—at least this appears a reasonable mode of explaining the occurrence of this circumscribed engorgement in several, and sometimes distant parts of the lungs, at the same time; and if the blood should be poured out from the mucous membrane during expiration, it is easy to conceive that certain portions of the lung may become so completely crammed with blood, as to preclude any subsequent admission of air: and that the appearances presented in pulmonary apoplexy may be occasioned in this way, I am convinced by a remarkable case in which they did occur, when the blood was introduced by the trachea itself. This happened in a patient of mine in this hospital, who was suddenly suffocated by hæmorrhage into the larynx, from an ulcerated opening in one of the lingual arteries. There had been no symptom of any pulmonary complaint, nor was there any morbid appearance in the lungs, except those which resulted from the presence of the blood that had been poured into them through the trachea. There were numerous hard, but not very large masses, precisely resembling those described by Laennec as constituting pulmonary apoplexy*.

That vascular engorgement of the lungs will be the necessary consequence of any obstruction to the free passage of the blood through the left cavities of the heart, is, I think, sufficiently obvious; but there probably will be a considerable difference in the effect produced by such an obstacle upon the pulmonary circulation, according to the exact place of the impediment. Thus if the obstacle was situated at the commencement of the aorta, the blood would tend to stagnate in the left ventricle, which would gradually dilate under the continued pressure of the contained blood, and gradually become thick and strong, in consequence of its own increased efforts to propel its contents; but the mitral valve being healthy, would form in some sort a shield between the lungs and the mechanical obstruction at the mouth of the aorta, and would have the effect in a great degree, and for a time, of preventing the regurgitation, or remora, of the blood in the lungs. But if the disease be situated in the mitral valve itself, converting the auriculo-ventricular orifice into a narrow, and at the same time a permanent opening, the blood which is poured into the left auricle by the pulmonary veins will escape from that chamber, in its onward course, with difficulty, and will even be in part driven back again into the auricle, through the unyielding aperture, at each ventricular contraction. In this case the whole stress of the congestion will be felt in the lungs; and it is under such circumstances that we should expect to

meet with copious pulmonary hæmorrhage, and with what we call pulmonary apoplexy.

And, in truth, this is the morbid affection with which we find pulmonary apoplexy, and its accompanying hæmoptysis, most commonly associated. You have just seen an example of it in the case of a patient of my colleague, Dr. Hawkins, the third case, viz. of death within the week, to which I before alluded. I need only remind you of the principal symptoms, as recorded in Dr. Hawkins's case-book, and of the chief morbid appearances, which you have had an opportunity of examining. This patient, James B—, 32 years old, came into the hospital on the 15th November; he had general anasarca, and difficult breathing, and he complained of pain in his chest. The heart's action was audible beyond its natural limits, and was attended with the "*bruit de soufflet*." Eight days after admission, with increased pain in the chest there came on hæmoptysis; and on the 3d of December he died. The pericardium was found full of fluid, the heart was enlarged, and the mitral valve was firmly and extensively ossified. There was some fluid in the left pleura. The lungs were gorged with fluid, especially on the left side; and in each lung there were large circumscribed spots, or portions, consisting of extravasated blood.

I do not expect, or wish, that one instance of this kind should satisfy you that the coincidence of these remarkable morbid states of the lungs and heart is of common occurrence, or more than accidental. Although I think I have shewn, by *à priori* reasoning, that the one is likely to occasion the other, the assurance that it sometimes really does so, requires the evidence of several such examples.

In October 1829, a married woman, 33 years old, became my patient here under the following circumstances:—She had been ill, according to her own statement, a month only; her abdomen was full of fluid, and her legs and thighs were anasarous. She had been subject to palpitation of the heart, and during the preceding week she had expectorated, at intervals, a considerable quantity of blood.

Leeches, and subsequently a blister, over the præcordial region, with diuretic medicine, removed all external appearance of disease in less than ten days; and she left the hospital believing that she was quite well. Rather more than two months afterwards she again presented herself as a patient. She then complained of palpitation, and pain in the situation of the heart, on the least exertion, and was unable to lie down. In the course of the two previous days she had twice coughed up clotted blood, to the amount of half a pint on each occasion; and the hæmoptysis still continued in diminished quantity. Her pulse was feeble and extremely irregular; she had no dropsy.

* Vide Med. Gaz. vol. iii. p. 156.

Perfect repose, and the employment of similar means to those from which she had before received so much benefit, mitigated, but did not remove, the most distressing of her symptoms. On the 9th day after her re-admission, whilst talking cheerfully with some of the other patients, she all at once complained of pain in her head, sunk back in the bed, and expired in about a minute.

In the brain there was considerable venous turgescence, and some serous fluid beneath the arachnoid, but no other trace of disease. The left pleura contained a large quantity of limpid fluid. There were two well defined, hard, dark-red masses, occupying the middle portion of the lower lobe of the right lung.

Some fluid was collected in the pericardium; the right cavities of the heart were dilated; the left ventricle was remarkably small; the mitral valve was rigid and unbending, and the cartilaginous ring to which it is attached was much thickened, so that the auriculo-ventricular orifice was greatly contracted, and yet incapable of being perfectly closed.

It is curious that in the same year, 1829, three cases, each presenting the same combination of morbid appearances in the heart and lungs, occurred in St. George's Hospital; they were made the subject of a very interesting paper by my friend Dr. J. A. Wilson, which was read at one of the evening meetings of the College of Physicians. Dr. Wilson was the first person, as far as I know, who publicly noticed this fact of the frequent dependency of pulmonary apoplexy upon disease of the mitral valve.

In the first of these cases there was effusion into the cavities of the pleuræ; a dark cubic mass in each lung; the mitral valve was so much thickened that the orifice was reduced to a mere chink; there were large and numerous coagula in the left auricle.

In the second case also there was effusion into the pleuræ, and many compact black masses in the lungs. The heart was of twice its natural size, and all its cavities were dilated. The thoracic aorta was small; the mitral valve thickened, so as to leave between its edges a narrow, permanent slit. It is worthy of remark, that in this case the effects of the venous congestion appear to have extended even to the right side of the heart, and to parts yet anterior to this in the order of the circulation. The pulmonary artery was larger than the aorta, the right ventricle much dilated, and the liver firm and of a deep mulberry colour. The leading symptoms had been orthopnoea, violent action of the heart, and general anasarca.

In the third case too there had been anasarca, abundant pulmonary hæmorrhage, and pain extending across the epigastrium. The pleuræ contained a quantity of serous fluid; there were several of the masses which characterize pulmonary apoplexy in each

lung, and there was great disease of the mitral valve. The tip of the little finger could not pass between its edges; and the auriculo-ventricular orifice was still further obstructed by a warty growth from the surface and border of the valve. The left auricle was entirely filled with a black coagulum of blood. You will understand how materially the passage of the blood must have been obstructed when I state, that upon stretching out the auricle and filling it with water, after the heart had been removed from the body, the diminished aperture did not permit more than a few drops to pass at one time into the ventricle.

Cruveilhier, in his lithographic delineations of morbid structures, now in the course of publication, represents and relates a case of pulmonary apoplexy, in which the left auriculo-ventricular orifice of the heart was greatly contracted.

It is right, however, that you should be aware, that there are instances of pulmonary apoplexy on record, in which no disease of the heart was noticed; and that there are some few examples where the appearances proper to pulmonary apoplexy were met with in the lungs, although no hæmoptysis had been observed.

Whilst upon this subject, I wish to direct your attention to one more case of pulmonary apoplexy, which is related by Andral; for it bears directly on the question previously touched upon—of the possibility that hæmoptysis may sometimes be the exciting cause of tubercular phthisis.

A man, ill of chronic peritonitis, had been for nearly two months in La Charité, and had never presented any morbid symptom which had relation to the organs of respiration; he had had no cough, and he breathed easily. One evening, for the first time, he suffered some dyspnœa, and in the course of that night he spat up a large quantity of florid and frothy blood. For the five following days the hæmoptysis continued abundant, then it diminished by degrees, and at length stopped; but the patient continued to cough, and to breathe with difficulty. Sometimes afterwards the spitting of blood reappeared, and the patient, already exhausted by the peritoneal affection, soon died.

In the right lung there were found several masses of a brownish red colour, exactly circumscribed; constituting, in short, that lesion which Laennec has described under the name of pulmonary apoplexy. One of these masses contained a considerable number of small, granular bodies, of a yellowish-white colour, and presenting all the characters of miliary tubercles in an early state. Two others of the apoplectic masses contained each a very small number only of these white granules; and in the remaining masses no tubercles at all could be discovered; nor was there any

trace of them in other parts of the lungs; but they were numerous in the substance of the false membranes of the peritoneum.

Andral argues that, in this case, the partial sanguine engorgements which were found in the lung could not have been occasioned by the presence of tubercles, because, in the majority of these masses, no trace of tuberculous matter could be perceived; on the other hand, their existence appears to have been connected with that of the engorgements, because no pulmonary tubercles could be seen except in the substance of some of these. He concludes, therefore, that here the formation of tubercles was consecutive to the partial congestions of blood in the lungs; and consequently that sanguine congestion, and the hæmoptysis, which is a sign and effect of such congestion, may, in certain constitutions, be inceptive of the formation of pulmonary tubercles, and so be regarded as a cause of phthisis.

Upon the whole, the occurrence of hæmoptysis, considered in reference to the probable duration of existence in those who are the subjects of it, is of melancholy omen.

I believe, that if from any given number of persons who have spat blood, we subtract those in whom that symptom is connected with irregularity in the uterine functions, there will remain but few in whom the hæmoptysis did not depend upon disease, incurable and progressive in its nature, in the lungs, or in the heart; and that if we still further subtract those persons in whom the hæmoptysis is symptomatic of cardiac disease, there will be very few indeed left, in whose lungs the existence of tubercles may not be confidently predicated.

Of course I do not include in this estimate of hæmoptysis, as a prognostic symptom, those cases in which (as in simple bronchitis) the expectoration is merely streaked with blood; nor those in which small quantities of blood are intimately combined with the bronchial mucus, and form the rust-coloured sputa so indicative of the presence of pneumonia.

Of those individuals whom Andral had known to spit blood at some period or other of their lives, there was only one in five whom he did not also know to have tubercular phthisis.

On the other hand, Louis states that for three years he asked all the patients who came before him, and who were not affected with phthisis, whether they had ever spat blood; and the answer was always in the negative, excepting only a few instances in which the patients had received violent blows upon the chest, and the cases of females in whom the menstrual discharge had been suddenly suppressed.

OBSERVATIONS

ON

THE LATERAL OPERATION OF LITHOTOMY.

By R. B. TODD, B.A.

Lecturer on Anatomy and Physiology at the Medical School, Aldersgate Street.

WHEN we consider the several stages of the lateral lithotomy, as performed by the knife and staff, we are naturally led to conclude that a steady and practised hand is the only requisite which the operator needs, in addition to a minute and *practical* acquaintance with the relative positions and bearings of the parts through and in the vicinity of which he has to cut. I am free to admit that a surgeon so provided ought, by the staff and knife, to perform the lateral operation of lithotomy without injury to neighbouring parts; yet how many are there who, dauntless and intrepid, will fearlessly hew off a quarter of the body by amputation at the hip-joint, or will boldly remove the inferior maxilla from its glenoid cavity, and still will take up the knife and staff, I had almost said, with fear and trembling? I believe I may, without fear of contradiction, assert that in this operation the surgeon derives less confidence from his anatomical knowledge than in any other. It is therefore obvious that it would be of great utility to establish some mode of operating which shall perfectly secure the patient from all danger, even in the hands of a timid operator. That mode of lithotomizing is alone to be deemed perfectly secure which shall afford certain provision against injury of the rectum and pudic artery; too free a division of the neck of the bladder, or parts connected with it, and, in fine, against transfixion of the bladder itself. Such security, it must be admitted, is not afforded by any of the modes of operating at present most commonly in use; and, in proof of the truth of this assertion, I would merely allude to the great diversity of opinion existing among surgeons, relative to this operation.

The object of this paper is to detail the steps of a mode of lithotomizing, which, I think, will appear to afford security against the dangers above alluded to, to a greater extent than any other method hitherto devised. It has

been for many years practised in Ireland with great success. I am induced to call the attention of your readers to it, because I do not find it noticed in any of the modern writers on operative surgery, either British or continental*. I have had extensive opportunities of witnessing its performance by the most adroit hands, both on the living and on the dead; and have myself frequently performed it on the dead subject.

The essential characteristic of this operation consists in the employment of a beaked lithotome, for the purpose of incising the prostate, to which it is conducted on a straight director. The lithotome (represented in profile by Fig. 1. of the accompanying sketch) consists of a blade, CD, and a handle, DE. The blade, a semiovoid in shape, and varying in size, is surmounted at its small extremity with a beak, *c*, united to it at an angle somewhat more than a right angle, and by its larger extremity is connected to the handle. Its convex edge is sharp; its strait edge (Fig. 2) is blunt and rounded, continuous with the handle, but tapering towards the beaked extremity. The thickness of the blade is, in a slight degree, greater towards its larger extremity, so as to enable the instrument to act somewhat on the principle of the wedge. The director (Fig. 3.) is adapted, by its pointed extremity, to fit in the groove of a staff, while the *f* shape of the other extremity, and the hollow handle, AB, fixed at right handles to the body of the instrument, constitute an excellent provision for its firm prehension. The groove on the upper surface is deficient in one of its sides near the handle, for the sake of facilitating the introduction of the beak of the lithotome, and is deep where it terminates at the pointed extremity of the director, in order to afford a direct obstacle to the passage of the lithotome.

An ample external incision, the perforation and free opening of the membranous portion of the urethra, by the ordinary staff and scalpel, constitute the first steps of this as of the other forms of the lateral lithotomy. Through the opening in the urethra the director is to be fitted into the groove of the staff, and on it introduced into the bladder.

The staff may now be withdrawn, and the operator inserting his fore and middle finger into the hollow of the director, and applying his thumb against the concavity of its curved extremity, gives that instrument the requisite lateralization, making the groove look upwards and outwards towards the groin. The director is to be held steadily in this position, and kept well up in the arch of the pubis, while the next and most important stage of the operation is being gone through. The lithotome is now to be adapted by its beak to the groove in the director, (the sharp edge of its blade being of course directed downwards and outwards), and pushed freely on into the bladder, the operator taking particular care to keep the handle of the instrument in apposition and parallel with the director. The lithotome being checked in its progress by the blind termination of the groove on the director, is then to be withdrawn, the same precautions being strictly observed respecting the position of both instruments,—as by maintaining both in the same position, as during the introduction of the lithotome, there is no possible risk of inflicting an additional wound while withdrawing it. The incision of the prostate will invariably be found exactly proportioned to the breadth of the blade. The bladder being now opened, the operation is completed in the usual way.

It will be seen that by this operation the surgeon attains the only advantage afforded by the use of the gorget, viz. that of certainty as to the extent of the incision in the bladder, while he is secured from the untoward circumstances so frequently attendant on the employment of that instrument. Mr. Key having experienced the great security and facility afforded by a straight director to the bladder, adopted the operation which he now practises with so much éclat. The operation I have just detailed was suggested in part by the very same principle, and in part by the fact that wounds of the pudic and rectum generally occur while the operator is withdrawing the cutting instrument. On a future occasion I shall contrast fully this with the other methods of lateral lithotomy at present in use.

I should state that this operation cannot be objected to on the score of its novelty. It was originally devised in its

* Mr. Hargrave forms an exception. In his useful work on Operative Surgery, the operation is briefly noticed. Mr. Key, in his treatise on the section of the prostate, alludes to the employment of a strait director to the bladder by Dease.

essential principles so long ago as the year 1750, by Mr. Daunt, a surgeon of great eminence in Dublin. The origi-

nal instruments of Daunt received some slight improvements from the elder Mr. Dease, at the suggestion of a com-



Fig. 2.

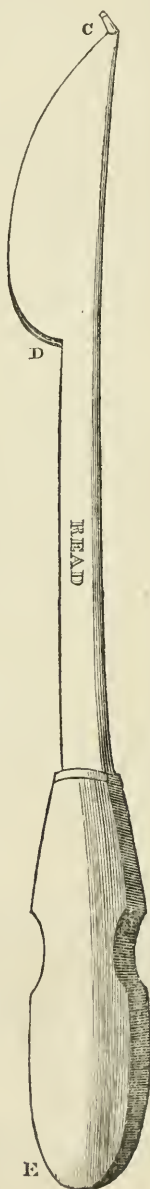


Fig. 1.

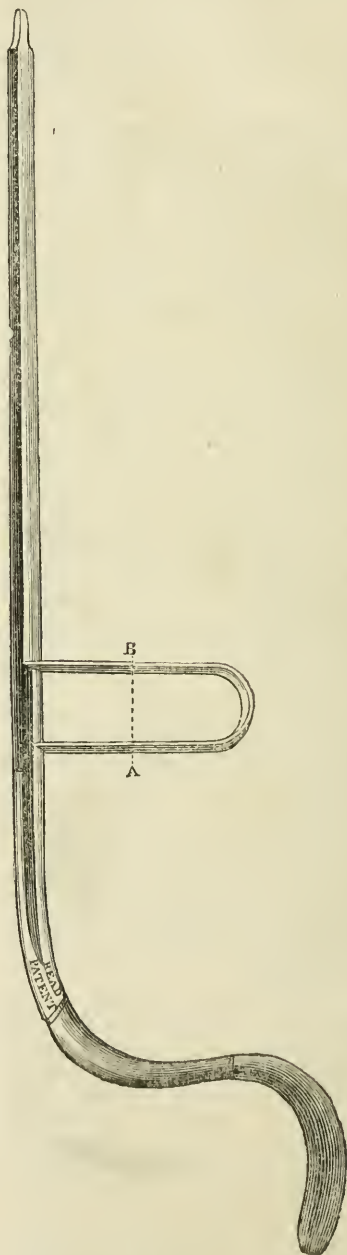


Fig. 3.

mission of the French Academy of Surgery*; but their present improved form is to be attributed to the ingenuity of my much respected friend Mr. Peile.

INJURIES OF THE SPINE.

To the Editor of the London Medical Gazette.

SIR,

MY attention has been recalled to the subject of injuries of the spine, by reading in your last number a report of the proceedings of the Medico-Chirurgical Society.

Mr. Stanley, in his remarks on a case of fractured spine, related by Mr. Barlow, says, "In a case of a similar nature, which occurred under my observation in St. Bartholomew's, a material cause of death was disease of the bladder. An ammoniacal state of the urine very early discovered itself, and in spite of all the care that could be given it, by the introduction of the catheter two and often three times a day for the last three weeks, symptoms of inflammation of the bladder supervened, and, upon examination after death, considerable sloughing was observed." These observations deserve attention; although not *new*, they are not the less accurate. Surgeons of great eminence and skill, beginning with Cline, have conceived the possibility of relieving fracture of the spine with depression, by removing the arch. Mr. Tyrrell has twice performed the operation, but without success. In the first case† Mr. Tyrrell attributes his failure to the cause related by Mr. Stanley; in the 2d case (which I have detailed minutely in the first Prize Essay, Johnson's Medico-Chirurgical Review, vol. vi. p. 601), the same cause certainly contributed to the unfortunate result; and in a case from St.

Thomas's Hospital (Gazette, vol. i. p. 224), which I reported to you, the ammoniacal urine was very apparent and troublesome. Indeed, from all I have seen, and my attention has been much directed to the subject since 1827 (the date of my Essay), I am convinced that unless some mode is devised to prevent the bladder becoming disorganized by the ammoniacal urine, we cannot advance a step in the treatment of spinal injuries.

Clearing away rubbish, and preparing the ground, is always the first step in building; and thus, in practical medicine, clearing away false notions, even although we cannot offer more correct ones, is a material point gained—it is clearing the ground for the superstructure.

Mr. Stanley seems, in the above quotation, to still hold the same opinion as Messrs. Cline and Tyrrell have expressed before him, but which I had hoped the experiments I performed and detailed in 1827, and repeated the following year, had shaken—viz. that from the "bladder having lost its nervous energy, *the urine becomes decomposed in that organ, as it does after it is voided under ordinary circumstances*, and thus acts as an irritant on the mucous coat." Such is Mr. Tyrrell's language; and from Mr. Stanley emptying the bladder only two or three times a day, I infer he thinks the same.

Now, to set this matter right, you must indulge me in a quotation from the Essay before alluded to. After stating that the patient had evidence of inflammation in the chest, which in part contributed to his death, but as this was not a *necessary* consequence either of the accident or operation, the fatal termination, were that the *entire* cause of death, would not militate against the operation being repeated; but I go on to shew that *another* more constant, indeed invariably-accompanying, cause existed, which *must* ultimately have been fatal. "It becomes a question, whether it is not almost certain, from the symptoms, that death *must* have eventually, and at no distant period, terminated the case, from the disorganization going on in the bladder, if the attack of pneumonia had not supervened. Mr. Tyrrell has, in his remarks on his former case, in Sir A. Cooper's Lectures, vol. ii. stated, 'My patient died of inflammation of the bladder,

* Vide an account of these improvements in some excellent observations on Lithotomy appended to an Essay on Hydrocele, by Mr. Dease. Dublin, 1782. Mr. Daunt's instruments differed from the present in that the lithotome was likewise a female conductor, the strait director being a male conductor. Mr. Dease slightly modified the shape of the blade of the lithotome, and made the crest of the director to run along its side with a view of precluding the necessity of lateralization. Mr. Peile, however, rightly judging that the degree of lateralization must vary with each individual case, adopted the original principle of Daunt; his improvement consists in having effected an exchange of sex between the two instruments.

† Vide Sir A. Cooper's Lectures, by Tyrrell.

occasioned by the irritation of the urine, which, I believe, *might have been prevented*; and I should have taken steps for that purpose had I known some circumstances connected with Mr. Cline's experiments relative to injuries of the spine. He invariably found, after paraplegia had been produced by the injury he inflicted on the spinal marrow of dogs, the bladder became affected from the action of the urine on its mucous coat. This, continues Mr. Tyrell, might probably be obviated by frequently emptying the bladder with a syringe, and injecting the fluid, to protect its mucous coat." Accordingly, in the second case (the one I related in Johnson's Journal), the bladder was emptied and washed on several times a day; still the ammoniacal urine appeared; still the irritation of the bladder went on, and destruction of its mucous lining must speedily have ensued. I thought the subject, therefore, deserving further consideration. Having observed that healthy urine out of the bladder required longer to give the ammoniacal test than was allowed between the washings, I suspected *that the urine came ammoniacal into the bladder*; and the following experiment seemed to decide that such was the case:—"After the bladder had been emptied by means of Read's syringe (which is admirably adapted to the purpose) warm water was several times injected and withdrawn, till no trace of ammonia could be detected. A small quantity of clear warm water was then injected, and allowed to remain fifteen or twenty minutes; it was then drawn off, and the *odour of ammonia could be distinctly perceived*. Supposing urine to have been mixed with the water the instant it was injected, and that, under the loss of nervous power, it will decompose as quick in the bladder as out of it, still it is known to every one, that more time than was allowed is required out of the bladder for its decomposition." This experiment I repeated with the same result in the case before referred to in the Medical Gazette. It becomes, then, a matter of demonstration, that in cases of *injuries to the spine, producing paraplegia, urine is constantly arriving ammoniacal in the bladder*; hence the failure of the means hitherto employed, and the destruction of that organ. "Ammoniacal urine is *constantly* passing into the bladder, but is only removed

occasionally, remaining hours in contact with the mucous coat, irritating and ultimately destroying it; whereas the above experiments would direct us to employ measures calculated to keep the bladder as nearly as possible empty; and, if practicable, to neutralize and render inert the little that may remain*." When you reflect, Mr. Editor, how universally fatal these cases have hitherto proved, and that the most important and constant cause of this fatality, in those cases in which the patients survive the injury for some time, and consequently those in which the best, perhaps only, chance exists of affording relief, is the disorganization of the bladder produced by the ammoniacal urine, you will, I trust, not think a portion of your valuable columns unprofitably occupied by giving insertion to this. I am satisfied that, could the disorganization of the bladder be prevented, removing the vertebral arch, in certain cases of fracture with slight depression, might be resorted to with fair chances of success.

In conclusion I may remark, that Mr. Alcock informs us, in his Essay on the Chlorurets of Soda and Lime, that a weak solution of chloruret of soda does not act as an irritant on a mucous tissue, and has the power of completely neutralizing ammonia, and rendering it inert. I have no knowledge of the fact from experience.

Yours very sincerely,
THOMAS H. SMITH.

St. Mary's Cray, Kent,
Dec. 21, 1831.

LOCAL APPLICATION OF CAMPHOR.

To the Editor of the London Medical Gazette.

SIR,

My latter communications to you have contained some striking instances of the powerful effects which the local application of camphor produces. I have reason to suppose that even the action of the heart is under the influence of its power, and adduce the following case in support of this opinion.

A child, suffering from some peculiar affection of the brain, sunk into that state of stupor which is generally the result of effusion into the cavities. The

* Vide Medical Gazette, vol. i. p. 224.

following was the state of the child when I determined to try the effects of camphor on the heart. It was lying perfectly senseless, with a slight flush in the countenance, was squinting, the pupils dilated, the pulse beating about 64 in the minute, intermitting every fourth stroke. I enveloped the whole chest, anteriorly and posteriorly, with a piece of flannel, soaked in the following camphorated solution.

Gum Camphor, \mathfrak{z} iss.
Sp. Vin. Rect. \mathfrak{z} ij. M. f. solut.

In less than five minutes, the child, who had lain perfectly senseless and quiet for some hours, became pale, moaned, and appeared restless and distressed, the pulse beating so rapidly that it was almost impossible to count it. These appearances increasing, I removed the flannel from the chest, and the child in a few hours had returned to the state in which it was previous to the application of the camphor, and died in about twenty-four hours. It was not with any expectation of benefiting my little patient that the experiment was made, but his hopeless state, in my mind, justified the trial. It is not my intention to suggest any peculiar conditions of the system, or of the heart itself, in which this application might be useful, but I can imagine many, and trust that my hopes may not be deceived, when the opinion is again put to the test of experiment.

I have lately used the same local application to the perineum in a case of gonorrhœa dormientium. Previous disease had induced excessive debility, which every paroxysm of this attack aggravated considerably. Nearly three weeks have elapsed since the application was had recourse to, and no paroxysm has actually occurred since.

From ascertaining, by repeated experiments, the great value of this application, and from having successfully applied it in cases that might be said to be somewhat analogous to cholera, I would venture, with some degree of confidence, to suggest its use in that disease. If a case occurred in my own practice, I should, without hesitation, envelope the whole abdomen, from the ensiform cartilage to the pubis, and the corresponding portion of the back in flannel, soaked in the solution, and I should do it in great expectation of some advantage following; for it ap-

pears to me that it is only in cases where the nervous power is absolutely deficient, (not in those in which it is in a state of diseased action) that the application could in any way be prejudicial.

I am, sir,
Your obedient servant,
HENRY GEORGE.

Kensington, Dec. 14th, 1831.

To the Editor of the London Medical Gazette.

Bilston, Jan. 7, 1832.

SIR,
As the following case remarkably assists in establishing the views of your correspondent, Mr. George, respecting the effects of the local application of camphor in some diseases, and as it may gratify him to find his hint has not been lost, if you think it deserves a place in your valuable periodical, you will oblige me by letting it appear as soon as it can do so, without interfering with more valuable matter.

I am, sir,
Your obedient humble servant,
C. M. ASHWIN.

Mrs. G., a short, stout woman, was delivered on Thursday, the 15th December, 1831, of her fourth child, after somewhat more than her usual suffering upon that occasion. She went on well until Saturday the 17th, when I found her complaining of pain in the lower part of the abdomen, somewhat wandering in its situation, and although constant in some degree, yet imitating after-pains. Pulse rather more frequent than was pleasant; bowels had been moved; milk secreted; lochia lessening; tongue tolerably clean; some pain in the head, but only noticed upon being questioned about it. These symptoms were ushered in by rigors during the night before; they were, however, far from alarming. Applied fomentations, and gave a diaphoretic mixture. In about six or seven hours I visited her again, and found all the symptoms materially increased; pain in the abdomen acute; abdomen swollen, and exquisitely tender; pulse greatly accelerated; skin intensely hot; lochial discharge totally ceased; considerable pain in the head, with intolerance of light and extreme restlessness. I drew blood until an impression was made upon the pulse;

directed five grains of calomel to be given every hour, of which she took seven doses. I saw her again in two hours, and found the symptoms by no means abated—increased they could not well be. I then applied a large flannel embued with a strong solution of camphor, as recommended by Mr. George, in one of the numbers of the London Medical Gazette a few weeks since. Upon visiting her early the next morning (Sunday, 18th), I learnt that the pain had ceased immediately upon the above application. Pulse still frequent, but the febrile and inflammatory symptoms lessened. No stool: to take ʒss. castor oil every two hours until it operates. In about four hours from this visit, I was sent for hastily; she was thought to be dying. I found she had had two or three stools, and had been out of bed to pass them, and thus induced syncope: she had taken a few spoonfuls of mutton broth, contrary to my wish, and nausea and vomiting ensued; pain in the region of the uterus returning. Repeated the solution of camphor; gave her tr. Opii gtt. xv. sp. Ammon. arom. gtt. xxx. stat. In the evening the sickness was unabated; the camphorette spirits had not produced such prompt and decided relief as upon its first application, but still the pain was not so great; lochial discharge was returning, and pulse more tranquil. To take the effervescing mixture.

Monday, 19th, morning.—Pain, and tenderness, and swelling of the abdomen, have left her; bowels moved twice in the night; stools copious, offensive, and dark coloured; passed urine freely, which she had done all the time; milk freely secreted; lochial discharge freely and fully re-established. Continue alkaline mixture.

Evening.—The only complaint she makes is of soreness of the tongue and inside of the lips; bowels again moved twice; no mercurial factor nor tenderness of the gums.

From this time she went on progressively improving, with the exception of a little faintness a few days afterwards, arising from her imprudent exertion in leaving her bed to evacuate her bowels, but from which she very soon rallied, and is now as well as usual in the same time after her lyings-in.

CONTRIBUTIONS

TO

THE PATHOLOGY OF THE BRAIN,
IN INSANITY, EPILEPSY, AND
GENERAL PARALYSIS.

BY WILLIAM DAVIDSON,

Member of the Royal College of Surgeons of
Edinburgh, &c. and Surgeon to the Lancaster
County Lunatic Asylum.

“Quisquis enim artificiosè corpora humana secare novit, eorumque singulas particulas diligenter inquirat, ex his latentium morborum causas et sedes faciliè intelliget, necnon accomodata remedia prescribet.”—*J. Riolan, Anthropol. lib. i. p. 15.*

ALTHOUGH the alterations which have been observed in the necroscopic examinations of the brains of lunatics are of great variety, both in reference to seat, nature, simplicity, combination, &c. yet, after having been for a period of several years engaged in the most extensive, and, at the same time, minute and careful dissections, in the largest hospital for insane patients in this country*, I can scarcely entertain a doubt that, in by far the greater number of cases, mental aberration has its primary seat in the brain, and that it is an idiopathic affection of this organ. I may even go a step further, and hazard, without much fear of contradiction from those whose anatomical researches have been equally extensive, and conducted under equally favourable circumstances, the opinion that the brains of insane persons, if examined with proper care and minuteness, indicate, almost invariably, that there has existed, during life, a chronic inflammation in the encephalon, and that this morbid action has committed its principal ravages on the periphery of the convolutions in the cineritious substance and membranes. The chief reasons for the opposite results obtained by pathologists in regard to the proximate cause of mental disease, and the different conclusions drawn from them, several writers embracing the opinion which I have just hazarded, while others will not admit of any material organic cause

* This asylum, probably the largest in Great Britain, containing, at this time, nearly 400 patients within its walls, belongs exclusively to the county of Lancaster, having been built out of the county rates, and is a splendid monument of the just views entertained by the magistrates of Lancaster, of what was necessary for the comfort and recovery of its insane population. It was raised at an expense to the county of nearly 100,000*l.* sterling.

whatever, appear to me to be the following.

1st. The very slight and almost unappreciable nature (at least to our means of investigation) of the lesions themselves, corresponding, in the majority of instances, with a trivial alteration in the intellectual functions; for it seems to be generally admitted that disorganizations of the brain and its membranes are of rare occurrence before the disease assumes the form of complete dementia, or general paralysis.

2d. The almost total absence of that minute care and patient investigation which are required in the anatomist who undertakes such delicate and important researches as those belonging to the pathology of insanity.

In this last assertion I am borne out by the testimony of M. Foville, whose opportunities, as physician to the Lunatic Hospital at Rouen, are only equalled by the depth of his researches into cerebral physiology, and his zeal in contributing, by means of his almost unrivalled opportunities, towards enlarging our acquaintance with the phenomena and causes of mental disease. The opinion of this distinguished writer is so explicit and conclusive, that I cannot refrain from quoting it in his own words.

“Medical men profoundly conversant with pathological anatomy—men whose labours have powerfully contributed to the progress of this beautiful department of science—have often brought to their researches, relative to cerebral lesions, an unaccountable negligence. I have frequently seen the brain examined with a fork and clumsy knife; when the fork was thrust deeply into the brain, through the membranes, and the ventricles laid open, after ascertaining whether they contained water or not, the examination was concluded, unless it was judged necessary to make a few large incisions into the cerebral substance *.”

This is assuredly a deplorable picture of this department of French pathology, and gives us any thing but a favourable impression of the manner of conducting such investigations in a country which has been termed, by a distinguished professor, the classic ground of pathological anatomy. It is hoped

that, with all our defects, we manage these things better in England.

“In the revolutions of medical science (says Dr. Abercrombie) there has been for some years a progressive and remarkable change of opinion, in regard to the mode of conducting medical investigations. There seems to have been a tacit, but very general admission, of the fallacy of medical hypothesis, and the precarious nature of general principles in medicine; and there seems to be an increasing conviction of the indispensable necessity of founding all our conclusions in medical science upon an extensive and accurate acquaintance with the pathology of disease. The facts which are required for this purpose can be derived only from the contributions of practical men.”

Influenced by these considerations, I have been induced to lay before the profession a series of morbid dissections of the brains of the insane persons who have lately died in this hospital; and, although the number of cases which are now presented is too inconsiderable to enable us to advance to the deduction of general principles or conclusions, yet, when taken along with those already published, and the opinions promulgated by men of great experience, they may considerably aid us in arriving at this end. If all those whose opportunities of observation are considerable, were to follow the same track, and content themselves with the character of faithful relaters of facts, we might, in the course of time, aspire to transcend the bounds of limited observation, and thereby ascertain whether they are facts, and whether the facts are universal; whether the principles which I have already proposed are well-founded, or whether they ought to be abandoned altogether, as being reared upon fallacious and inadequate grounds.

It will readily occur to the reader, on perusing the following cases, to remark the frequency of the accumulation of serum either in the sub-arachnoid tissue or in the great cavity of the arachnoid. When I first became engaged in this department of pathological anatomy, I had no hesitation in considering it as one of the results of inflammation of the investing membranes of the brain. Since that time I have met with it in such opposite conditions of these membranes, that I have been induced to take a different view of the nature of

* Dictionnaire de Médecine, art. Alienation Mentale.

this effusion, at least as occurring in the chronic cases of old, bed-ridden, fatuous patients. Under such circumstances it is of almost universal occurrence, being met with in every variety of quantity and quality; and I am disposed to consider it, not as the index of any morbid action during life, "but rather as the effect of such congestions as form more a part of the process of death than a portion of the original disease."

With regard to the other morbid deviations recorded in these observations, I think they will be readily admitted, in most cases, to be the result of decided inflammation, although a few of them appear to arise from causes obstructing the circulation of the brain, such as tumors (yet these may be deemed the ultimate result of inflammation) and possibly venous congestion, and congestion of the posterior great sinuses arising out of the long-continued recumbent position of the patients.

It may be objected to these observations that they are extreme cases, and that they present a much greater extent of morbid deviation than what has been met with by other investigators. To this I would reply, that they are not selected cases; that they are the results of the examinations of those patients who died in this hospital from the beginning of September to the latter end of December, 1831; and that they are laid before the reader because the dissections were conducted with more than usual minuteness, and the facts were recorded with the utmost care and exactness.

Suppose that the lunatics who form the subject of these observations, had been cut off by an attack of inflammation, or of any other disease distinct from derangement; it is more than probable either that dissection would have revealed no organic lesion, or that the traces of the previous existence of insanity would have been of a much less appreciable nature; or that the changes would either have escaped our observation or have been considered not to have any reference to the mental state. But in the majority of the cases now submitted to the public (which there is every reason to believe arose from the action of slow inflammation, but in which "that part of the nervous system that ministers to voluntary motion, and that on which organic life depends, were at first spared, while those por-

tions of the brain which are particularly associated with the manifestations of thought, were labouring under disease.") such changes had afterwards gradually supervened, as were incompatible with the due action of the two former functions, and consequently with the further prolongation of life.

But the morbid action may, through the whole course of the disease, be entirely confined to the intellectual portions of the brain (if the expression may be allowed), producing a train of symptoms altogether referrible to the mind, and often running its course to a fatal termination, by giving rise to permanent changes in organization, without any affection of the other portions of the nervous centres, except what arises from sympathy or pressure. In such cases the fatal termination has, in a great measure, arisen from the progress of the mental derangement and of the bodily changes on which it depends. In the majority of cases, however, we have reason to believe that a different train of phenomena is established, and that this form of the disease passes into another by spreading of the morbid action from one structure to another, and thereby giving rise to general paralysis, marasmus, and all those changes which depend on the destruction of the nutritive functions.

"In cases of simple mental aberration (says Foville), in the greater number of cases that have been inattentively observed, and confounded under the name of meningitis, in which active delirium was present, what do we find?—a simple inflammation of the cortical substance of the brain, of more or less intensity, of more or less extent. But has the derangement lasted any time; has it degenerated into confirmed dementia; has acute delirium given place to a state of coma?—the pathological alteration becomes a ramollissement, an induration, or any other species of degeneration or obliteration of the organ; or it is a compression produced by serous effusion; or (what, at least, is equally frequent) it is a sort of suffocation of the brain, the result of a general tumefaction (gonflement), which is only checked by the walls of the cranium*." In the further support of the views which I have hazarded throughout these observations, I cannot do better

* Lib. citat. art. Encephale.

than conclude with the following very judicious reflections of my friend, Dr. Combe.

“ If, however (says this eloquent writer), we examine the numerous cases in which the mental alienation has arisen from a species of morbid action, involving organic changes by its simple continuance (and such are the examples of chronic meningitis narrated by Bayle), or in which death has proceeded from the mere progress of the insanity, in consequence of the malady on which it depended having gone so far as to be incompatible with life, we shall generally meet with unequivocal traces of cerebral disease, in the various forms of changes of colour, of consistence, or alterations of structure; and in such cases we naturally meet also with the symptoms characteristic of these changes—such as palsy, imbecility, &c. superadded to those of pure insanity *.” Such, at least, are the results at which I arrived, and they seem to be so accordant with what occurs in other organs as to present no unusual difficulties to the pathologist.

CASE 1.—Old. Thomas Bullock, æt. 55, died Sept. 3, 1831. Prominent symptoms referrible to the mental and corporeal functions, were profound affection of the locomotive powers; ulcers of the sacrum and extremities; marasmus; occasional excitement, with pyrexia; complete fatuity; automatic life; frequent screaming, as if from fear.

Autopsy, September 4, 1831.—Cranium very thick. Minute pisiform eminences disseminated over the mesial lines of the hemi-spheres. About a pint of pure serum in the cavity of the arachnoid. Oedema of the arachnoid elevated in the form of small bladders, containing limpid serum. White, thickened, mucous condition of the arachnoid, from subarachnoid effusion confined to the vertex and mesial margins of the hemispheres, and particularly in the intergyral spaces. Clouded, opaque, and thickened condition, from aluminous deposit on its inner surface, of that portion of the arachnoid unconnected with the pia mater, and situated behind the fourth ventricle and commissure of the optic nerves. Pia mater completely adherent throughout to the cortical substance, and, when raised,

tearing up its outer layer. Cortical substance in a state of ramollissement throughout, its whole depth of a lilac colour, and the torn surface granular, bloody, and resembling rotten fruit in consistence. Vessels of the cortical substance much dilated and tenacious. Small, bloody, punctuated extravasations, in the cortical substance throughout. Medullary substance rather soft, and pale in colour. About two ounces of pure serum in the ventricles. Small white granulations disseminated over the arachnoid of the ventricles, giving it a downy appearance: these were met with in greatest abundance over the arachnoid of the corpora striata and thalami. White pulpy ramollissement of the septum lucidum and fornix, of the lower part of the corpus callosum, and of the cerebral matter immediately surrounding the ventricles.

This is one of those well-marked cases of general paralysis but too common in large lunatic hospitals, by which the unfortunate patients are more or less deprived of their muscular powers, as well as of their intellectual faculties. The disease was first described under the name of *Paralysie générale des aliénés*, by M. Delaye, of the Salpêtrière, and more recently by M. Calmeil, of the Charenton, in a good sized monograph on the subject, a work equally distinguished by careful observations and cautious conclusions, and in which it has been the author's object to investigate the condition of the brain, and its membranes, on which the disease depends. This species of paralysis, invariably produced by a chronic inflammation, or its consequences developed in the periphery of the brain, is first characterized by partial palsy of the tongue, giving rise to indistinct articulation, and a tremulous motion of that organ, with stammering, and afterwards by palsy of the extremities and muscles of the face. In the early period of the affection it is extremely difficult to recognise its presence, and requires considerable experience and acquaintance with these melancholy and fatal cases. Later on, the patient is unable to sit up; he is condemned to vegetate the remainder of his existence in bed, until gangrenous sloughs, which attack the nates, and other parts of the body exposed to pressure, disease of the lungs, of the gastro-intestinal mucous surfaces, or the progressive increase of the origi-

* Observations on Mental Derangement, by Andrew Combe, M.D.

nal morbid action in the brain, the *fons et origo mali* itself, precipitate the termination of an existence marked by total incapability of motion, and prostration of both body and mind to a state of negative life. The articulation is completely suspended at this period of the disease; the anus may still be capable of some slight degree of action, but the lower limbs refuse all support to the body; scarcely a ray of intelligence is left; the head droops forward upon the chest; the whole muscular system is sapped, and the evacuations pass unheeded. Still the general health appears but little affected, except towards the last period of "*this species of slow agony*;" the appetite is tolerable, sometimes voracious; the skin is cool, the pulse inodorate, and natural; in short, the profound and pronounced anormal condition of the nervous centres can be divined by him alone who has been a witness of dissections of those who die in this deplorable state of human misery and imbecility. This very formidable and frequent accompaniment of mental derangement is far from being well understood, except by those medical attendants attached to the extensive receptacles of the insane, who have taken advantage of their superior opportunities for investigating its phenomena and morbid appearances. It is one of the chief causes of the very great mortality of insane patients; a mortality which, when compared with that arising amongst those labouring under ordinary disease, is well calculated to surprise persons not experienced in the history of insanity, and who are probably as ignorant of the very existence of this striking species of palsy, as they are unacquainted with its nature and dangerous character.

CASE II.—Old. Margaret Bradley, æt. 45, died September 3, 1831. Prominent symptoms, referrible to the mental and corporeal functions, were marasmus in the last degree; constant confinement to bed; complete fatuity; automatic life; frequent screaming, as if from painful sensations; pyrexia.

Autopsy, September 4, 1831.—Cranium remarkably gorged with blood, as well as the scalp. Sanguineous infiltration in very considerable quantity, and collected here and there in clots along the surface of the arachnoid; also over the surface of the pons, medulla oblongata, and vermiform processes of the cerebellum. Dry, lardaceous,

thickened condition of the arachnoid of the convexities, from albuminous exudation, like the buffy coat of inflamed blood. Adhesion of the pia mater to the cortical substance slight, but evident. Cortical substance marbled, soft, and reddish; the marblings were about the size of a shilling. Vessels of the cortical substance dilated. Convolutions shallow. Medullary substance soft, and of a remarkably pale colour. Sinuses of the dura mater full of dark blood. Remarkable vascularity and redness, with increased thickness of the pia mater; its venous trunks gorged with dark blood.

The other cavities were not inspected.

[To be continued.]

PAPERS AND CORRESPONDENCE

ON

CHOLERA.

CHOLERA AT BISHOP WEARMOUTH.

To the Editor of the London Medical Gazette.

John Street, Bishop Wearmouth,
January 23, 1832.

SIR,

THE following brief, though, I hope, accurate, sketch of the first appearance of the malignant form of cholera in this town and neighbourhood, and its subsequent epidemic state, has been undertaken with a view to the correction of the many erroneous statements which have appeared from time to time in the different public prints. The earliest insertion of it, therefore, in the pages of your journal, will very much oblige

Yours respectfully,
WM. DIXON, Surgeon.

The latter part of the July of last year, and the month of August, were marked in this town and immediate neighbourhood by a very general prevalence of the indigenous cholera of this country, bearing, in most instances, its usual leading feature—that of excessive bilious discharges. At the same time, few persons who were not attacked with actual cholera were free from disordered digestion, most frequently accompanied with diarrhœa, consisting of copious

evacuations of a white or clay colour, great irritability and susceptibility of the mucous membrane of the intestinal canal, with considerable flatulence. During the predominance of the above forms of disease, however, many cases of cholera occurred, of a very unusual and malignant character, and of such a type as induced me, with one or two other medical practitioners, to believe in an unusual atmospheric or epidemic influence as the cause. Two of these cases, in particular, happened in my own practice; and, as I consider them essential to the purport of this paper, I shall subjoin the particulars of the first case, taken at the time of its occurrence.

— Allison, about fifty years of age, the subject of this disease, a painter of earthenware, residing in a low situation on the banks of the river, about two miles above the town, was attacked on the 5th of August last, at four o'clock in the morning, with vomiting and purging, in considerable quantities, of a white, watery, turbid fluid, resembling oatmeal and water, followed by coldness of the extremities, cold clammy sweat over the whole of the body, an anxious livid countenance, eyes sunk in the orbits, and surrounded by a dark halo; blue lips, cold moist tongue, cold breath, voice weak and husky; pulse almost entirely imperceptible at the wrist, and very feeble over the region of the heart; excessive thirst, a sensation of heat and anxiety at the præcordia, violent spasms of the feet, legs, arms, and hands, with blue or purple colour and shrivelled appearance of the fingers and toes. There was pain on pressure over the regions of the stomach and liver. The above symptoms were succeeded by a state of reaction and consecutive fever of a typhoid type. There had not been any secretion of urine for three days. This man eventually recovered. The other, a healthy, temperate man, of the name of Arnott, a labourer at country or farm work, living on the opposite side of the river to the former, was attacked at two in the morning of the 8th of August, with all the symptoms of malignant cholera since observed in the subsequent general prevalence of this formidable disease. He died in twelve hours from the commencement. On particular inquiry, I found that neither

nexion or intercourse whatever with either seamen or shipping.

On the 14th, another case, bearing the same malignant features, happened in the person of a pilot whose name was Henry, and who died in about twenty hours. It has been stated that this man was the means of importing the disease into Sunderland, by having boarded a vessel from an infected port in the Baltic; but these accounts are of the most vague description, for it is not clear that he boarded a ship at all from any suspected place. It is further an ascertained fact, from his wife's testimony, that he had precursory symptoms of the disease, in the form of diarrhœa, upon him, before putting to sea. On the 28th, a shipwright suffered death, also two at the distance of four and five miles from the sea; besides others which either suffered from the disease or were cut off during September and October. One of these, on the 9th of the latter month, I attended myself. It was on the 26th October that a man of the name of Sproat died; his son was removed, a day or two after, to the fever-house ill, and died in a short time. After him, one of the nurses in the Infirmary, who had not been in personal attendance on the individual, but had assisted in removing the body to the dead-house, took ill a few hours after, and died. It must be remarked that this woman was in a state of excessive alarm at the time. The disease, from this time, began to shew itself in greater numbers, and to assume its epidemic character. The persons of those living in the lower and more densely populated part of the town, in dirty, ill-ventilated, damp dwellings, situated in the narrowest lanes and alleys, and who had, through the debilitating effects of intemperance or previous indisposition, acquired a predisposition which might, in all probability, subject them to the deleterious action of any distemperature of the atmosphere, from whatever source generated, or the poison of whatever disease it might bear upon its wings. For a short period the disease manifested itself in isolated attacks, in persons living far apart from each other, and where no intercommunication had taken place. The houses of the sick were always crowded to excess, by their immediate attendants, relatives, and others, from mere curiosity—people in the same

sphere and of similar habits; yet contagion did not affect them with the disease, even in the necessarily crowded room, or cellar, containing the sick, labouring under all the symptoms of the most malignant form of Asiatic cholera! Such was the manner in which the disease first commenced its career—without any apparent chain of seizures, without any traceable connexion in the cases with each other. After a lapse of about a fortnight, however, it began to appear in families; father, mother, and children, occasionally suffered—sometimes struck by the disease simultaneously, at others one after another; and, although it may be urged that the same circumstances of habitation, poverty, and family predisposition, might exert their influence over them, it cannot be denied that the disease, under these *unfavourable auspices*, spread from man to man, or, in other words, to assume a contagious type. In one crowded lane, the disease made its inroad in one night, and attacked upwards of twelve individuals, principally old women, and children of from five to twelve years of age. From the first general appearance of the epidemic, it increased in numbers and fatality up to the early or middle part of December; since which time it has gradually, though irregularly, subsided, the recoveries exceeding, in each day's report, the number of attacks. On the 7th of January, the town was declared free from disease by the Board of Health. Whilst the cholera was raging here, fatal cases happened at some distance, where no connexion with this town could be the cause. One Aswald Reay, living a little below Newcastle-upon-Tyne, took ill on the 26th October, and died with every appearance of malignant cholera. It was set forth that “the disease was not considered a case of continental cholera, because it was not of foreign origin!” So far as symptoms influence opinion, however, there is not the shadow of a doubt but that it was a sporadic case of the genuine disease. November 26th saw another death in Newcastle, and the 7th December ushered the disease into that town; and on the 25th it was in Gateshead, where, two days after, fifty-three patients and thirty-three deaths were recorded in one day. Since then, the villages situated on the muddy and marshy borders of the Tyne have been the seats of the disease, to such an ex-

tent that, at the date of this paper, the mortality in the small village of Newburn, containing not more than about 400 inhabitants, amounts to one in seven. Other small towns and villages, at some distance, generally in a direction westward, are becoming affected by its erratic nature. I visited a case which proved fatal in twelve hours; it was in the person of a female, aged 50, occurring on the 12th December, in a lonely district, unconnected in situation with any previously infected place, and where no personal liability to contagion had taken place. A young man, lodging in the house, died three days after.

Before drawing any inference from the enumeration of the foregoing statements, it will be proper (after the report of the pilot before-mentioned) to notice the other alleged sources of the introduction of cholera into this town by importation.

They are, 1st, the clothes-chests of seamen having suffered from cholera in the infected towns on the shores of the Baltic.

2dly. Ships having been allowed to ride quarantine in this river.

With respect to the first cause, I have not been able to ascertain that any case has been at all connected with the washing of the clothes or opening out the chests of sailors; neither has it particularly prevailed in their families. Moreover, the same cause would have operated in many other seaports on the eastern coast; it is impossible, therefore, that this town could be the only selected one of the many for the importation of cholera.

Secondly, the performance of quarantine in the river. It is a notorious fact that these ships, which have been made the handles to spreading the disease by bringing it here, were only on *precautionary quarantine*; they were from a port in Holland, where we well know the disease has not, even up to this late period, arrived.

On looking at the progress of the epidemic or malignant cholera in this town, it will be observed to have spread, almost exclusively, in the narrow circles of the low, dirty, confined lanes, in the thickly-populated parish of Sunderland; that not more than twelve cases occurred in the upper and more widely built portion, although the freest and most unrestrained communication existed with people equally wretched, and

to all appearance equally susceptible, but living in a purer atmosphere.

The disease, as before observed, is travelling in a western direction principally; if, then, it has spread from this town by *human intercourse*, why, I would ask, has it not been propagated by persons leaving here in the coaches, by carriers and market-people, to every other town and village in this and the adjoining counties? Why should it have been carried to Newcastle and the river Tyne, without scattering its seeds at Gateshead, South Shields, and many other towns, in passing through?

Observing still the progress of epidemic cholera from its source, through its various ramifications, it will strike the inquirer very forcibly that there seems to be insurmountable difficulties in making out its importation into many places. The existence of cholera in the Mauritius previous to the arrival of the *Topaze* frigate, as stated by the medical authorities there, is at least probable.

The inability of the physicians to trace its origin in Orenburg to imported contagion; the conclusive reasons of Dr. Sokolon against its introduction by land; the certainty of three fatal cases noted by a physician in Warsaw (according to M. Chamberet) weeks and months before the breaking out of the disease on the 10th of April, after a bloody battle with the Russians; added to these, the undeniable fact of the malignant form of cholera arising in this town and vicinity, unconnected with any source of importation, and consequently springing up spontaneously, appears clear, that atmospheric phenomena of some kind is the principal source of propagation; although, when once established in any district, it becomes contagious, yet that seems to be but a secondary cause.

Reviewing the foregoing facts and observations, and condensing the evidence therein contained, the rationale may be expressed in the succeeding collaries.

1. That the epidemic or malignant form of cholera arose in this town spontaneously, from some atmospheric distemperature, not by imported contagion.

2. After the disease has been established, effluvia may be exhaled from the bodies of those labouring under the disease, capable of transmitting it to susceptible persons in situations favourable to its generation.

3. A stagnant and confined atmosphere appears essentially necessary to the action of the human miasma, and that it is easily decomposed and rendered innocuous by the admission of pure air.

CHOLERA NOT AN EPIDEMIC.

To the Editor of the London Medical Gazette.

SIR,

THERE are some who have taken advantage of the peculiar laws of propagation belonging to a new disease, to deny its contagious or infectious properties. As, in many cases, their arguments appear to be founded only upon a knowledge of a few facts obtained through another medium, as in the case of Dr. Johnson's letters to the *Courier*, or at other times consist of actual misrepresentations (Dr. Granville's *Catechism*, 3d edition,) they are neither tangible nor answerable. I am induced to make these remarks as an apology for my now intruding a few observations, upon Dr. Ogden's letter in the *Medical Gazette* for January 21st. I do not wish to enter into a lengthened discussion on the essentially contagious characters of the asphyxia pestilenta; I wish merely to point out, that by the Doctor's own showing, it is not an epidemical disease. His letter merits this attention, because he appeals to *facts*, because he has studied the propagation of the disease itself, and because I know that his mind is unbiassed and open to conviction.

In the first place, Dr. Ogden assumes the disease to be epidemic, which is not quite philosophical, and then states that three cases occurred between the 26th and 31st of October; but he says there had been several sporadic cases in autumn: there were four cases occurring in different parts of the town. It is impossible to discuss the real nature of these from the meagre details given by Dr. Ogden. I have had frequent opportunities of conversing with Mr. Dixon on the first of these cases, and always considered it myself as a case of cholera, and not asphyxia. With regard to the others, I was not aware of their occurrence, and we all know the difficulty, as shown at many late inquests,

of fairly ascertaining the real nature of the disease. If any of them were cases of asphyxia, I have no doubt but that they had communication with sources of infection, though, as has happened at Doncaster, Durham, Morpeth, and other places, one or even two persons have died of the disease, and no others have continued its propagation: this is at once a strong argument against an epidemic influence, and a reason for segregation. Dr. Ogden pointed out to me, in Sunderland, the residence of a man and his family, of which himself or part of that family, I forget which, had had the disease that very summer at Riga, and had come to reside in Sunderland previous to the breaking out of the pestilence. Dr. O. in another part of his letter, mentions the liability of bringing chests into the town, which had not been opened for a time, or which even might contain the clothes of a deceased person: not only does such a fact explain away many anomalous cases, but it shows how a ship coming from infected towns may be put in quarantine at one place, and on leaving that, may, on arriving at their own port, when their chests, &c. were taken out and carried home, become the source of infection. Out of the first six cases there was an evident chain of connexion; but in two cases the links of the chain were wanting. In the first place, was there any direct information obtained that case 4, a keelman, of the same trade as case 1 and 3, had no connexion with them? and was it equally well ascertained that Roddenby, whose house I visited to obtain information, without success, had also no connexion with infected persons or things? Dr. Becker mentions several curious cases, in which the patients only at the moment of death acknowledged having been in the vicinity of infection, and when the fact only came accidentally out some time afterwards. I need only add, that four out of six originated from exposure to sources of infection: negative facts could not destroy the evidence of these positive facts; but it would require to show that the sporadic cases could not have been even in the vicinity of the same causes. The disease is now at Newcastle, &c. Why did not the case of Roddenby occur, then, at one of these towns, since, according to Dr. O., there are such things as "forerunners"

of an epidemic. Ships came from Hamburg with clean bills of health, *only* these bills certify, that at that time the town and port of Hamburg continued free from it, (pestilenta asphyxia); and Dr. O. states a little farther on, that "up to this day, there is no suspicion of cholera in Holland." The character of such bills of health requires no comment. The slow propagation of the disease I shall not attempt to theorize upon here; it depends upon laws which there is every reason to believe will be both known and appreciated in this country, before the pestilence leaves us. Dr. O.'s next argument is the similarity of circumstances under which persons are placed in the same house. Becker says, in an unanswerable manner, that at Berlin, and it was the same at Sunderland, in the small fraction of a population which was affected, the constant recurrence of the disease in the same house cannot be considered as accidental. Dr. O. says, the person supposed to have been infected by a person labouring under the disease in the same house, might have had a diarrhoea previous to this latter case. If this diarrhoea was a bilious evacuation, it was a predisposition to disease; if a modified form of the disease, then the patient had been exposed to the same influence as the other case to the infection, from whence an incautions observer might have attributed the origin of the disease. Dr. Ogden then terminates with the startling fact to some, of the immunity of medical men, and even of their patients: I think I could remind Dr. O. of at least two cases where the *relations* of medical men were affected; but with regard to the general immunity, I hope to God that the disease will never take more strongly the character of a pestilence than it has hitherto done. I stated that Dr. O. was an unprejudiced observer. His letter shows that there were merely some difficulties connected with the subject, which have not allowed him to make up his mind on the subject. He terminates, indeed, by saying, that, in many familiar cases, cholera has attacked one person alone; but in others it has committed dreadful ravages, and then in some cases there is much probability of their having taken it one from another by contagion; in others there is not this probability. It is commonly observed that he who writes a long answer has generally the worst

of the argument. I think I am on the the favourable side. I have contented myself with answering Dr. O.'s difficulties; they are none of them objections; and to your readers I leave the trouble of bringing the general features of the propagation of the disease, such, for example, as are crowding upon us, to brush off the last cobwebs of scepticism that may obscure their organ of perception.

I remain, sir,

Your obedient humble servant,
W. AINSWORTH.

2, Woodstock-Street,
Bond-Street.

MODE IN WHICH CHOLERA IS PROPAGATED.

To the Editor of the London Medical Gazette.

SIR,

HAVING, in the former part of our examination of the mode in which the present epidemic cholera is propagated, seen reason to conclude that it has not been spread solely by contagion, we have now to inquire whether it has ever been communicated from one individual to another? There is scarcely to be found, throughout the whole range of the science of medicine, a question of greater difficulty than this. Contagions, at least such as this is supposed to be, are agents which cannot be made apparent to our senses; their existence can only be strictly inferred when no other cause is present to account for the effect, except the previous direct exposure of those seized with the disease to some one suffering under it, or to substances capable of conveying the noxious matter from them. We may, indeed, get over the difficulty of ascertaining the absence of other causes, when we find that the disease occurs in those persons only who have been previously exposed; and if a few instances are met with where this is difficult to be traced, so long as there shall not be reason to believe that it cannot have taken place, our confidence in the existence of this agent need not be shaken. Yet when we find but one single case where this previous exposure, either directly or indirectly, has been impossible, the existence of other causes is proved, whether we are acquainted with their na-

ture or not; and before communication by contagion can be in any case decided, it becomes necessary to shew that these other causes were not here present. It does not by any means follow, that because the sick have been previously exposed, they must have received the disease by contagion. How often do we witness, in other non-contagious epidemics, individuals having communication, and members of the same family, attacked in succession, as greater strength of constitution, or a different state of the general health, enables some to bear up longer against the influence of their cause, or as exposure to atmospheric vicissitudes, and many other circumstances, hasten its operation in others? In all, or in any one of those instances of apparent communication of the disease, which have been brought forwards as proofs of contagion, has it been ascertained that other causes were not present? There can be no difficulty in answering this question in the negative. This disease has not been proved to be in any case contagious; yet, whilst it remains undecided, prudence requires that we should not neglect to make use of any measures which may prevent the possibility of its extension in this manner, except those which, in the amount of injury produced in other respects, should surpass the benefit to be expected in this.

It is true, such is the difficulty of ascertaining the existence of contagion, that in some diseases which spread in this manner only, the effect was witnessed long before the true cause was discovered; and it is also true, and more related to the subject now before us, that there is a fascinating simplicity in this mode of accounting for the production of diseases, which, together with the obscurity involving the operation of other causes, and the disposition in the mind to infer, that that does not exist, which we are too idle to seek, or too ignorant to know where to look for, have of late years retarded the advancement of knowledge, by favouring a hasty and unauthorized belief in the contagious nature of many diseases.

We have at length arrived at the last division of our subject—what other causes besides contagion have produced and extended the cholera? On looking over the observations which have been published, for some general facts to di-

rect our inquiries, we find that its ravages have been almost entirely confined to those persons who were labouring under weakness and disorder of the functions of the alimentary canal. It is also observed frequently to have followed excess in eating or drinking; sometimes it has been traced to the use of a particular article of diet, and has ceased on the discontinuation of that cause; occasionally it has appeared to be excited by the use of particular purgatives, salts especially; frequently it has seized its victims during, or immediately after, exposure to atmospheric vicissitudes and fatigue, and those most exposed have formed the greater part of the number attacked. There are also other agents whose operation on the functions of the digestive organs is, under common circumstances, severe: I allude to grief, fear, and anxiety, emotions which have probably never before been combined in a greater degree of severity, or more generally present. Yet weakness and disorder of the alimentary canal, gluttony, drunkenness, purgatives, bad diet, and fatigue, exist at other times without any disease like this being produced. Hence it appears, that however greatly they may have assisted, they are in themselves inadequate to occasion this effect; there must exist some additional agent, which many have been induced to seek in peculiar states of the atmosphere, because to their influence has been attributed the production of many epidemic diseases, and amongst others one so similar to the present, that it is not yet decided whether they differ in any thing but the degree of their severity; and because we are not acquainted with any other agent which can have been so extensively diffused.

Many have, however, rejected this explanation, for reasons which it is now the proper time to examine. Until the question of contagion had been disposed of, the attention could not have been devoted to this very difficult subject. We may collect the reasons which have been urged for its rejection under the following heads:—

First, It is asserted that the state of the air, believed to produce the common cholera, has not existed here.

Second, That no uniform condition has been found to occur at the places to which the disease has spread.

Thirdly, That nothing peculiar, or at

all adequate to produce this violent effect, has been found anywhere.

To which arguments the progressive course of the disease has given no little support.

With regard to the first reason: it is true that extraordinary heat has not accompanied the progress of the present epidemic; but it would be well to inquire, whether the common cholera has been rightly attributed to this cause. The earlier writers, from Hippocrates to Sydenham, considered the proximate cause of this disease to be bilious, acrid, or corrupt humours, which their pathology supposed to be generated from the putrefaction of the ingesta; (their secretion was a later observation.) Hippocrates has accordingly given us a long list of articles of diet as the exciting cause of the disease, and adds, “*Fiunt autem magis æstate, quia hinc facilius recensita corrumpuntur, vel inconfecta manent.*” Disordered ingesta were found inadequate to account for the common cholera, by the same reason as they are insufficient to account for the present—namely, they do not at all times produce the disease. Our venerated Sydenham observed, that it comes as certainly at the latter end of summer, and at the approach of autumn, as swallows at the beginning of spring, and as cuckoos at the heat of the following season; that though it is epidemic, yet it very seldom lasts longer than August, wherein it began, as if there were some secret and peculiar thing in the air of this peculiar month that impresses on the blood, or on the fermentation of the stomach—some such specific alteration as is only for this disease. This secret thing has been by others supposed to be heat, because, as Dr. Cullen observes, “This is a disease attending a very warm state of the air; and in very warm climates it may perhaps appear at any time of the year; but even in such climates it is more frequent during their warmest seasons; and in temperate climates it appears only in the warm seasons.” No doubt the common cholera has frequently appeared in warm seasons; but is this so invariably the case as to lead to the inference that heat is the cause? It happens with this, as it does with other epidemics whose visits are few and far between, that the length of any man’s life is insufficient to collect the number of observations necessary to any general conclusion; he

will therefore be inclined to assume that connexion invariable, which he witnesses at the present time, and against which no contradictory facts have, in his recollection, occurred; and with regard to this question, there has not been before that urgent necessity which we now feel to examine its truth: now that they are wanted, we find ourselves on the one side deprived of the observations which inquiry would have caused to be made, and overwhelmed with them on the other. We are obliged to take up with such scattered remarks as may be accidentally found in the writings of those who have attended to the influence of the weather on disease. Except in the works of Willan and Bateman on the Diseases of London, we are not able to find anything of value. However, such facts as they may have observed, cannot have been misstated through prejudice, since they did not think of questioning the received opinion. The period through which their observations extend is eighteen years; the number of cases witnessed by both in that time is 200. In Dr. Willan's practice 58 occurred, 49 of which were met with in the course of five summers, between the 20th August and 20th September. In two years only out of the five did any cases occur between the 20th September and 20th October; in this time four are found, and four likewise in the last three months of 1800. In the thirteen years through which the observations of Dr. Bateman extend, 142 cases occurred in his practice, three of them in the spring quarters of three separate years, and three in the winter quarters of two years; 83 in the summer quarters, and 52 in the autumnal quarters, of the whole time. The greatest number of cases which occurred in any one period is, 33 from the 20th August to 20th September, of 1800; 16 in the months of July and August, 1807; 14 in the autumnal quarter, 1810; 11 in the summer of 1815. As to their connexion with the state of the weather, Dr. Willan observes, that the summer of 1806 was the hottest in his recollection; that from the 22d of June to the 19th August 57 clear days occurred, the range of the thermometer being in July from 54 to 81, and in August from 55 to 89; but the disease did not appear during the extreme heat; it was only after the rains of the 19th and 20th of August,

and in September, that these cases occurred. In the summer of 1811, in which the next largest number of cases are found, the season was remarkably dry and warm; no rain fell from the 6th June to 30th July, when two or three heavy showers occurred. The non-appearance of the cholera until after the rain, leads Dr. Bateman to observe, that "they may probably be owing to the occasional rains which have fallen, since rains in the course of a hot season have been frequently found to multiply cases of cholera." In the autumn of 1810, the weather from August to the middle of October was dry and warm, with little variations, scarcely so much as a shower of rain occurring, and the temperature never having reached any high pitch, but being uniformly warm and grateful. The summer of 1815 is remarked to be in general warm and dry. On another period, between the 20th August and 20th September, in 1797, which furnished the second highest number of cases noticed by Dr. Willan, he observes, that a season so damp and unfavourable seldom occurs, there having been scarcely a day without rain for several weeks, and that cases of summer fever and cholera have been numerous and violent. Dr. Bateman remarks on an autumn, 1804, remarkable for the heat of the month of September, the thermometer having reached 77 or 78 for several days in the third week, that cholera was much less prevalent than in the hot months of the year; but that cases of disorder of the alimentary canal, and particularly diarrhoea, were very numerous. In the year 1811, the temperature of the summer was variable, and on the whole moderate, except a few days of summer heat at the end of June and July; August was cold, with frequent showers, and for a few days unusually chill, at 50 or 52; and in the summer of 1814 moderate in temperature, and rather cool, except the last week in July; in each summer eight cases occurred. In the autumnal quarter of 1801, which was preceded by a hot dry summer, continued to the beginning of September, only two cases occurred. In the mild and tolerably uniform temperature of the autumn of 1806, seven cases occurred; and the same number in the cold summer of 1816, which is said to have had scarcely one hot day, and very few without more or less rain. The au-

turn of 1812, in which the greater part of September was dry and warm, and October wet, only one case occurred. In the summers of 1809 and 1805, both cold and wet, seven cases are found in the former, and six in the latter. In the summer of 1813, cool in the early part, uniformly dry, and of moderate temperature in the latter, six cases occur. In that of 1806, uniformly warm and dry in the former, and moderate with heavy rains in the latter part, four cases are found; and the same number in the warm uniform temperature, and remarkably dry autumn, of 1815. In one place Dr. Bateman could not help remarking particularly the occurrence of a case of cholera independent of the (supposed) usual cause; in the month of November, 1812, which commenced with a somewhat unusual degree of severe cold, and some days frost, during which the atmosphere was commonly loaded with a fog, the last ten days were mild but damp; and we have to make our acknowledgments to both, for the means of refuting the assertion, that the disease occurs only in the warm seasons of temperate climates. Cases are noticed in the springs of 1813, 1814, and 1815; there is no particular observation on the state of the weather in the latter; that of 1813 was variable, low, and damp; and the case which appeared in 1814, occurred in a few days of unusual heat in April. In the winters of 1807 and 1811, three cases occurred; nothing peculiar is observed in the state of the weather.

It appears from the preceding observations, at least equal in precision to any made during the present epidemic, that the common cholera occurs under atmospheric conditions of great variety; and that the doctrine which attributes its production to excessive heat alone, is erroneous; and it is not proved that the cause of the common cholera has not been the cause also of the present epidemic. If want of uniformity be any argument against the production of the present by atmospheric influences, it must also be equally decisive against the same agent producing the common cholera. We may again remark, that had the attention been as strongly directed to ascertain the causes of the latter as of the former, and had an equal opportunity been afforded of collecting as many facts in the space of a few years,

we should have in all probability found abundance of parallel facts in each disease. In the writings of Hippocrates, Celsus, and Riverius, there are a few observations which tend to shew that the occurrence of fever with cholera was not unnoticed by them. The authors above quoted frequently notice the general prevalence of fever at the time that cholera, dysentery, and diarrhoea are common; and Dr. Bateman pointedly observes, page 202, that when the dysentery was very prevalent in the summer of 1812, in one case which had commenced as cholera, delirium, with brown dry tongue, frequent small pulse, prostration of strength, and other typhoid symptoms, ensued, and terminated fatally.

The second reason for the rejection of the explanation which supposes the cholera to be produced by atmospheric influences, is, the affirmed want of uniformity in the states of the air at different places. The temperature and moisture of the air at places during the prevalence of the disease, is very seldom stated; and it is chiefly from the different seasons in which it has raged, the difference in climate between India and North Russia, between the dry and burning plains of Persia and Arabia, and the swampy banks of the Ganges and Nile, the difference in the elevation of different places, and close or open situation, that this conclusion has been drawn, supported by a few more particular observations here and there made; and we must acknowledge, that the disease continued its course through a very wide range of temperature, if not in degree of humidity. What degree of uniformity was necessary? This question relates to the sensible state of the atmosphere; we may throw together our observations on it, with the examination of the third assertion, that nothing peculiar or at all adequate to produce this effect is to be found anywhere. If for these reasons the agency of atmospheric influences in the cholera is rejected, we must deny that they have produced any epidemic whatever. All attempts to ascertain what are the peculiar states of the air to which these diseases are to be attributed, have hitherto been unsuccessful. Dr. Bateman observes, with regard to epidemic febrile diseases in general, "that no plausible cause has yet been assigned for their absence or prevalence; meteorological observations have thrown

no light on this subject, and chemistry has not detected any change in the state of the air by which they might be explained;" and yet many fevers certainly occur entirely independent of contagion. Dr. Heberden, on catarrh, *Med. Trans.* vol. 1., observes, that the season preceding one severe epidemic, that of June 1767, was only remarkable for being unusually cold; but then it is observable that the similar epidemic cold of 1762 was preceded by weather as uncommonly warm. The last visitation of this disease (so generally attributed to cold and damp) occurred in June 1831, a season of universal dryness and warmth. Dr. Sydenham, on epidemics in general, remarks, that, although he had observed, with as much diligence as possibly he could, the various dispositions of divers years, as to the manifest qualities of the air, that from thence he might learn the causes of this great variety of epidemic diseases; yet he received no benefit thereby, for he perceived that years, which do agree as to the manifest temperature of the air, are infected with various diseases; and so on the contrary. It is needless to multiply similar examples. These apparent contradictions have induced many persons, with the author last quoted, to seek for their causes not in manifest qualities, but in "occult venoms;" "in some secret and inexplicable alteration in the bowels of the earth;" and in astral influences, reminding us how similar is now the situation of this branch of knowledge to that of chemistry in the dark ages, when men's minds, untrained to true deduction, and unencouraged by the success which now attends the patient investigation of these subjects, were startled by the observation of each fresh phenomenon, and ready to find their cause in any agent whose connexion and operation was unknown and mysterious. It is in vain to seek the causes of these effects in occult influences; and believing that there is sufficient to prove that the main causes of many epidemic diseases are the manifest changes in the conditions of the atmosphere, we shall endeavour to trace the chief difficulties, and reconcile as many as possible of the apparent contradictions met with in the course of this inquiry.

The first difficulty is the want of facts, from the investigation having been little cultivated; the attention of the major

part of the labourers in the field of medical science has been more particularly directed to the more profitable questions—how shall we cure, rather than how shall we prevent, disease: it is only under circumstances like the present, which are of rare occurrence, that its necessity has been generally apparent. The second difficulty is, its very complicated nature. It is only of late years that we have had any standard by which to measure and compare the degrees of heat; those of moisture and electricity are not even yet to be ascertained with any degree of precision, and that at an expense of time and instruments which few can afford. They are combined in such a great variety of proportions, and similar states recur at such distant intervals, that the course of a long life is seldom sufficient to afford the means of confirming or correcting a few conclusions. We cannot have recourse to experiment, as in other sciences, but must patiently await the revolution of years. We are not aware of the approach of epidemics, and it is only after they have appeared that we think of enquiring what has been the state of the air, and are obliged to trust to vague recollections. But, however difficult it is to ascertain the states of the atmosphere themselves, it is far more difficult to discover their influence on the body; such a multitude of circumstances assist or counteract their effects, and the exact degree of whose operation is extremely difficult to appreciate. The laws of life are yet very imperfectly known. The chief contradiction is, the apparent want of uniformity. To this we must observe, that it is not any one degree of atmospheric heat (and the same may, to some extent, apply to moisture and electrical state) which is found to disorder the function of the living body, but in relation to a preceding state; it is the extent of the change. The native of temperate climates, who has spent part of his life in the Torrid Zone, feels, on his return, the hottest day of his own summer cold, and his health is affected by the changes, very different from that of the frigid resident. Those who ascend lofty mountains, experience, during their ascent and descent, very different effects from the same temperature; cold at one time, hot and oppressive at the other. Every day's experience tells us, that we can bear, without injury, vicissitudes at one time, which produce violent effects

at another; when the general health is disordered, it may be by an unfavourable state of the air; it may be by other circumstances. In all epidemic visitations some constitutions are able to bear up against their influence, and, according to the degree of their severity, more or fewer of the community are affected. If, during any epidemic, we find no violent change to account on the effect, we must know that preceding states have not disordered the functions, and rendered them liable to be affected by slighter causes, before we reject the explanation altogether. Before we can prove the cause of any epidemic, as well as of the present, and its mode of operation, we must know the nature of the disease, and what are the peculiar derangements of the functions, which constitute it. This branch of the inquiry is too extensive to be entered into now. If enough has been advanced to shew that the rejection of atmospheric agency was unauthorised, my present object is attained; and this I have attempted from these general facts. A sufficient number of particular observations have not been made, and we are not yet sufficiently acquainted with the manner in which atmospheric states and changes influence the functions of the body.—With regard to the progressive course of the present epidemic, the attention has not been before directed to ascertain the peculiarities of such as are not contagious; we cannot, therefore, deny, that parallel instances are not to be found in others: yet the course of catarrhs over one half the world has been before observed, and some epizootic diseases found to travel in a particular course.—If the theory we have last examined be correct, it is necessary that the pre-disposing and exciting atmospheric states shall have travelled the same course; yet the science of meteorology does not afford us any observations of the usual or occasional tracks followed by these agents. We have omitted a number of observations on the state of the general health of the population at some places during the prevalence of this disease, which give great support to our inferences. In conclusion, we must again remark, how exceedingly imperfect is the evidence which has been published! We could have wished to have ascertained in how many cases, out of the number occurring in each place, communication with the sick had not previ-

ously taken place, in how many it was doubtful, in how many positive, and in how many instances this exposure had existed, without the disease following—what circumstances attended its progress from place to place, both as to apparent communication by individuals, impossible extension thus—and what circumstances suitable to contagious propagation existed without any such effect—what was the general state of the air, especially as regards moisture, both for some time previous to and during the existence of the disease, and what the other prevalent affections, through the same periods.—It is to be hoped that the patience and industry of our countrymen will furnish abundant means of setting at rest the perplexing questions relating to this disease, and that a liberal government will devise some means of combining the labours of all, and not expect that the services of the medical practitioner should be alone unpaid by the state. The highest offices and emoluments are open to the lawyer and divine, while little but honour, a scanty maintenance, and a premature grave, rewards the dangerous and anxious devotion of the practitioner of medicine.

I remain your obedient servant,

JOSIAH ALLSOP.

11, Colemore-Row, Birmingham,
January 24, 1832.

NATURE AND TREATMENT OF CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

I VENTURE to make some few observations respecting the nature and treatment of that destructive disease, called Indian cholera, but must be classed with those who have not seen the disorder. The only excuse that such persons can proffer is, that no certain and satisfactory progress has been made in its cure, by those who have had great opportunities of observation and practice on this lamentable disorder. My observations will be brief; but, if worth consideration, will I hope be readily understood.

From the facts brought forward, my inferences are, that the disease is caused

by contagion, and may produce its deleterious effects in two ways. First, through the rectum, by parties receiving the contagion, and thence affecting the course of the intestines, from places immediately after alvine discharges of Indian cholera patients, or soon after such discharges have been there deposited: secondly, that the contagion may enter the body in some other way, and be received into the system. The first means of infection must be the most sudden in its effects, and probably the most violent.

It will not be difficult to perceive that a specific contagion may exert its influence on a particular membrane, and that this may be effected through the habit or by local application, as in the case of small-pox contagion, which, whether by inoculation or through the habit, equally produces its effects on the skin.

Having thus explained the origin of the disease, it will readily be admitted, through universal testimony, that, in Indian cholera, a morbid secretion takes place in the intestines, which I infer to be of a most irritating nature, overpowering the whole train of vital actions, and too frequently irrevocably destroying them. This is not contrary to our observation of the effects of intestinal irritation in many forms of disease, though carried on in the affection under consideration in a most rapid course to fatality.

If we become satisfied that there is probability in the above observations, our next course is to inquire by what means we may be able to combat or to mitigate these violent and often fatal irritations. I leave the effects of the irritation on the vital powers to be opposed in any of those ways that have been employed, and may seem best to be adopted, confining myself to some observations on the means of mitigating intestinal irritation, as above explained.

Without entering into discussions on the preference of the methods to be proposed, as their advantages, if any should arise, must be discovered from their trial, I would strongly recommend always the means of diluting, as far as possible, the contents of the intestinal canal by clysters of a diluent and soothing character, and these in no case to be omitted. The clyster I recommend should seldom be less than two, and, if possible, three pints, and the first con-

taining a pint of decoct. Papaveris, the rest tepid water; but, in case the decoct. Papaveris is not at hand, then the clyster to be in the same quantity, but with a drachm of laudanum. Such clysters to be administered frequently, say every two hours, for some time, but regulating the decoct. Papaveris or tinct. Opii, as each case may appear to demand. A common pipe and bladder would not do for a purpose like this. I am aware that the use of clysters in this disease is not a new proceeding, but I have not learnt that they have been of the kind and quantity here recommended. In your Supplement, page 493, Sir W. Crichton speaks favourably of clysters with mucilaginous substances, but nothing of the quantity used. If, however, they should be administered for the purpose of diluting the contents of the intestines, nothing is likely to be so effectual as water itself. If, then, by this means the original irritation can be abated, it may be hoped that the habit may, with proper assistance, be able to right itself.

Admitting this highly morbid secretion to be the cause of all the other fatal symptoms, it must be evident that not only the mitigation, but the expulsion of this offensive secretion, ought to become a matter of consideration. This may be effected in some degree by the clysters; but there will be wanted purgatives to assist in unloading the upper part of the intestinal canal. Although various means have been used for this purpose, no purgative has been employed that appears to have gained with practitioners an opinion of preference. Some may, perhaps, in a degree, increase the evil: if it be supposed that the nature of the morbid action on the mucous membrane of the intestines should be tending to erysipelas, oily purgatives may do harm, and others may be too irritating.

Under this uncertainty I would venture to propose a remedy, which, if given in sufficient quantity, may be effectual, and void of the exceptionable qualities above mentioned. This is the Hydrargyrus cum Creta, mixed up in mucilage, which, given in the quantity of 3ss. every half or every hour, as the case may demand, will soon act on the bowels; but to quicken it, if the stomach will allow, some tinct. Rhubarb. may be also recommended. The Hydr. c. Creta has also the quality of not be-

ing readily vomited, and the Creta in its preparation is a sort of soother of intestinal irritation. Should ptyalism be promoted by this remedy, the result might be favourable.—I am, Sir,

Your very obedient servant,
THOMAS SUTTON, M.D.

Greenwich, Jan. 15, 1832.

CHOLERA, WITH BLUENESS, IN LONDON.

To the Editor of the London Medical Gazette.

SIR,

I REQUEST, respectfully, to submit the following case for the consideration of the profession, and have the honour to remain

Your most obedient humble servant,
G. H. WEATHERHEAD, M.D.

14, Bloomsbury Square,
Jan. 31, 1832.

Mr. P., a merchant in the city, had occasion to go to Leeds, where he remained about three weeks, and, in returning, he was obliged to ride on the outside of the coach from one o'clock to three in the morning, on account of a young person being unwell inside.

The day following his arrival in town, he complained of having passed a bad night, with great sickness at stomach, and an oppressive feeling about the præcordia; nevertheless, he went to his counting-house as usual.

This morning (26th instant) he consulted me. He had again passed a restless night, with continued sickness at stomach, pain in the bowels, and frequent watery evacuations, which he described as resembling thin gruel. He had cramps at the extremity of his fingers during the night—pulse 55—countenance pale and sunken—eyes suffused.

R. Tr. Cardam, c. ʒij.

— Rhei comp. ʒj.

— Opii, ʒij.

Aquæ Menthae.

— Font: a. a. ʒss.

M. fiat haustus statim sumendus.

He persisted, notwithstanding my advice to the contrary, in going to his counting-house.

Vespere.—The draught had the effect of quieting his bowels, and when I saw him in the evening his pulse was fuller, and more natural.

R. Solut: Muriatis Morphiae, ʒxxx.

Aquæ Cinnamomi, ʒj h. s. s.

27th January.—He still complains of some uneasiness in his bowels, and of great weakness—eyes not so much suffused—pulse more natural.

Rept. haustus ut heri.

He again persisted in going to the city for the day—recommended him to take a little hot port-wine negus, and dry toast, for luncheon.

Vespere.—Returned from business a great deal worse—countenance collapsed with a dark blue colour of his cheeks and nose, (his natural complexion being a fair florid)—surface remarkably cold—pulse 52., and scarcely perceptible—has had five watery evacuations, tinged red with blood. Had his bed warmed, and he took

Tr. Opii, ʒxxx.

Tr. Catechu, ʒj.

— Cinnamomi, c. ʒij.

Aquæ Cinnam. ʒj. h. s.

28th.—Bowels quieted by the draught, and he passed a good night—this morning he is much better—pulse still slow, and feeble—(remains at home.)

Sumat. Sulph. Quininae, gr. ij. bis in die ex pauxillo vini rubri (Oporto) et aquæ ferventis (vulgo, Negus.)

Vespere.—Has continued tolerably easy all day.

Rept. haustus horâ somni, ut heri.

29th.—Passed a good night, and feels no uneasiness to-day.

Cont. Sulph. Quininae in die et haustus horâ somni.

30th.—Convalescent.

Cont. Quinina solummodo.

The above case suggests the question “What symptoms are to be considered as pathognomonic of the prevailing pestilences, familiarly known under the name of cholera?” Are the reduced state of the pulse, the coldness of the surface, the spasmodic feeling at the extremity of the fingers, and the white and watery evacuations, to be deemed such? If so, perhaps some of your readers may be inclined to believe, with me, that the case recorded above was one of cholera, as it is called, in a mild form. No sensible person, I am sure, can confine his notion of this peculiar pestilence to that type of it alone, which, from its intensity, announces, at its very commencement, its inevitably fatal termination; and, if there be truth

in the adage, that a knowledge of a disease is half its cure, the profession cannot be too early alert to meet and combat this terrific scourge at its first onset, how mild soever its form. G. H. W.

ON THE CHOLERA AT CONSTANTINOPLE.

To the Editor of the London Medical Gazette.

Constantinople, Jan. 1832.

SIR,

It has occurred to me that at the present season, perhaps, any facts relative to cholera may be in some degree interesting; and as such, the following observations, connected with its history and appearance at Constantinople, the method and practice generally adopted there, and in how far it was attended with beneficial results, may perhaps be acceptable. The cholera was first observed amongst us in the month of June last, and raged with more or less violence till the middle of November, when happily the storm ceased. On the first appearance of the evil, there was a general confusion and interruption of business; and each in the exigency provided himself with a variety of drugs, of supposed curative and prophylactic efficacy. The opinions of medical men were not in unison; and questions arising on the usual topics—contagious, not contagious, Asiatic, or simple cholera—much valuable time was lost in discussions, combined with considerable mortality, owing to the uncertainty of practice, and want of appropriate measures, as sometimes happens at the commencement of epidemics. Eventually, however, all agreed as to the name and nature of the new importation: and we began seriously to reflect, with a view if possible to devise and establish some suitable method of prevention and cure. Unfortunately, owing to existing prejudices, and the state of medical science in Turkey, we had not the advantage of post-mortem examinations. The Indian cholera was unknown to me from actual experience, but such, I believe, was the precise form of disease which prevailed at Constantinople. Having all its characteristic attributes, and peculiar malignity, I am perfectly justified in considering it as such; and on inspection of the bills of health of all vessels from the Levant, it

will appear that this was the general opinion. The exact denomination is of importance, as there was some peculiarity in the medical practice generally adopted, and which appears most suitable, consistently with the results of our experience. Where it was possible, venesection was invariably resorted to, combined with the usual helps for restoring heat and the balance of circulation; and to this end even a few ounces of blood trickling down the arm were found more effectual than any other remedy whatever. Under certain conditions, it is well known that the employment of stimulants combined with the antiphlogistic method, is not inconsistent with the principles of sound pathology. We observed, however, that the more cautious and prudent use of that class of remedies at the onset, the fewer were the cases of secondary inflammation; and that in the same proportion, in a variety of instances, the recovery was quick and complete. The first and alarming symptoms subdued, we required no medicines, administering simply copious mucilaginous drinks, and keeping the patient on very strict diet and warm regimen.

The utility of this method was so generally acknowledged, that all persons attacked had themselves immediately bled, and perhaps leeches applied to the region of the stomach and abdomen, previous to calling in medical advice. I do not, of course, presume to recommend the plan unconditionally, as allowances are to be made for the modification of climate, constitution, and other accidental influences. It is, however, undeniable, that in the totality of cholera cases thus managed, about one-fourth only were unfortunate; and surely this number is inconsiderable, if compared with the bills of mortality in other parts. Though no class of persons was exempt, the disease prevailed chiefly among the lower orders, Europeans, Jews, Greeks, and Armenian Catholics. The Turks, and Armenians, properly so called, suffered less, being in comparative affluence, and their houses clean and well ventilated.

All our observations led to the conclusion, that the disorder was not contagious; and as there are apprehensions entertained on this head, and the cholera is a subject of daily discussion among persons not of the profession, it would be useful at least to allix a pre-

cise meaning to medical terms. In my humble opinion *contagious* and *infectious* express one and the same idea, or the property of a certain class of evils to extend themselves from one individual to another within their immediate influence as to proximity and disposition. *Contagious* and *epidemic*, in contradistinction, cannot be employed with propriety, those qualities being often allied, and co-existing under certain conditions. The meaning of contagious being obvious, I have only to observe, that the term epidemic, as is clear from its etymology, is applied to a disorder attacking great numbers at a given period, and, according to circumstances, may be contagious or the reverse. The first case is exemplified in the presence of plague, scarlet fever, or any infectious evil, under the alleged conditions; common inflammatory affections, so general in the beginning of winter, may be adduced as an instance of the second.

I am, sir,

Your obedient servant,
W. M'CARTHY, M.D.

CHOLERA AT MUSSELBURGH.

To the Editor of the London Medical Gazette.

2, Duncan-Street, Edinburgh.

SIR,

As I have had an opportunity of seeing several cases of the cholera asphyxia, and of trying the efficacy of the subsulphate of mercury in one extreme case, combined with bleeding, I shall send it you, contrasting it with another which I also noted at the bedside of the patient, but did not treat.

CASE I.—William Brown, aged 15, very stout, in previous good health, a fisherman's son, is constantly at sea, and in the habit of walking through the water in high boots. Had one loose evacuation at six o'clock. Fell in the water at 10 p.m. and was immersed to the middle of the body about half a minute. Was seen by John Brown half an hour prior to his falling in the water, his face being very pale and much altered. Alexander Ritchie saw him *cramped* before he fell in the water. I saw him at half-past 11. At this time he was cold as ice all over, and strikingly blue. Pulse quite gone in all the arte-

ries, and action of heart apparently paralysed, though I could not ascertain this by the stethoscope, for he was so contorted with spasms that it took four stout men to hold him in bed. The jaws were rigidly closed. I fancied I felt a thrill in the left brachial artery. Mr. Rutherford* had opened a vein without effect, and looked for his death every minute: he declared to me it was the worst case he had seen at Musselburgh. Mr. R. politely allowed me to treat the case. I accordingly applied boiling water to the chest, which made him start, but it did not make the blood flow from the orifice Mr. R. had made in the median vein of the left arm. I reapplied boiling water, tied up the right arm, and after a renewal of the hot water to the heart and chest, opened the vein of the right arm; the blood flowed thick, and nearly as black as ink, but stopped; I then steamed the arm: the blood now continued to flow till the *pulse became full*, and the spasms decidedly better. I next gave him six powders of the subsulphate of mercury†, each containing ten grains. As in half an hour they only caused a slight effort to vomit, I gave him six more, and in about twenty minutes he vomited a small teacupful of a brownish thin fluid.

I abstracted, as nearly as I can guess, between forty and fifty ounces of blood, and the effects during the flow of blood, were as follows. The pulse, when two soup-platesful had been drawn, became fuller, and of common quickness, *the spasms were greatly relieved*, and the blood became lighter in colour: when I had abstracted two more soup-plates nearly full, (though partly mixed with water) the pulse was quicker, about 90, and not quite so full, but so far from any symptoms of exhaustion taking place, he revived astonishingly, and the spasms entirely ceased. The jaws could now be opened with tolerable ease, and the colour of the blood was natural, of which four or five more ounces trickled away, which seemed to do him good. The blood flowed at intervals for nearly a quarter of an hour. Two hours

* Mr. Rutherford is a surgeon at Musselburgh: many of the *worst* cases occurred in his district.

† Owing to the tetanic affection of the jaws, I was obliged to use great force in giving the powders, so that a great portion of each powder was forced back by the patient, or lost in the bed. I do not think more than a third of each powder was swallowed: in all he did not take more than two scruples.

after the bleeding the pulse was 86, full, and quite perceptible in anterior and posterior tibial arteries, and in those of the superior extremities. I got him to swallow about ten grains of calomel, and applied heated sand and salt all over the body, excepting to the feet, which were heated with bottles, as most convenient. At 4 P.M. the heat of the whole body was restored, and the pulsations in all the arteries were quite perceptible and full, about 80. At this time he first spoke to his father. Has vomited once. At half-past 5 had two slight spasms; pulse smaller, but not quicker. An injection of turpentine and gruel was given at 6 o'clock, and at this time he constantly asked for a drink of small beer. As he was very importunate I gave him a little, warm, but he vomited it up directly. At a little before 7 he was colder, and his pulse was quicker. Many of his attendants had left him, thinking him much better, but at 7 he was seized with a spasm; he turned up his eyes, drew up his legs, extended the left arm, and died as dreadfully rigid as when I first saw him, thus blasting all my efforts, and the fair prospects of his relations. I did not leave this boy for more than five minutes during the time I attended, and only then to bleed other patients. It is my belief a worse case than this has never occurred in any country. I think from the treatment of it we may draw the following conclusions:—1st. That bleeding will restore animation when apparently gone. 2dly, That it will smooth the path of death. 3dly. That it will prolong life even in the most extreme cases. 4thly, that in using bleeding we should not be guided by the quantity drawn, but by the effects. 5thly. That the subsulphate of mercury was, and will be, of little use, even in large doses, in exciting reaction, by causing vomiting in this and other similar cases, where the powers of life are all but extinct; but that in cases less severe, when the pulse is to be easily felt, as in Robinson's case, and where there is no spasmodic affection of the jaws, it may be useful combined with bleeding.

CASE II.—Margaret Robinson, ætat. 28, a stout, pale fish-wife, has been complaining for two days of her bowels, and of inappetency; was taken ill at 3 o'clock this morning, January 26th, with sickness, succeeded by vomiting

and purging of a thin brownish fluid, very offensive. At 11 P.M., eight hours after her seizure, she was labouring under the following symptoms: pulse 90, feeble, of radial artery of left side, perceptible in brachial, temporal, and subclavian, imperceptible in anterior and posterior tibial arteries. Lips blue and cold; tongue not cold, though I think diminished in temperature; skin of chest, nails, and hands, decidedly blueish, and very cold. Look expressive of great anxiety. She moaned constantly, and, when this was observed to be increased, spasms of legs, fingers, and arms, followed; and she complained of violent pain at the heart, shooting to the spine. There was constant jactitation, and extreme restlessness. Breathing during inspiration much oppressed; answers questions coherently. Has passed a small quantity of urine. At this time there was no vomiting or purging, but there had been some an hour previously. This case was treated with Calomel gr. ij. Opii gr. ss. tertia quaq. horâ; mustard poultice, and I believe brandy and water; and she was seen every three hours. She died at one o'clock, ten hours after her attack. I did not interfere in the treatment of this case.

Remarks.—The disease varies considerably according to the age of the person attacked. It is most appalling in the young, most characteristic in the middle-aged, and most ghastly in the old. Females are oftener attacked than males. There is a decided appearance in the countenance and colour of the skin, which is diagnostic, and when once seen, can never be forgotten. In fifteen patients I have already seen, most of whom I have conversed with and treated, the symptoms were as follows:—In the young, spasms most intense, sometimes preceded by diarrhœa; hands not so blue as in the middle-aged; lips blue; great anxiety, and jactitation, and moaning, which increases before the spasms. Sometimes tetanus; expression most anxious; eyes everted. In the middle-aged, the spasms are not so intense; the hands and lips bluer; there is more anxiety; shooting pains in heart and chest; jactitation, but seldom trismus; the moaning is almost constant; and the look most anxious. In the old, (those past 60,) the hands are blue, nearly black, incurvated like a bird's claw, and shrivelled; the pulsations are not to be felt in any of the arteries, with the ex-

ception of the left brachial, which sometimes has a thrilling movement: they lie with their mouths slightly opened, but seldom moan, and their appearance has pertinently been compared to a corpse which has been a long time on the dissecting table.

There are three stages in the disease (the first, however, is sometimes absent in extreme cases, and in old people), viz. the irritable, the congestive, and the febrile. In the irritable the patient, having been exposed to cold and contagion, is seized with an icy chilliness, peculiar pain and fluttering at the heart; the pulse, if natural and full, becomes thready and quick; he is unable to sleep from extreme restlessness, and has transient pricking pains in legs, toes, fingers, and arms, with slight twitchings. These symptoms, if not checked, continue for twenty-four hours or more, and pass into the more aggravated form of the congested, or blue, or second stage, such as I have described in the cases drawn from nature*, and which passes sometimes into the febrile, or third stage, possessing all the characters of congestive fever generally in its worst form.

The treatment I think should vary according to the stage of the disease. In the irritable, brandy and hot water; mustard emetics, if the symptoms do not subside, and after their operation Calomel gr. v. to x. and ʒj. of carbonate of Soda in warm water. In the congestive, bleeding till the blood gets lighter or symptoms appear relieved,—repeating it if spasms recur; turpeth mineral for mustard emetics, and calomel after their operation; heated sand put in large flat bags, and applied by flannel bandages to the spine, chest, and bowels; with drink of soda water, or carbonate of soda. In the febrile, the treatment I have recommended in my last letter is the best.

The disease is *undoubtedly infectious*.

I have seen three cases in Edinburgh: two dead, one remaining. No new cases this day, January 30th.

If you are not supersaturated with cholera, perhaps you will have the

goodness to insert these cases, with remarks, in your next Gazette.

I remain, sir,
Your obedient servant,
JOHN KNAPP, M.D.

P.S. The cases in Musselburgh have been the worst that have occurred in Great Britain: gentlemen who have been familiar with the disease abroad have never seen it more malignant.

GRATUITOUS ATTENDANCE IN CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

By one of your late numbers, I am glad to observe that the members of the medical profession are, at last, aroused to complain of the injustice and unreasonableness of the public in demanding "*gratuitous attendance*" in cases of cholera occurring amongst the poor, during the existence of the disease, *as a pestilence*, in this country. This opinion, although for many and obvious reasons, one, that individuals feel a delicacy and reluctance to express, nevertheless prevails very generally amongst medical men, and deserves, at all events, a certain degree of consideration. As an act of justice, therefore, to the profession, I sincerely hope you will admit another appeal to reach the public ear through the medium of your liberal and impartial journal.

Although highly commendable in a political and humane point of view, both on the part of government and the country generally, to institute every possible means of prevention, as well as succour against the day of need, still, in so doing, justice and liberality should, as far as possible, distinguish the providence and forethought which *fear*, perhaps as much as *charity* and *humanity*, have hitherto been so much on the alert to suggest. However zealous a man may be in the pursuit of knowledge, or anxious to succeed in his professional career—and none display these feelings more energetically than medical men—the lives of most of us, though often unnecessarily exposed by ourselves, are not exactly public property, and are, at all events, of moment

* I have been most careful to describe the disease from nature in all its stages; indeed, I have never read the works of Orton, Bell, or Annesley—not that I disregard them, for I believe they are excellent—but because I was determined to copy from nature. *I have had the first stage.*

to our families and friends. If suddenly cut off in the discharge of those duties which philanthropy would prompt, should cholera visit this metropolis, would the survivors, though indirectly deriving the most invaluable protection by our exertions, step forward and provide for the widow and the orphan, unexpectedly bereft of a husband and father, because he fell a sacrifice to his benevolence? To expect the *gratuitous* services of a professional man in behalf of hospitals, dispensaries, workhouses, and similar charities, is a very different case from that under consideration. His attachment to those institutions does not, under ordinary circumstances, expose him to any particular risk; experience is afforded him; and in the course of time, if he merit success, honour, fame, and prosperity, seldom fail to reward his industry. But even in these cases I am of opinion that the public often demand more than should be conceded, and take undue advantage of an honourable and laudable competition.

I am really surprised that not a single parish, amongst the hundreds that have considered the subject, should have proposed any recompence to those men whom they have severally appointed *to expose the lives of themselves and families*, for the general good*! Let the public evince an example of that disinterestedness demanded of medical men, by refusing to accept a boon offered at so great a risk. It is unworthy a liberal community to take advantage of the laudable competition and generous spirit which animates, and I sincerely hope will continue to animate, the breast of every member of the profession to which I belong. A physician's advice and time are his *livelihood*, and what other man, for the benefit of the public in general, though he volunteers his *time*, will give also his means of subsistence? Many things, besides medical superintendence, are requisite, at the present moment, to succour the poor. Flannel, and other articles of warm clothing, good diet, wine, brandy, drugs, and the like. Will, then, the wine-merchant, the spirit-dealer, the woollen-draper, the butcher, the baker, the druggist, come forward and supply, "*ad libitum*," for the general good, the various articles they

severally gain their livelihood by selling? They, individually, run no risk; they will not be exposed to contagion; they will run no chance of carrying cholera into the bosoms of their own families, or of injuring their business by carrying it into the houses of their customers. Such generosity would be charitable and humane, and equally in the way of their duty, as it is in that of the physician to give advice, and superintend the administration of the means they would thus supply. These persons, however, though ready enough to be elected members of the Board of Health in the parish to which they belong, or to offer any services that do not immediately interfere with their interests, and perhaps willing to subscribe a guinea or two, to aid in arresting the progress of the disease, which, if neglected, would in all probability extend to their own doors, tell you they cannot afford to give away articles by the sale of which they live—that these constitute their stock in trade. The truth is, in plain language, keep the disease from their doors, and the poor may die of cholera, or any thing else, for ought that many of them care. The higher and more noble feeling which prompts medical men to offer their services on the present and on every occasion, viz. a zealous wish to promote the interests of the science they profess, is a sentiment unknown to, and wholly unappreciated by the public; but on this I have no intention to enlarge, my object being to point out how unreasonable and inconsiderate it is to expect one class of men to grant in the way of their profession what no other, professional or otherwise, would give. Will the clergyman bury *gratuitously* all who die of cholera? Will the lawyer, on the score of humanity, *and without fee or reward*, settle the worldly affairs of all who fall victims of this disease? or the undertaker supply the means of carrying them to their graves, *without receiving his usual emoluments*? The universal reply is, No. Why, then, I would ask, should the physician give his time, and expose himself in behalf of his parish, more than any other member of the community? The activity looked for on the part of the profession at the present moment in behalf of the *poor*, is the protection, nay, salvation of the *rich*: those who can afford it, then, ought to pay for the advantage thus in-

* This is not quite correct, but the general position certainly is so.—ED. GAZ.

directly derived. Let each parish levy a tax on householders, for the purpose of remunerating those men whom they appoint to risk their lives for the benefit of the rest; and let the remuneration be such that the appointed may attend to cases of cholera alone, lest the disease be carried from the hovel of the pauper to the palace of the prince. Men, already established in general practice, may not perhaps be found willing to enter on the duty on such terms, and young men, who have had no practice, are not the persons to be entrusted with so momentous a charge. Let government, then, supply from the half-pay list of army and navy surgeons, men who are familiar with the disease, as it has appeared in foreign climes, and experienced in its treatment, and let the parishes individually, by the tax I propose, increase the scanty pay of government, and act with liberality towards those who are willing to serve their country in the hour of need. The case with the public, like the pestilence itself, is a new one, and a new remedy should be devised to meet the exigency.

One word more, Mr. Editor: it is a word to those who use the argument, "that there is a mighty difference between the tradesman contributing goods and the physician advice," on the fallacious supposition that the latter *costs its possessor nothing!* Now, sir, let me assure those who take this view of the matter, that they never were more mistaken in all their lives. Advice and time are the physician's stock in trade; and if they consider the sums expended during his studies, especially if he have been educated liberally, and travelled for information, as well as the unrequited outlay of capital during the first years of his professional career, the disagreeable duties he has had to perform, the midnight oil he has consumed in research and study, they will find that the power to give advice, and the capability of professionally directing others, have been purchased at a high price, and that no inconsiderable sum must be received annually to repay the simple interest of the capital thus expended. Although a man may dispense his drugs, and have thus what the tradesman considers a "stock," yet I presume it is the instructions given to the patient how to employ drugs that render them of value. What would it avail a sick man to turn him into a chemist's

laboratory, and tell him to help himself without directing his choice, judiciously combining the various articles suited to his case, and apportioning the dose of the compound. The chances are, that he would kill, and not cure himself. In conclusion, I must observe, that the foregoing remarks are not the dictates of illiberal feeling, or intended to apply to medical, or other charities in general, but only to the case under consideration.—I remain,

Your obedient servant,
ONE DESIROUS OF TELL-
ING THE TRUTH.

MEDICAL GAZETTE.

Saturday, February 4, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

THE POPULATION RETURNS OF 1831*.

It will be readily understood why we take up this subject,—one possessed of the liveliest interest to all who delight to inquire into the resources of the land we live in. With population, however, as the great source of wealth and power to the country, we shall have as little as possible to do: we mean simply to offer a few remarks suggested by the professional view which may be taken of the documents before us. Here are facts which may supply a foundation for many valuable inferences,—materials to furnish answers to some of the most important inquiries. Mr. Rickman is entitled to great praise for the uncommon labour, ingenuity, and prompti-

* "The Population Returns of 1831; with a statement of progress in the inquiry regarding the occupation of families and persons, and the duration of life; an historical and descriptive account of London, Westminster, and other parts of the metropolis; a table of Mortality calculated upon the ages of 93,333 persons buried in the county of Essex during eighteen years, 1813–1830; and a summary of the Population of Great Britain in 1801, 1811, 1821, and 1831. With an Appendix, Maps, and Plans.—Moxon, 1832."

tude, with which he has brought out this volume of returns—so well digested in its contents, and so available in its form and bulk,—and for thus putting it in the power of all the better-informed members of the community to satisfy themselves as to the actual amount and most probable value of human life among us,—a species of inquiry, to the practical results of which we cannot pretend to assign limits.

Statistical researches are of a comparatively modern origin; and it is worth observing, with reference to the use we are now proceeding to make of those before us, that they had their beginning in dry remarks on the public health, and were derived from the humble source of the “Bills of Mortality.” From such a source has statistics grown up into a science: nor is it confined any longer to its original objects; it traces from the simplest principles the gradual progress of *several* branches of knowledge, describes their actual condition, and points out their mutual relations to each other, the success which they have attained, and the deficiencies which remain to be supplied. Its application to the objects of government has created political economy; and it is more than probable that a careful cultivation of it, in reference to the natural history of man, both in health and disease, would materially assist the completion of a genuine philosophy of medicine*.

But short as is the time this science has existed, it has afforded us the means of ascertaining much that is satisfactory and encouraging. Until within the last thirty years, there were no population returns that could be depended upon. Let us see what has taken place since then. From the summary given us by Mr. Rickman, we learn that, in 1801, the population of Great Britain was 10,942,646; in 1811, it was 12,609,864;

in 1821, it amounted to 14,391,631; and it has now attained, by the last census, the large amount of 16,537,398, an increase of considerably more than two millions within the last ten years, and much above five and a half millions within the last thirty!

Now population, it may be observed, is increased by all those causes which tend, first, to render the people more prolific—the preservative powers, or the means of preserving their offspring, remaining the same; or, secondly, by whatever tends to preserve the lives of offspring—the prolific power continuing the same. We believe it will be generally admitted, that people are not less prolific now than they have been in preceding ages; the powers of the human race, in this respect, having undergone no greater mutation, probably, than their stature;—and even allowing that poverty may have become more prevalent with the enlargement of the numbers of mankind, yet facts do not shew that this is any check upon their generative properties, whatever it may be upon their means of preserving their offspring. Great changes, however, have been effected in this latter particular—we mean the economy of the preservation of the species. Not to go back to any very distant times among ourselves, or to refer to what occurs among savage nations, it was no uncommon thing, as Adam Smith informs us, in the middle of the last century, for a hardy Highland woman to give birth to twenty children in the course of her childbearing, and not bring two of them into the world alive. “In some places,” continues the same writer, “one half the children die before they are four years of age, in many places before they are seven, and in almost all places before they are nine or ten. This great mortality, however, will everywhere be found chiefly among the children of the common people,

* See Dr. Bisset Hawkins's Medical Statistics.

who cannot afford to tend them with the same care as those of better station. Though their marriages are generally more fruitful than those of people of fashion, a smaller proportion of their children arrive at maturity. In founding hospitals, and among the children brought up by parish charities, the mortality is still greater than among those of the common people." And while population was thus checked among the lower orders, in Dr. Smith's time, it was also much limited, it would appear, through pampering among the higher. "A half-starved Highland woman frequently bears more than twenty children, while a pampered fine lady is often incapable of bearing any, and is generally exhausted by two or three. Luxury in the fair sex, while it inflames, perhaps, the passion for enjoyment, seems always to weaken, and frequently to destroy altogether, the powers of generation*."

But when we look back upon what has happened within the last thirty years, and find that the increase of our population has, within that period, amounted to above 50 per cent., we cannot help admitting that some very great changes have been effected in the habits and resources of our people; and facts will be found to bear out the admission. We believe we may safely venture to assert that improvements have taken place in the habits of *all* classes; the upper and middling are become more abstemious, and the lower orders generally more temperate in the use of spirituous liquors. The almost universal use which (maugre the denouncements of Cobbett) the article of *tea* has obtained, is, perhaps, one of the greatest blessings, both to the rich and poor, that was ever conferred on the nation—not even the potato excepted: the habitual use of the beverage has contributed, in

a high degree, to the health and comfort of every class in the community. We have certainly heard that a supposed increase in the number of insane persons has been attributed to the use of tea: but the visionaries who entertain such fancies would do well, before they propagate them, to inquire whether madness is a prevailing disease among the Chinese, who may be said "to eat their tea, drink their tea, and sleep on their tea." The potato, too, was for a long time held to be an unwholesome and poisonous root; yet it is pretty certain that a very large proportion of the healthy people of Ireland subsist almost entirely upon it alone.

Habits of comfort and cleanliness have also co-operated much in this striking progressive increase; and the almost universal and plentiful use of flannel and cotton next the skin, has contributed in no small degree to the preserving of health and the adding to our length of days.

In this brief enumeration, however, of the causes of increase in the value and duration of human life, nobody, we hope, will imagine that we mean to forget those which are immediately connected with our own vocation. The improvements that have taken place in the treatment of diseases, and the perfection to which surgery has been carried, it is well known, have most materially abridged the usual mortality. Vaccination alone saves thousands annually from an early grave; and would, no doubt, ere this, have exterminated one of the most destructive diseases that afflict mankind, if prejudice and envy, or interested and other unworthy motives, did not shed their malignant influence, and keep in deplorable activity the scourge of the small-pox.

In the management of fevers extraordinary things have been done. A mortality from this source, which amounted to a frightful sum not many

* Smith's Wealth of Nations.

years ago, is now reduced, in most parts of the kingdom, to about one in twelve. Jail fever, which was once so common and fatal, should it now ever happen to shew itself, is immediately subdued; and what with the palpable benefits—at last well understood—of ventilation, fumigating, white-washing, disinfecting, and the superior skill evinced in the regulation of our hospitals, prisons, workhouses, and fleets, a signal triumph has been achieved by the influence of medical principles over the ravages of pestilence. The recent strikingly progressive increase of population, too, be it observed, has been contemporaneous with the improvements in medicine to which we have already alluded,—so much for those who would indulge in the paradox, that there is nothing new in medical science, and that it rather retrogrades than advances.

In the digest of the Parliamentary population returns of 1821, Mr. Rickman calculated the diminishing annual mortality of Great Britain to be as follows:—

In 1780, one died out of every 40 of the population.	
1790	45
1800	47
1810	49 or 50.

In his present work there is some variation, chiefly on the side of excess, in the account of those decennary periods; but it is to be recollected that, until within the last thirty years, not much accuracy in statistical returns of this sort could be attained. “The increased duration of life in England is a phenomenon which has attracted more notice abroad than at home, but seems now to be generally admitted by those who have studied the subject with the best means of arriving at a well-founded conviction. From the parish register returns of the decennary years of the greater part of the last century (1700—

1780), which vary from one death in 31, to one death in 42, is deduced an average rate of mortality, *i. e.* one death in 37, or 38, of the existing population. This becomes one in 45 in the year 1790; one in 48 in 1800; one in 54 in 1810; and one in 60 in England and Wales in the ten years preceding 1820 *.”

Busching, the geographer, calculates the average mortality of all countries (taking towns and villages together) to be from one in 32, to one in 37. About eighty years have since elapsed, and a surprising improvement in the physical condition of man has progressively developed itself. In almost every civilized country of Europe we find the annual proportion of deaths considerably diminished, and continuing to diminish, relatively to the particular circumstances in which each country is placed; and in Britain the value of life is nearly doubled, if we compare Busching’s rate of one in 32, with the rate afforded by the census of 1821, of about one in 60.

Further, according to the conclusions of Sir Gilbert Blane, Mr. Rickman, Mr. Milne, Mr. Finkelson, and Dr. Bisset Hawkins, in England; Dr. Villerme, in France; and Dr. Casper, in Germany; the mean duration of human life has been everywhere improved throughout Europe within the last century. In this country, it is calculated, that the mean duration to the middle classes, which appears to have been no more than about 37 years, at the close of the seventeenth century, rose to 52, at the expiration of the eighteenth; and it may be fully credited that a change equally encouraging in the health and duration of life of our entire population has taken place.

Such are a few of the facts which, upon touching this subject, it seemed worth while to lay before our readers. Mr. Rickman’s new volume, however,

* Population Returns of 1831, p. 28.

contains many important details, which we cannot prevail on ourselves to conclude without glancing at. His statistical history of the metropolis is highly interesting. Among other pieces of information which we find in it relative to the component parts of the Babylon, after describing London within-the-walls, he says:—

“In the beginning of the last century the population (of this part of the city) was not much less than 140,000, as proved by deduction from the parish registers; and the annual mortality was as *one to twenty* of that population. Fortunately for the health of the citizens, space is become more valuable for warehouses than for human habitation, so that the population within the walls is diminished to 55,778, and the rate of mortality to *one in forty*.”

One in forty is certainly a very gratifyingly low average, scarcely exceeded by that of any other city in Europe*. The mortality of Paris is about 1 in 32; of Naples, 1 in 28; Vienna, 1 in 26; Rome, 1 in 25; and Amsterdam, 1 in 24†.

The account of the Bills of Mortality is worth extracting:—

“The Bills of Mortality, from which the fifth division of the metropolis is designated, require some explanation. London used always to suffer heavily from the plague, and in the great pestilence which, originating in the East in 1345, reached England in 1348, it seems well established that 100,000 persons died and were buried in the city. In 1563 above 20,000 persons died of the plague; in 1592 above 15,000; and in 1603 more than 36,000. This frequent recurrence caused the establishment of notices, called Weekly Bills of Mortality, which were kept and published by the parish-clerks, as a warning to the court, and to others to leave London, whenever the plague became more fatal than usual. In the year 1625, above 35,000 persons died of the plague; in the year 1636 above 10,000; and 68,596 persons

died in the last great plague of 1665. The conflagration which destroyed the whole city occurred in 1666, after which the plague languished, and finally disappears from the Bills of Mortality in 1672. The somewhat obsolete names of diseases in these bills have injured their reputation, and in some of the large parishes they are discontinued. The population of this division of the metropolis was 326,000 at the beginning of the last century: it now amounts to 760,000.”

These are particulars which it would be useful sometimes to remember, especially when we see “the Bills of Mortality” about the end of the year, with all their stupid blundering and inaccuracies, running the round of the newspapers. How much importance is to be attached to the said bills is at once evident from the fact, that “in some of the large parishes they are discontinued;” that they only relate at best to less than half the whole population of the metropolis, and that in the metropolis generally, with all its reputed care of parish registers, about 8000 burials are deficient annually*.

There are some curious particulars in the Statement, relative to the mode in which the returns before us have been made; but these we are obliged to pass over, as well as the large portion of the volume which is composed of tables and details of the most obvious and vital interest to the politician. For us, be it enough to have made the preceding remarks on the general summary of information which the volume contains. In the teeth of Malthus and his inhumanities, we rejoice in the authentic record which we here possess of the enlarged resources of the country: we fear no plethora from a superabundant population.

* Population Returns, page 28.

* Glasgow is said to have a mortality of but 1 in 45.

† See Dr. Bisset Hawkins's Med. Stat.

CHOLERA IN NORTH BRITAIN.

WE this day present our readers with some very important papers on the subject of cholera, chiefly by gentlemen who have witnessed and treated it in this country. One of these communications is from the pen of a correspondent in Edinburgh, and we cannot suffer the opportunity to pass without expressing our admiration of the means adopted in that city to mitigate the severity of the visitation. A Board of Health has been formed, comprising the names of many of the most influential gentlemen of the place, and with such a host of talent in its medical department as must command the fullest confidence of the public. Five hospitals have been prepared in different districts of the town for the accommodation of the poor, and we have reason to hope that scientific and well-directed efforts will now be made to put to the test of clinical experience the various methods which have been proposed—particularly galvanism—different kinds of emetics—neutral salts—and others, with regard to which the accounts at present are so contradictory that it is impossible to draw any satisfactory inference*. From the correspondence which we have established, we hope to be able to continue to supply our readers with the earliest and most important information on the subject. We understand that in Edinburgh six stations have been opened for the supply of provisions to the indigent, and that at present 5800 quarts of good soup, and 6500 rations of bread are daily distributed. This is, indeed, setting about the thing in the right way, and the northern metropolis has shewn an example which we should be glad to see imitated elsewhere.

* We observe it stated in one of the Edinburgh papers that galvanism has been successfully employed in Scotland.

ANATOMY BILL.

To the Editor of the London Medical Gazette.

SIR,

You will oblige me by inserting the enclosed in the Gazette next Saturday. I am not hostile to the bill, but to the *money-traffic*; and shall be happy to state my arguments more at length in your next number, if you will give me leave.

Your obedient servant,
B. TRAVERS.

Bruton-Street, Feb. 2, 1832.

*Observations respectfully addressed to
Members of Parliament.*

The Anatomy Bill, now before the House of Commons, is so objectionable in some material points, that a more particular consideration of its operation and consequences, should it pass into a legal enactment, is earnestly to be desired.

The source of supply is so easy and ample, that although fenced by strong restraints in the shape of penalties for the violation of its conditions, on the part of buyer and seller, it will still act as a stimulus to the worst passions of men; and not only encourage a repetition of the nefarious crimes which have been lately brought to light, but will assist to screen them from detection. A man who would commit murder for *ten* pounds, would not shrink, under urgent circumstances, to do it for *two*. The absence of the motive is the only security against the propensity, and the motive is the most powerful of all—the love of money.

In every instance (and it is to be feared there are many) in which a lodging-house-keeper has a poor and friendless inmate, the body of that inmate may become a security against loss, and an incentive to murder.

Nor could the licensing of anatomical schools, nor hedging round the license with legal penalties, much less leaving the violation of the law to the exclusive cognizance of the Attorney-General, put an end to the practice of burking, so long as the transfer of bodies is a matter of legal traffic by sale and purchase.

The desiderata for the common interests and protection of the public and

the medical profession, and they are inseparable, are—

I. The legalization of anatomy.

II. The licensing of schools of anatomy, and the prohibition to practice anatomy, except in the licensed schools, under a heavy penalty. Excepting always such examinations to ascertain the appearances after death, as are practised by permission or desire of relatives, in the house in which the deceased died.

III. The official registration of the licenses.

IV. The official supervision of the schools by inspectors duly authorized.

V. To permit resident owners and masters of houses, governors or superintendants of institutions, public and private, of every denomination, to deliver up, for anatomical purposes, the dead bodies of persons, their lodgers or inmates, who have died without friends, or whose relations consent to such appropriation, the contrary not having been expressed by such persons before death, to a licensed anatomical school, through the medium and with the approbation of the inspector, provided always, a certificate of the illness and cause of death by a duly authorized medical practitioner be delivered with the body.

VI. To require that the certificate delivered with each body should be registered by the teacher of the school to which such body is conveyed; and that the register be always open to the licensing authorities.

VII. To provide a salary for the persons appointed to inspect and superintend the transfer of dead bodies, out of a fund arising from the supply of the schools.

VIII. To make it a misdemeanor, punishable by fine or imprisonment at the suit of the party aggrieved, for any person to obtain money in or by the aforesaid transfer, or by the sale of bodies.

The bill is good, inasmuch as it suggests the source of supply above-mentioned; the delivery of certificates, and the appointment of inspectors; and as it repeals the offensive penal statute condemning the bodies of murderers to dissection; but the removal of this need not be accompanied by the infliction of others, even more odious. No punishment can reach the dead, nor does any enactment of this description

produce any salutary influence upon the living. But that should not be made by association odious, which the laws of humanity render indispensable—viz. the dissection of the dead human body; therefore the substitution of other posthumous penalties is objectionable, not to say disgraceful, in an age and country emerged from a state of barbarism.

I think it may fairly be questioned, whether the Secretary of State for the Home Department is the proper person in whom to vest the power of granting licenses for schools of anatomy, and appointing inspectors for their regulation.

It is with the deepest conviction of the importance of the bill, and the most earnest desire for its speedy and full success in the prevention of crime and the promotion of science, that I offer at such a moment these observations. But as it would be more consistent with a sense of moral duty that ten should die for want of anatomical knowledge in the surgeon, than that one should be murdered for the purpose of obtaining that knowledge, let the temptation to assassination, now unhappily discovered to be equally easy of perpetration and concealment, be removed—first, by rendering bodies an illegal object of traffic, and then by opening the abundant sources of supply above-mentioned. But while they can be bought and sold, the fluctuations of a market, perfectly undefined by license or qualification, will furnish a continual inducement to burking.

It would, in my opinion, be injurious to the interests of the medical profession, as well as to those of the public at large, that anatomy should be practised except in licensed schools, or that licenses should be granted without due examination of the claims of applicants by competent authorities.

BENJAMIN TRAVERS, F.R.S.

Surgeon to St. Thomas's Hospital.

Bruton-Street, Jan. 30, 1832.

MEDICO-CHIRURGICAL SOCIETY.

Tuesday, Jan. 24, 1832.

MR. LAWRENCE IN THE CHAIR.

Pathology of the Glands and Spleen—Styptical Experiments.

THE Secretary proceeded to read the remainder of Dr. Hodgkin's paper on certain appearances of the glands and spleen:—

Another morbid appearance (according to Dr. Hodgkin) is sometimes presented by the spleen; it is partially indurated, and suffers a loss of colour: its bulk is diminished, and it bears, at first sight, a good deal of the appearance of tubercle. This change appears to begin with a sort of partial apoplexy of the organ, and when the effusion of blood (whether spontaneous or from injury) is recent, the part affected is distinguished from the sound part by its deep venous hue. After a time it becomes brownish, and, in short, the change is precisely similar to that which takes place in apoplexy of the lungs, and even in cerebral apoplexy: the portion of the spleen thus altered may yet retain its vitality, and merely a thin semi-transparent boundary may seem to separate the altered from the healthy structure of the organ. Although the derangement in question may remain stationary for an indefinite length of time, yet it would seem to predispose the adjoining parts for changes arising either from external force or unusual detention.—Mary Hamblin was admitted into Guy's Hospital, in a state of mania; she was the mother of twins, whom she had suckled for sixteen months, and being a spare and delicate woman, was greatly reduced thereby. For some time before her admission she complained of her head, and was apprehensive of insanity: restraint, approaching to violence, had apparently been employed. Whilst in the Hospital she generally lay in a state of insensibility, but was occasionally delirious. She died in six days after her admission.—*Inspection.* The brain presented nothing remarkable; the contents of the chest were healthy; the liver seemed healthy in form and texture, but its convex surface was mottled with spots of a yellowish colour; the spleen was of its natural size, and of a healthy appearance, with the exception of some spots of a darkish venous hue, the central parts of which were of somewhat a lighter colour. On cutting into the spleen those spots were found to consist of portions of the organ indurated by coagulated venous blood. It appeared probable that these changes, both in liver and spleen, were the effects produced by the means employed for restraint during her insanity.—Daniel Patrick, a patient, whose case is on record in Guy's Hospital, presented a spleen rather large, and at first appearing to be tuberculous, but upon closer examination, proving to be merely dependant on peculiar alteration of structure. Several other cases were then adduced by Dr. Hodgkin, the particulars of which our space will not permit us to detail; in one of them, that of Maria Lowther, the derangement in the structure of the spleen in the neighbourhood of the indurated portion, amounted to complete softening, accompanied by a softening in the central part of the degenerated spot; and Dr. Hodgkin

was of opinion that this state of the spleen was the exciting cause of a severe peritonitis, under which the patient laboured. Some remarkable symptoms which attended the same case, and seemed to indicate the presence of phlebitis, evidently depended on the same gangrenous softening of the spleen in contact with the indurated part. External injury probably gave rise to the original derangement, and a repetition of the same cause might have promoted the secondary affection, by injury to the surface of the indurated part. It is possible that slight external violence may have been adequate to this effect; it may, however, be made a matter of question, whether intermittent fever had not existed here; in the cold stage of which, the spleen being gorged, may have sustained a partial laceration.

Mr. LANGSTAFF was rather surprised that no notice was taken of the state of the liver in those cases of diseased spleen. He (Mr. L.) never saw an instance of the one organ being diseased without the other being so too. In scrofulous habits tubercles were generally to be found in both.

Dr. HODGKIN denied that he had overlooked the liver in his examinations and details; but it was his impression that the appearances he described were the results of accidental causes, and not properly connected with constitutional injuries. In one or two of his cases, the liver had some small tubercles in it; but he attached no particular consequence to that fact, and in general he recollected that the liver was *not* apparently diseased in the cases which he had laid before the Society.

The PRESIDENT now called the attention of the meeting to a paper, by Mr. Casar Hawkins, on some experiments recently performed with a view to the arresting of hæmorrhage. The paper was accordingly read by the Secretary.

Account of some Experiments on the Use of Styptics in Hæmorrhage from Arteries.

The experiments of MM. Falrich and Halmagrand, which were said to have been invariably successful in Paris, have proved to be less uniformly so in this country. Out of five, three had proved fatal; and in the other two, the animals were killed before the time at which secondary hæmorrhage might have taken place had expired. Most English surgeons are acquainted with Dr. Jones's method. In 1805, this gentleman minutely described the coagula, which are principally instrumental as a barrier against loss of blood: he shewed also the manner in which contraction of the artery added to the security of the animal, and the circumstances under which retraction at one time checks the hæmorrhage, and at another increases it, according to the kind of wound which is made in the artery; he also explained how

a wounded artery is sometimes obliterated by the operation, and again the wound is completely cicatrised without interfering with the current of the blood. All these circumstances, however, have been recently described, as if for the first time, in a French periodical, and the effects attributed solely to the "new styptic:" but Mr. Hawkins was induced to suspect that this remedy acted only a subordinate part in arresting the hæmorrhage. He accordingly undertook to demonstrate to some of those who had witnessed the experiments of MM. T. and H. the real principle to which the effect ought to be ascribed; and it was in the presence of the inventors of the styptic, and a considerable number of other spectators, that Mr. Hawkins proceeded to shew that, in most instances, he could arrest the hæmorrhage as well without the styptic as with it.

In Dr. Jones's experiments, the free passage of the blood from the wounded artery was prevented by ligatures in the skin, the blood forcing its way into the cellular texture, and there becoming coagulated. M. Halmagrand's mode is, to place three small compresses of cotton dipped in his "*liquide hæmostatique*" upon the wound of the artery, and retain them there by the fingers for ten minutes, after which a single ligature is drawn through the skin, merely to prevent the compresses from being thrown out of the wound.

Experiment 1.—The carotid artery of a sheep was wounded longitudinally to the extent of half an inch, and in the aperture were placed three small compresses dipped in decoction of galls. After holding them there with the fingers for ten minutes, a single ligature was put through the skin, and the fingers were withdrawn; the compresses were removed on the second day: it was on the tenth day that the sheep died, in consequence of another experiment on the opposite carotid. On examination, it was found that the wound made in this experiment, had become perfectly closed by lymph, without any coagulum to interrupt the passage of the blood.

Exp. 2.—The artery of another sheep was divided transversely to half its diameter. M. Halmagrand, who assisted at the operation, had so much difficulty in finding the vessel, that before Mr. Hawkins could press the artery against the vertebrae with his fingers, the animal lost a pint of blood, and fainted: when it revived, the artery began again to bleed; upon this, three compresses, moistened with Ruspini's styptic, were placed upon the wound. No hæmorrhage whatever occurred.

Exp. 3.—Mr. Hawkins cut out a circular portion of the carotid artery, and applied compresses dipped in *water only*; the animal was very restless, and coagulation was thus prevented. It was found necessary, therefore, to alter the experiment by uniting the skin with a continued suture; so coarsely,

however, that upon removing the fingers not only was the wound instantly filled with blood, but a good deal escaped also between the sutures; no farther hæmorrhage ensued.

Exp. 4.—The left carotid of the sheep used in Exp. 2 was laid bare, and a considerable portion of it removed. Compresses dipped in plain water were held on the aperture for ten minutes, and then a single ligature was made in the skin. Not a drop of blood flowed till next day, when from drinking out of a high pail, the compresses were disturbed; hæmorrhage ensued slowly, and the animal died on the third day. This experiment, however, as well as the following one, was performed under rather unfavourable circumstances, as the opposite carotid had been already operated on, leaving only the vertebral arteries to convey blood to the head; but the same circumstances, at the same time, render Experiments 1 and 2 still more striking.

Experiment 5.—A hook was placed under the left carotid of the sheep employed in Exp. 1; sutures were then passed through the skin; and the artery having been drawn up, a circular portion of it was removed, and the vessel suffered to recede. The sutures were then drawn, so as nearly, but not completely, to stop the bleeding. After a copious hæmorrhage for a few seconds, no more blood flowed until the third day, when it began to trickle from the wound: on the fourth day the animal died.

In all these experiments it appears that the flow of blood was arrested for a time; the shortest period at which secondary hæmorrhage took place being twenty hours. And out of three experiments performed after the manner of M. Halmagrand, though without using his "*Liquide*," two were permanently successful.

From these considerations Mr. Hawkins would infer, 1, That it is possible by pressure for ten minutes upon an artery, in a *nearly open wound*, with small compresses moistened in various liquids, to check hæmorrhage from an artery cut transversely or longitudinally, or from which a portion has been removed. Dr. Jones's experiments were made with *closed wounds*. 2. That a circular aperture made in an artery can be closed by allowing a diffused aneurism to take place in the soft parts around the artery, after Dr. Jones's manner. 3. That the pressure, whether from the compresses or the diffused aneurism, acts on the same principle—the formation of coagulum, and subsequently the deposition of lymph, uniting the cut edges of the wound, or joining the coagula to the sides of the vessel. 4. That if this coagulation take place imperfectly, or the coagula be disturbed, hæmorrhage will ensue. 5. That it is probable that styptics, whether applied directly, or taken internally, can only act in two ways—first, by inducing contraction of the coats of the

ROYAL INSTITUTION.

Friday, Jan. 20, 1832.

GEORGE MOORE, ESQ. F.S.A. VICE-PRES.
IN THE CHAIR.

bleeding artery; or, secondly, by promoting coagulation, which some styptics positively do—the decoction of galls, for instance, and M. Halmagrand's "liquide;" either of which, mixed with fresh blood, instantly coagulates it. Ruspini's styptic, however, and plain water, neither of which produces this effect, have yet the power, as appears from the preceding experiments, of arresting permanently or temporarily, the flow of blood, even from the worst kinds of transverse wound. The action of styptics would therefore seem to be only secondary to pressure in staying hæmorrhage. 6. That the effects of both pressure and styptics are precarious, even in brutes, and *à fortiori* should not be depended on in man, to the dispensing with the ligature. And, lastly, that while he (Mr. H.) deprecated the idea of styptics being relied upon in wounds of large arteries, he admitted that sometimes in the cases of smaller vessels they might be usefully employed as auxiliaries. In conclusion, he should have as much pleasure in acknowledging the fact, should further experience prove that the *Liquid Hæmostatique* of M. M. Talrich and Halmagrand was really superior to other styptics, as he always had in ascribing to their distinguished countryman, Paré, the revival of the still greater security against hæmorrhage afforded by the ligature.

Some discussion followed the reading of this interesting paper, in the course of which

Mr. LANGSTAFF found fault with the term contractility, employed by Mr. Hawkins, without the admission of the existence of that property in a muscular coat of the vessels.

Mr. HAWKINS did not conceive it at all necessary, on the present occasion, to enter into a discussion about the muscularity or non-muscularity of arteries: it was sufficient to know that these vessels possessed a fibrous texture, in which the contractile power resided. He referred to Mr. Guthrie's work on the arteries, for a striking example of this contractile power residing in the protruded part of the axillary artery, where the shoulder was torn off with a grape shot.

The PRESIDENT entirely concurred with Mr. Hawkins, in his view of the contractile power of wounded arteries; and he approved in especial the use of the term contractility, as not involving any hypothesis with regard to the structure by which that power was exercised.

A gentleman, whose name we did not learn, took the same view of the question, and, with reference to styptics, gave it as his opinion that pressure, by facilitating the formation of a clot, was the best remedy for hæmorrhage: he exposed the futility of Sir Charles Scudamore's alum styptics.

Mr. HAWKINS, before the close of the meeting, gave a summary account of the results of Dr. Jones's experiments.—Adjourned.

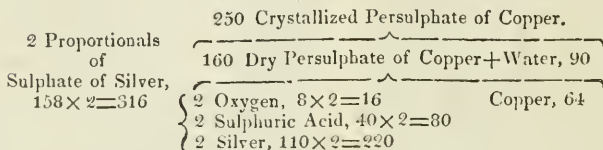
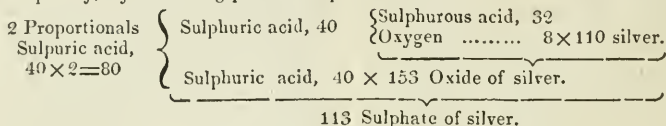
Mr. Brande on the refining of Gold and Silver.

THIS being the first meeting of the present session, we found a very numerous assemblage of the members and their friends, in the Library of the Institution, at an early hour; who, after examining various natural and mechanical curiosities, adjourned to the Theatre, where Professor Brande explained, in his usual perspicuous manner, the improvements which have been lately made in the art of refining gold and silver. This process, which has been introduced into our mint, has been, as it were, forced upon us in our own defence; for had we not resorted to it, all our gold and silver coin would have disappeared, and been returned to us in the form of bars.

It is well known that native metals are seldom pure, and even when reduced by ordinary means, some traces of admixture are easily to be found. Thus gold very frequently contains silver, and, on the contrary, silver gold, and so on; but if the gold in a silver ore be in too minute proportion to repay the cost of separation, it is left; and so likewise when silver occurs in gold, in lead, &c. Now the proportion of the precious metals which would repay the cost of separation has been much lessened of late; so that what formerly could not have been extracted without loss, may now be separated with profit: even so little as five grains, or even as two grains, of gold in the pound of silver, may be, under certain circumstances on the large scale, extracted with advantage; and thus the precious metals which formerly were sunk in the alloy are now replaced by others of less value, and our present gold coin contains no silver, and our silver coin no gold: hence it is that the sovereigns lately issued are of a deeper colour than those of a few years back, and of a very different hue to the older guineas. Indeed, so well is the difference and its cause understood, that it has become an object of trade, especially in France, where, if you pay any amount in gold, the merchant or shopkeeper will sort the paler from the yellower coins, considering the latter as currency, but reserving the former for sale at a premium; for the admixture of silver, on which the paler hue depends, will be extracted and replaced by copper, to render the whole standard gold. The possibility of this separation is nothing new; the profitable application of the knowledge is the only novelty, and this depends on the substitution of sulphuric for nitric acid in the process, and on the cheapness at which the former can now

be made. The mode of procedure is as follows.

Add to the gold from which the silver is to be extracted, two or three parts of silver (for without such addition the silver would be protected from the action of the acid), fuse, and granulate, by pouring the molten metal into water: then boil in sulphuric acid, and, during ebullition, oxygen from the sulphuric acid will convert the silver into an oxide, sulphurous acid in a gaseous form escaping, while the gold remains unaltered; the oxide of silver which is held in solution is subsequently, by immersing plates of cop-



MR. HUME's new forms of experiments on capillary attraction, almost made us think that there was less absurdity than we had been wont to believe in the nursery tale of catching water in a sieve; for he absolutely suspends a globe of water in a vessel of wire gauze. The principle is not new, but the application is ingenious; it depends upon the law that when the perforations in the gauze or sieve are so small that the attraction between the particles of the film of water between them is equal to the pressure of the atmosphere, then the film of water renders the sieve a close vessel, and this interior globe of water is contained within it.

MR. BROCKEDON's new pen is very ugly, but, like other things which do not boast external beauty, it appears endowed with many useful and excellent qualities. It would be in vain to attempt a description of it; and this is the less needed, as, in a few days, we prophecy it will be in the hands of almost every one. It differs from all other pens, in the slit being oblique instead of straight; by which means a very agreeable elasticity is given to steel, as well as to quills and other materials.

On Friday, January 27th, Mr. Faraday gave a very interesting account of "*Dr. Johnson's researches relative to the genus *Phanaria**;" but we are unavoidably obliged to postpone our report of it until next week, through want of room.

READS STOMACH-PUMP.

WE are requested by Mr. Read, of Regent-Circus, to correct a mistake in our notice of Jukes's syringe. It was stated that the latter had the priority of invention, whereas it appears that the reverse is the fact.

per, precipitated in a metallic form; it is pure, and only requires melting, and is technically known as water silver. The sulphate of copper, the refuse of this operation, is a merchantable article, and the sulphurous acid gas may be again converted into sulphuric; so that, if economically performed, nothing need be lost. The process for the separation of gold is a counterpart of that for the separation of silver, which may, perhaps, be rendered still more simple by two tables which were exhibited by the Professor.

METEOROLOGICAL JOURNAL,

Kept at EDMONTON, Latitude $51^{\circ} 37' 32''$ N.
Longitude $0^{\circ} 3' 51''$ W. of Greenwich.

Jan. 1832.	THERMOMETER.	BAROMETER.
26	from 25 to 42	from 29.80 to 29.77
27	25 36	29.82 29.97
28	22 35	30.06 30.15
29	32 48	30.11 30.26
30	32 45	30.23 30.13
31	35 43	29.98 29.81
February 1	32 45	29.49 29.25

Wind variable, S.W. prevailing.

Except the 26th and 28th, generally cloudy. On the morning of the 27th, from 8 till 10, snow fell, for the first time this season, nearly covering the ground, yet from the moisture caused by a little rain during the previous night, it entirely disappeared before 3 p.m.

Rain fallen, .375 of an inch.

CHARLES HENRY ADAMS.

BOOKS RECEIVED FOR REVIEW.

How is the Cholera propagated? By an American Physician.

Letters on the Cholera. By Whitelaw Ainslie, M.D. &c. &c.

The Anatomy of Drunkenness. By Robert Macnish. Fourth Edition.

NOTICE.

Dr. J. C. Ferguson's pamphlet has come to hand.

W. WILSON, Printer, 57, Skinner-Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, FEBRUARY 11, 1832.

LECTURES
ON
THE THEORY AND PRACTICE OF
MEDICINE;

Delivered at the London University,

By DR. ELLIOTSON.

PART I.—LECTURE XIX.

New Formations—continued.

I WAS speaking, gentlemen, at the last lecture, of one of the non-analogous formations, or one of those depositions which is unlike any thing in the healthy body. I was speaking of a tubercle which is often called a scrofulous tubercle, and occasionally has the word tubercle applied to it alone, at least an attempt is now made by the French so to limit the expression.

Tubercle.

I mentioned that this new deposition is of a friable nature, cheesy in texture, and opaque; that originally it is semi-transparent and greyish; that it afterwards becomes yellowish; that it softens down generally in the centre, but sometimes at the circumference; that pus will take its place to a great extent, but that flakes of it are still seen in the pus; and that the whole of it is frequently discharged together. I mentioned that in adults it was particularly seen in the lungs, but that in infants it was continually seen in the mesentery.

These tubercles are said particularly to occur in men from the age of 21 to 28, and in women especially before the age of 20. In men they appear later than they do in females. They are not confined, as you may suppose, to the human subject: they continually arise in brutes. I believe, that both in this country and at Paris, monkeys generally die of tubercular disease, and tubercles are found in a large number of organs. They occur also in horses, pigs, cows, rabbits,

hares, sheep, and birds. It is said that a great number of the parrots which die at Paris have tubercles. Parrots, like monkeys, come from a warm climate, and become affected by the coldness and vicissitudes of such latitudes as Paris and our own. It is said, but I do not pretend to know much of such matters, that they are not found in dogs.

Now the predisposing cause to tubercles certainly appears to be a want of proper food, and a want of proper external temperature. If an animal be kept in the dark, and likewise in a damp situation, so that it is exposed to cold and damp, and particularly if it be fed on not very nutritious food, you may in many instances produce tubercles at pleasure. I believe that the great and almost the only cause of tubercles in the human subject, is the want of good food and of proper external temperature. Exposure to cold and moisture, and particularly, perhaps, alternations of temperature, will produce tubercles. When the disposition to them is once produced, it becomes hereditary, and you see children with every comfort around them, well fed and well clothed, become at a certain age the victims of tubercular disease. The disposition, when once established, certainly becomes hereditary; but thousands and tens of thousands die of tubercular disease, from exposure to cold, united with moisture, and the want of good food; and in all probability in a great measure from vicissitudes of temperature. However, the influence of these may be much diminished by good food; by keeping up a good fire within, we suffer much less from an external low temperature. It is food which is required for this purpose, not mere stimulants, such as spirits or wine. These will not answer the purpose; it must be good nourishment, which maintains a slow but constant fire, if I may so speak, within, and does not give merely a temporary excitement, which always increases the injurious effects of a subsequent low temperature.

This subject, however, will be particularly spoken of when I come to the treatment of phthisis, which is the great tubercular disease of this country.

This tubercular deposit acquires an enveloping membrane, and frequently two. Sometimes you see no lining membrane; sometimes the deposit takes place in a diffused manner, and it is said then to be infiltrated; but in the lungs you more frequently than otherwise see the tubercular deposit surrounded by a membrane; you will see a regular capsule; and in the interior of this membrane you often find a softer membrane, which may be easily peeled off, while the external is frequently pretty tough. When it is formed, tubercular deposit may remain for years without injury; but if the deposition be very great, it forms a source of irritation. If a great quantity be deposited in different parts, and often, indeed, where the deposit is trifling, the surrounding substance falls into a state of irritation, the tubercular substance softens, and that process takes place which I have already mentioned.

The cavity, when the matter is discharged, is for the most part irregular; it is not of a definite shape, but irregular, and the parietes generally grow harder and harder, and you frequently see the tubercular substance not entirely discharged, but adhering pretty firmly to the sides. These cavities frequently have sinuses communicating with the external surface, or with the large bronchial tubes. When the tubercle has been near the surface, and that is very common in the case of the absorbent glands, the ulcer has generally a flabby edge, which is turned inwards—the very reverse of what takes place when an ulcer is cancerous, and they often heal up perfectly well. Even in the case of the lungs, we shall see that occasionally such cavities do certainly heal.

These tubercles are attended by no pain, so far as the deposition itself is concerned, but the irritation around is frequently a cause of pain; and if a tubercle be near the surface of the lungs, the pleura without generally falls into a state of inflammation, there is more or less pleuritis, and the person suffers pain in the side. When a tubercle is producing much inflammation around, of course there must be the usual signs of inflammation. In the case of the glands of the neck, there is considerable pain when active inflammation is excited.

The disposition to form these tubercles is called a scrofulous habit of body, and the person is said to labour under scrofula or struma when labouring under such deposits as these. The marks of a constitution so disposed are generally a fair and fine skin, with fine soft hair, a dilated pupil, a large upper lip; and it is observed, that when the internal parts labour under scrofula—not the external parts, but the internal viscera—that the ex-

tremities of the fingers and thumbs, and even of the toes, become enlarged. It is a remarkable circumstance, but it continually happens, that in a great number of cases of phthisis, and certainly in scrofula of many other parts, the liver, and the mesenteric and lymphatic glands, the ends of the fingers become enlarged. I presume that a faint scrofulous inflammation attacks the last joint. Many persons, however, are scrofulous who have neither a fair skin nor soft hair, but on the contrary a dark complexion, but look pale, and still have dilated pupils and a tumid lip, and perhaps are pock-marked; but those more particularly disposed to it have certainly a fair skin, a pulse disposed to be quick, and elongated fingers. When the disease, however, becomes established, when tubercular matter is deposited, you see the ends of the fingers enlarged, so that the nail is prominent; it is like an acorn, exceedingly convex, and the last joint altogether becomes broad. This disease, as I said, may undoubtedly be hereditary, and is so in a very large number of cases. It is a disease that attacks persons of all ages, but particularly in the early parts of life.

Scirrhus.

The disease of which I am now going to speak is rarely seen in the young—rarely seen before the middle period of life—and generally after the middle period is past. This disease is scirrhus.

In scirrhus it appears that there are two changes—there is a transformation and a new formation. It would appear that the cellular membrane of the affected parts becomes exceedingly indurated, and is changed into a hard fibrous membrane; but in the midst of this there certainly is a new deposit of a particular description. When scirrhus takes place, you will see a firm, exceedingly hard, unequal, irregular mass; it is of a light greyish colour at first, and if cut into thin slices it is semi-transparent. Here is a specimen of scirrhus, but it is hardly cut thin enough to shew the translucency. Here is another specimen, in which you see a large number of fibres traversing in different directions, and between these there is a substance less white than the rest. The deposition constituting tubercles is inorganic; it is not a new organization, but a new deposition. In scirrhus there is a transformed structure at any rate, and besides that, there is an inorganic substance deposited between the fibrous portions. These fibrous portions, running in different directions, form septa—divisions; and you see they are opaque and paler than the others—that is to say, of a more dead white; in fact, a scirrhus tumor like the one I have just shewn you cuts exactly like a turnip. In a turnip you see fibrous septa running in

different directions, and a softer less white substance between them. The septa in scirrhus run in every direction, and sometimes are seen to form regular cells. The proportion of less hard substance between the fibres is exceedingly various, and the mode in which the fibres are distributed is likewise exceedingly various, so that you may sometimes have a mammary tumor, sometimes a pancreatic tumor, and sometimes a tubercle—that is to say, a tumor something like a mamma, something like a pancreas, or something like the tumor of scrofula—you have a tubercle in the common acceptance of the word.

The less hard substance at last undergoes the same process as a true scrofulous tubercle; it softens down into an ichorous fluid, into something like jelly or gum, and the process here begins usually—perhaps always, but at any rate usually—in the centre, as is generally the case in scrofulous tubercles, the centre having been originally the hardest part. The skin above becomes puckered, or retracted, and its colour also becomes changed; it assumes a leaden, or livid hue. At first the whole tumor is moveable; though it will not allow the whole of the fingers to be placed under it—it will not allow the edge to be turned up; but after a time it forms adhesions to the neighbouring parts, and becomes immovable.

Cancer.—Ulceration takes place exactly as in the case of a scrofulous tubercle; and when the ulceration begins, that state of things is called cancer; scirrhus being the first stage—the stage of induration; and cancer the second stage—that of softening and ulceration. In this ulcer the edges are everted and elevated; you see the edges much raised, irregular, and turned out. The surrounding cellular membrane undergoes the process of suppuration. Now and then we see a sort of fungus sprouting up from the ulcer—a hard gristly fungus. The centre of such an ulcer is deep, the discharge is generally very fetid, and great irritation is produced. Sometimes, instead of a simple suppuration around, we have sloughing, and now and then nature succeeds in throwing off the whole mass; the scirrhus tumor has not formed adhesions to the surrounding parts, but it suppurates, or rather sloughs out. The lymphatic glands, to which the absorbents of such a tumor run, generally become contaminated; they generally become indurated, scirrhus, and undergo the very same process as the original part.

Now this is a disease which generally affects these parts primarily which are not necessary to life. It affects glands, the functions of which have been interrupted, or have never been performed; it affects the breasts, particularly when a woman is past child-bearing; and it particularly affects the

breasts of women who have never had any children. It also, where there is any predisposition, particularly affects parts which have suffered mechanical injury. Many women have had cancer in the breast after a blow, who in all probability would not otherwise have suffered. It particularly affects the breasts, the uterus, the ovaria, the testes, and the thyroid gland, none of which parts are necessary to life—the breast, uterus, ovaria, and testes, being all for the sake of the next generation, and not for the sake of ourselves, except, indeed, as a gratification. The thyroid gland is, of course, a part unnecessary to life. When, however, it has existed in these, it affects other parts secondarily, and then we find the lungs, liver, omentum, mesentery, spleen, pancreas, the brain, the medulla of the bones, and the skin, become the subjects of the affection. Now and then it may affect these parts primarily, but as a general rule—and one can only speak generally—it only affects the parts which I have now mentioned secondarily. When it affects the skin primarily, which it now and then does, a sort of wart is the first thing which appears, and it becomes cancerous; if it affect the skin only secondarily, then I believe more generally you have a tubercle, in the common sense of the word—a little hard lump; for example, when the breast has been affected with cancer, the skin in the neighbourhood will become the subject of tubera—little hard scirrhus lumps. However, we certainly often see it affect the cardia, the pylorus, and the rectum: yet although it does affect these primarily, it is by no means so frequent an occurrence as the affection of those particular organs which are not necessary to life, and whose functions may cease without the body at large suffering. When it affects the alimentary canal, it attacks particularly certain portions, which form divisions of it; for example, it affects the lips, which are the first part; then it will affect next to them the fauces, the cardia—the lips forming the commencement of the mouth, the fauces of the throat, the cardia of the stomach; then it affects the pylorus, the commencement of the intestines; and again it affects the rectum, which is the termination of the canal. It is a curious circumstance, but generally it is the openings of cavities that are affected. We shall see when we come to consider diseases of the heart, that it is the openings which suffer far more than any other portions of that organ.

Speaking of secondary cancer, I may mention, that it has been observed that when the breast has been the subject of this disease, the bones often become affected; they become flexible, and easily break, and the fracture discloses a bloody mass in the medullary cavity. The affection spreads around, so that muscles, cartilage, and bone, are all

blended together into a cancerous mass. You will find a paper on this subject, in the 15th volume of the *Medico-Chirurgical Transactions*, by Mr. Salt.

This disease is attended in general, almost from the first—from the very earliest stage—with severe pain,—pain that is sharp, lancinating, and of the most dreadful kind. The pain certainly does not depend upon inflammation; it will occur where no inflammation can be discovered.

When the stage of irritation has arrived in which there is inflammation, sloughing, and suppuration, the irritation is so great that the whole constitution suffers materially, and the skin acquires a peculiar sallow tint—a sort of pallid yellowish straw colour,—and that state of the body is decidedly cachectic; that is, the whole body is in an unhealthy state. Andral ascribes this sallow appearance to a change which the blood undergoes, the constituents of the vital fluid being absolutely altered. There is, however, a sallow look by which any experienced person will suspect that the individual is labouring under some malignant disease. This has been called *cancerous cachexia*,—cachexia meaning a bad habit of the whole body.

I mentioned that this disease sometimes sloughs out, and the person recovers; that, however, is so rare a circumstance, that, unfortunately, if the surgeon remove the disease, the relief is but too often temporary, and in the greater number of cases the disease reappears, either in the same neighbourhood, or in an opposite part. It would appear that it depends, in a great number of cases, upon some constitutional tendency, and all that art can do is to remove those parts which have fallen into a diseased state; but the tendency remains in the constitution, and therefore, sooner or later, persons generally shew the disease again in some other situation, or around the spot of the operation. This disease, like scrofula, may be hereditary—I mean, of course, the disposition to it; and it is not known, at least as far as I am aware, what circumstances give a predisposition to it. Those which give a predisposition to scrofula are evident enough, but I am not aware of any circumstances which are known to give a predisposition to scirrhus and cancer. Unquestionably the disposition may be hereditary. I have known many persons die of cancerous disease, one of whose parents had previously died of the same affection. [The learned Professor here exhibited several drawings by Cruveilhier, illustrating the various stages of this affection; and also three preparations, one in which the disease had attacked the brain, one in which it had affected the breast, and the third shewing the stage of induration.]

Encephaloid Disease.

The next disease of which I shall

speak is one which differs from scirrhus and cancer, by affecting the young rather than the old: it is what is called *fungus hamatodes*, or *encephaloid disease*. It has been called *encephaloid* because the deposition is not hard, like scirrhus, but to a certain extent resembles the brain; and it has been called *fungus hamatodes* because, when it arrives at the stage of ulceration, a fungus sprouts out, of a bloody character, and, on cutting into it, you see large distinct cells filled with effused blood. It is, therefore, like brain in some parts, and bloody in others; the deposition is like brain, but in the cells in which the deposition takes place hæmorrhage continually occurs, so that blood is effused there, and a coagulum forms.

There is a great disposition in this disease to hæmorrhage, but the deposition itself is an opaque, whitish, homogenous substance, and may be compared, in colour and consistency, to cerebral pulp. Here is an excellent representation of the affection, by Dr. Carswell, in which it is situated in the brain and liver. I have opened many brains and livers with tumors about the size of a walnut, which, on being cut into, presented a homogeneous brain-looking pulp. There is often nothing like the firm fibres of scirrhus—no distinct white bands ramifying in the tumor, but a mass such as I have just described. When exposed to the air, it softens down just as the brain will do, and it may be washed away, leaving a filamentous texture. If you take a tumor of this description and expose it to a stream of water, you may wash the pulp away, and then you see an exceedingly fine filamentous structure, which has contained the opaque brain-looking substance; but then this structure is not hard, like scirrhus, but exceedingly fine and delicate. The consistency of encephaloid varies from that of a soft custard down to the firmest part of the brain when perfectly fresh. One portion of this is sometimes pinkish; and sometimes portions will be as red as a clot of blood; but if the blood have been effused in great quantity, then you have regular clots of blood. You will frequently find various portions differing in colour, size, and consistency, and some are even cartilaginous—that is to say, I presume there is a double formation, a double disease—that scirrhus is united with it. Now and then you will see bony particles. It occasionally happens that portions of what is usually a fine filamentous membrane become converted into an indurated substance, like cartilage, and now and then there are little deposits of bone; for many of these structural diseases are all blended together. Sometimes you see portions of it in a regular cyst; instead of fine filaments containing this brain-like matter, you see absolute cysts; and sometimes you see portions re-

sembling the white of boiled eggs—coagulated *albumen*.

Now this deposit, as it grows, softens down, and, like scirrhus and a scrofulous tubercle, it generally softens first in the centre. When it softens down, cavities are formed; or perhaps we might say cavities become discernible; and these cavities then chiefly contain blood, and on washing this away you see filaments or shreds floating in the cavity. If a portion of it be near the surface, the skin grows discoloured and the tumor adheres to the subjacent parts, and increases; or if the tumor be situated within, the serous membrane above it grows thin till it gives way. So that you may have the tumor ulcerate through the surface, or you may have a serous membrane within give way.

When the tumor ulcerates, a fungus shoots forth, but it is not firm, like the fungus of scirrhus, but, on the contrary, it is soft, easily torn, and bleeds very much. It is irregular, and of a dark red colour. When the fungus is very small, it exactly resembles the red soft polypi which grow from a mucous membrane. It grows very rapidly, and pours forth a fetid sanious fluid; and people sometimes die, not from the irritation of the tumor, but from hæmorrhage. I had a patient who died from hæmorrhage, occasioned by one of these fungi in the bladder. There was a fungus as large as a walnut in the interior of the bladder, which never gave him any pain, which produced no irritation to the constitution, but it bled in spite of every thing which could be done; and he discharged, not only bloody urine, but pure blood, and at last sank under it. The hæmorrhage from a fungus of this description is often very copious. Now and then portions will slough.

This is a disease which affects every part of the body. It will affect the testicle—and then it is called by some, *soft cancer of the testicle*—the breast, the eye—it is very common in the eye of children—the uterus, ovaria, spleen, pancreas, liver, urinary bladder, brain, mesentery, and bones. It is continually seen in the extremities, and the absorbent glands become contaminated as they do in the case of common cancer; and when the glands are affected, and you make a section, they disclose nearly the same appearances as the original tumor, but there is this difference, it is said, that they never send forth a fungus.

You frequently have in the neighbourhood smaller tubera, circumscribed, but without a capsule at all, varying from the size of a pea to that of a walnut, of a pale greyish colour, and firmer than the original tumor, but they are the same disease. This is a disease which commonly affects other organs secondarily, so that when persons have it in the breast it is very common

to find them gradually become the subjects of cough and pulmonic disease, and, on opening them, you find similar tumors within. It is said that the organs which are affected secondarily are never affected primarily—it is so said. It is very common for many organs to become affected at once; and so great is the disposition to this disease, when there is any disposition at all, that Mr. Travers says he has never known a person survive four years in whom he had operated for the extirpation of the disease. The constitution becomes impaired; there is a cachectic look even earlier than in scirrhus. Usually the person becomes emaciated, but now and then there is hardly any irritation at all, and you will sometimes see a person die of this disease in the stomach without having suffered any pain at all, and frequently without any person having suspected the nature or even seat of the disease. With respect to the case I mentioned of the disease in the urinary bladder, the man, although he died from the hæmorrhage occasioned by it, never suffered the least pain. Even when the mass is very considerable, there is generally little or no pain, and sometimes there is little or no irritation of the constitution. The case differs, therefore, in another respect very materially from scirrhus. Scirrhus, I mentioned, is almost always attended with violent deep lancinating pain; whereas, in encephaloid disease, there is very little pain, frequently none, and sometimes there is very little irritation.

A tumor of this description is exceedingly soft; its external appearance is remarkably smooth and equal, and it gives you the idea of fluctuation, so that I have known surgeons of great experience deceived in cases of this nature. They have imagined it was a collection of fluid, and plunged a lancet into it. I have seen this mistake occur over and over again, from its extreme resemblance, with respect to the touch, to a tumor containing fluid. Frequently, for a length of time, the part will not be at all discoloured. It is soft and elastic, and it is these two circumstances that give you the idea of fluctuation. The tumor, when taken out, is generally more or less round, and as I stated before, it differs from scirrhus in occurring at an early period of life. You saw a kidney the other night, taken from a child six years of age, in which several portions of disease were of this nature. It seemed to be a mixture of various diseases—some portions were scrofula, some were of scirrhus hardness, and in other parts there was certainly fungus hæmatodes.

I do not know what gives the disposition to this disease. I am not aware of any external circumstances which cause persons to be more liable to it, nor do I know whether it is hereditary. It is very probable that the

disposition to it is hereditary, exactly like the disposition to scirrhus, but it is carefully to be remembered that this is a disease, not of pain, like scirrhus—and that it is a disease which affects the opposite period of life to that in which scirrhus particularly prevails. Some consider it a mixture of scrofula and cancer.

At the next lecture I shall proceed to consider melanosis, and so terminate the consideration of structural diseases.

CLINICAL LECTURES,

Delivered in the Dispensary of the London University,

BY

DR. ANTHONY TODD THOMSON.

LECTURE IX.—Jan. 13, 1832.

Pulmonary Consumption.

GENTLEMEN,—In the case of the woman, Emma Warwick, who presented herself on the 31st of December last, you have had an opportunity of seeing the opinion that Phthisis is contagious fully verified. The husband of this poor woman had been a patient of the Dispensary for many months, and is now either dying, or has died, in one of the hospitals. He was in the last stage of Pulmonary Consumption, expectorating purulent matter, worn down by hectic, and presenting every symptom characteristic of the closing scene of that intractable disease. The poverty of the family of this man prevented his wife from occupying another bed; and she soon became the victim of decided Phthisis. The poor woman stated, that soon after her husband began to spit matter, she felt her breathing shorter, and more hurried than usual, on any slight exertion, and accompanied with pain of the side passing through to the back; she had also had a short troublesome cough, which lately much increased. Her countenance was sallow, her tongue furred, and her bowels were irregular, but generally confined. She had occasionally felt sick after taking food, and had lost her appetite; her urine was high-coloured. She could give no account whether any of her brothers or sisters were ever affected in a similar manner; but affirmed that she had always enjoyed good health until her husband became so ill. The husband, since her application here, was removed to an hospital, and her disease is rapidly advancing to a close, constituting that variety of the malady which has been termed *galloping Consumption**. I have brought the case before you, in order to make a few comments on the treatment of the disease, which is followed in this Institution, and

on the particular fact of its contagious property, still doubted by many physicians.

The first mention of the contagious nature of Consumption which deserves notice, is by Galen; for I do not place much reliance on the query of Aristotle, which is rather to be referred to some floating popular opinions of its contagious nature, than to his own knowledge of the fact. Galen expressly says, that it is dangerous to pass a whole day with the consumptive; and his opinion is supported by Ballonius, Riverius, Morton, Van Swieten, Withering, Darwin, Dr. Reid, and others. Dr. Beardsley mentions an instance where the disease passed from a European family in the West Indies, to the negroes on the estate; and so strongly were both Morgagni and Valsalva impressed with the contagious character of this malady, that they even considered it dangerous to dissect the body of a person who had died of it—an opinion which, notwithstanding the justly-celebrated names of its promulgators, I cannot help designating as extremely absurd. On the contrary, Cullen doubted its contagious character, although he did not expressly deny it; and the same idea of it was entertained by Castillani, Heberden, and Beddoes. Portal thought that it was not contagious, but might be communicated by the milk of a nurse. The College of Tuscany decided completely against its contagious nature. As in many other affairs, truth lies in the middle; and my own experience leads me to coincide with Hoffman, that it is contagious only where an hereditary predisposition exists. In the case before us, and in many others which I have seen, this hereditary predisposition is not easily traced; but when it has occurred in the middling and higher walks of life, I have had little difficulty in tracing it. Of the existence of this predisposition there can be no doubt; and, if the presence of incipient tubercles in the lungs can be regarded as a proof of it, many instances might be adduced in which these bodies have been discovered in the lungs of infants, and those of adults who have died of diseases not at all of a pulmonary nature, and who have never displayed any obvious tendency to Phthisis. Now, it is easy to imagine that such individuals, placed in the situation of husband or of wife, or even of nurse to a person in the last stage of Consumption, and weakened by watching, fatigue, and anxiety, should be very susceptible of having disease set up in these latent tubercles, by constantly inhaling the breath of the consumptive patient. On the other hand, I cannot conceive how the disease can be communicated to a person possessing sound lungs, when we have every reason for thinking that, whatever may be the exciting causes, the anterior existence of *nascent tubercles* is an essential ingredient in the formation of Consumption—cold, or whatsoever can produce

* Since this lecture was delivered, the poor woman has died, and her husband also.

inflammation, merely converting the latent into active disease.

If the previous existence of nascent tubercles, therefore, in all cases of Phthisis be admitted, there is no difficulty of explaining the manner in which the breath exhaled from the ulcerated lungs of a consumptive person may communicate the disease to any one in whose lungs nascent tubercles exist. We know that, in such individuals, mechanical irritants—for instance, the fine metallic dust produced in pointing needles, the dust arising from the dressing of flax and feathers, and from other sources—excite the disease by their effect on the nascent tubercles. There can be no doubt, also, that the air expired from ulcerated lungs may carry along with it irritating or contagious matters; indeed, we have an assurance of this by the impression which the factor of the breath makes on the organ of smelling. These effluvia, carried into the lungs of another person, whether absorbed or merely applied to the lining membrane of the bronchial tubes and cells, will readily excite inflammation in the neighbouring tubercles, and set up that train of symptoms which constitute Pulmonary Consumption. In this manner I am persuaded that the disease is communicated to the predisposed, who sleep with, and are in close attendance on, individuals in the last stage of Phthisis; and that it is not the consequence of inflammatory action commenced in healthy lungs by any exciting cause whatsoever; nor is it dependent on a scrofulous diathesis, although much resembling it, since we find that the progress of tubercles to ulceration is not the same as that of inflamed glands, or tumors enlarged from lymphatic obstructions.

The consumptive diathesis is certainly not identical with that of Scrofula; yet both may exist in the same person. Dr. Withering mentions that one of the frequent characteristics of a consumptive habit, is an unusual magnitude of the pupil; this is also not unfrequent in strumous individuals, where no symptoms of the advancement of Consumption can be traced, and no hereditary predisposition is known to exist.

As some of you, gentlemen, have probably never seen tubercles, either in their nascent or diseased state, it may be proper to describe to you what appearances they present. Nascent tubercles are small, granular, indurated substances, found in the interstitial or cellular texture of the lungs, and in other parts of the body. You will also find it to be true, although not easily explained, that the left side of the lungs always contain more of these bodies than the right side, and the superior than the inferior part of the lungs. At any period anterior to the appearance of phthisical symptoms, in which circumstances have afforded opportunities of examining the lungs of those predisposed to

consumption, these tubercles appear extremely minute, of a greyish white colour, nearly but not wholly opaque, and not unlike the seeds of the miller. Soon after the cough commences, they acquire a slight degree of transparency, and often present the appearance of dark yellow points dispersed thorough them. By degrees they enlarge, until they exceed the size of common peas, when they soften in the interior, and additional tuberculous matter being deposited around them, many coalesce into one mass, which is either in part internally changed into imperfect pus, or that substance is formed within them, and, filling their centres, dilates them, until they give way, at several points, and the pus oozing out is discharged into some of the neighbouring bronchial tubes, and expectorated. The ulcerated cavities thus formed sometimes become lined with a pellicle, beneath which a semi-cartilaginous membrane extends, constituting a kind of continuous surface, with the internal coat of the bronchial tubes, into which they open. It has, with much probability, been supposed that the enlargement of tubercles, and the formation of pus in them, is generally the result of inflammation set up in the lungs; and this is rendered more probable by the thickening and induration of the adjacent cellular matter, caused by coagulable lymph, the usual effect of inflammation, being thrown into it; and also by the not unfrequent expectoration of calcareous matter, which can only result from inflammatory action, in those habits in which nascent tubercles exist. Whenever, therefore, the exciting causes of inflammation influence the lungs, a new action commences in the vessels surrounding and supplying the coverings of the tubercles, and this continuing, the tubercles run into one another, suppurate, and form vomicae, which, on emptying themselves, leave a large ulcerated surface exposed to the action of the air at every inspiration; so that hectic ensues in the same manner as if large ulcerated surfaces on the exterior, in highly irritable habits, were left exposed to the action of the air.

These remarks, gentlemen, might lead you to conclude, that in my opinion cold or sudden alternations of temperature, such as generally operate in causing pneumonic inflammation, is the sole exciting cause of phthisis whenever nascent tubercles exist; but this is too hasty a conclusion, and I am induced, like those who find others impatient in argument, to say—nay, nay—hear me out.

Always keeping in view the predisposition to the disease, and thence the probable existence of nascent tubercles, in every case in which real Phthisis occurs, the exciting causes of pulmonary consumption may be arranged under two heads, *physical* and *mental*.

The physical causes appear to comprehend many varieties of agents. Thus, as I have

already stated, mechanical irritants finding access to the lungs, may set up inflammation in pre-existing tubercles, or it may be set up by acrimonious states of the secretions, the consequence of other diseases, such as Scrofula or Small-pox. Measles, Scarlatina, and similar affections, may produce the same effect, or it may follow exposure to sudden alternations of temperature, capable of producing catarrhal inflammation in the chest. It is not necessary, however, that the temperature should be very low to produce this effect: I have seen it induced by sudden exposure to the evening air, in the middle of summer, when the thermometer was standing at 65°. With respect to the influence of *mind*, as an exciting cause of consumption, it is well known that persons predisposed to this disease are usually of a nervous temperament, highly susceptible of mental impressions. A look, a word, will hurry the action of the heart, even to palpitation; disturb and embarrass the respiration; influence powerfully the secretion of the liver, and impede the functions of the stomach. In such a state of habit, it is easy to conceive in what manner moral and intellectual agents may develop diseased action in nascent tubercles. Nothing so forcibly illustrates this influence of mind over body, even in altering organic structure, than the progress of the disease named *Nostalgia*. In this affection, when the longing for home cannot be gratified, emaciation gradually creeps on, attended by cough, pain of the chest, hectic, and many symptoms resembling those of pure Phthisis, until the patient has his desire gratified, or he sinks a victim to the disease. Postmortem examinations have detected appearances of inflammation, adhesions, and even ulcerations of the lungs. In the same manner, when much debility, accompanied with great irritability, consequent on too rapid growth, occurs; or when, in those predisposed to Phthisis, there is much mental disquietude, anxiety, or depression of the spirits (particularly in young females in the middle and higher ranks of life), congestions take place in the larger vessels of the lungs; the capillaries soon afterwards become also affected; inflammation of a sub-acute kind is set up, and morbid action in nascent tubercles rapidly developed. It is lamentable to reflect how often from these causes society is robbed of many of its most amiable and highly-accomplished members.

I may take this opportunity of remarking, that, in my opinion, hæmoptysis, which is put down by many authors as an exciting cause of consumption, is the result of such a state of lungs; and therefore it is to be regarded rather as an indication of a state of the chest likely to induce tubercular consumption, than as a remote cause of the disease. In these cases, there is no rupture of vessels; and the hæmorrhage is to be re-

garded as symptomatic of advancing tubercles in the lungs. I am aware that the high authority of Andral opposes this opinion, but my own observations are the source of my belief on this point.

To return to the consideration of mental anxiety as an exciting cause of Phthisis. Why, you may reasonably demand of me, do you not refer Emma Warwick's case to that source, rather than to contagion? My reply is, that I had not observed those sympathetic feelings in this woman that would lead me to form such a conclusion; and that the attack was too sudden also, to have originated in such a source. It would be unjust to affirm that she had not been anxious on her husband's account; at the same time her anxiety was not of that corroding kind which is likely to bring on such a rapid Consumption as she is now labouring under.

Gentlemen, in examining the chests of phthical patients in this Institution, you have seen the ear, with and without the stethoscope, and percussion, employed; and it is proper that you should know what information you may expect from the employment of these tests of the state of the lungs. In the early stages of the disease, neither auscultation nor percussion are of much value; the tubercles are then rarely congregated in such numbers as to form a mass capable of so far obstructing respiration, in any portion of the lungs, so as to render it inaudible; or to afford a dull sound on percussion. But as soon as the expectoration assumes traces of a purulent character, then we may expect to discover pectoriloquism, more or less complete, indicating the existence of a cavity in the substance of the lungs. This is best ascertained by applying the excavated end of the stethoscope on various parts of the chest, and desiring the patient to utter a few syllables. If the voice of the speaker appear to proceed through the bore of the instrument, we may certainly conclude that there is a cavity of the lungs beneath it, communicating with the trachea by means of some of the bronchial branches. On the examination of the chest of Warwick, pectoriloquism was perfectly evident; but it could not be traced in that of his wife when she was examined, two days after she applied here for advice. In making this examination you should be aware, that in thin people with a sharp voice, some degree of pectoriloquism may be obtained by applying the instrument over particular parts of the chest, under the arm-pit, for instance, or at the junction of the clavicle and sternum, even when the lungs are perfectly healthy. It may be said that as correct a diagnosis might be drawn from the appearance of the sputa, as by the aid of the stethoscope; but the difficulty of distinguishing the mucus of an inflamed surface from pus, is too great to permit much reliance to be placed on this mode

of deciding; and no chemical test has yet been discovered for accurately determining the character of pus. The best test is that proposed, I believe, by Dr. Young, which depends on the colours produced by placing some of the expectorated matter between two pieces of plate glass, holding it near the eye, and looking through it at a distant candle. If the sputa be purulent, a circular arena of colours, green and red, will be observed surrounding a red area, of which the candle is the centre; but various circumstances may occur to render this test doubtful; it is, however, a good accessory to the stethoscope.

With regard to the treatment of Phthisis in its early stage, as few cases present themselves to our notice at that period, and having, therefore, little opportunity of exemplifying, by reference to cases, the correctness of my opinions, my remarks shall be very brief. As I have contended for the tubercular origin of the disease in all instances, you might, gentlemen, imagine that I must necessarily coincide with M. Bayle, and many other eminent men, in supposing that, owing to the natural tendency of tubercles to increase in size, soften, and suppurate, Consumption is incurable in its early stages; and that, if recovery ever take place, it must be, in a more advanced stage of the disease, the result of those curative efforts of nature, which form the semi-cartilaginous membrane on the surface of the tuberculous cavities, so as to constitute a kind of cicatrix, or shield, for guarding the diseased surface from the action of the air. But even admitting the accuracy of this opinion, are we to stand with our arms folded waiting the approach of this event, which is not likely to occur once in one thousand instances? Certainly not. It is our duty to arrest, if possible, the progress of the disease, admitting that we cannot cure it; and, therefore, those measures most likely to effect this, should instantly be resorted to. As yet the strength is not reduced; and bleeding, Cathartics, with other means of resolving inflammation, and low diet, should be employed. At first, nothing more will be requisite; for I am of opinion that Expectorants at this period are useless, and rather tend to do harm than to benefit the patient. Our principal object is to subdue the inflammatory action, and reduce the congested state of the pulmonary vessels; but the question is, whether this is to be accomplished by a full bleeding, or repeated smaller abstractions of blood. The degree of strength of the patient, the state of pulse, and the appearance of the blood, must regulate our decision. If there be evident febrile excitement, accompanied with pain on taking a deep inspiration, and a buffed state of the blood drawn with a full stream, then there can be no doubt of the propriety of repeating the venesection; but, if the pulse be soft and weak, although the blood may show

the buffy coat, it ought not to be repeated; this state of the blood being often the consequence of much nervous irritability, when no actual inflammation is present. In this case, however, the first bleeding may be useful in removing plethora; and although the lost volume of the blood is soon replaced, yet the abstraction, by increasing the *vis a tergo* of the overloaded vessels, enables them to contract upon their contents, and to carry forward the blood more freely. By this agent the tendency to morbid action in the capillaries, which would be the necessary result of the continuance of a congestion of the larger vessels of the lungs, is taken off. When a repetition of blood-letting is advisable, it is sometimes better to substitute cupping for the lancet. The chief mode, however, of reducing the inflammation in the early stage is by the influence of counter-irritants, or as they are termed, perhaps more justly, contrastimulants. These may be divided into *internal* and *external*.

With respect to internal contrastimulants, the *Tartrate of Antimony and Potassa* is perhaps the best in this stage of the disease; and the effects which you have witnessed from its employment, both in incipient Phthisis and in chronic Bronchitis, in this institution, have been such as to secure your confidence in its contrastimulant powers. I have never found it necessary to prescribe it in the large doses recommended by the Italian physicians; and I have found that nausea is maintained better by small doses than by large. Even in doses of one grain every third or fourth hour, although nausea and vomiting follow the first and second dose, yet neither occur afterwards; whereas, by half-grain doses, I have been able to keep up a state of sickness for several days;—and it is only by doing so that any benefit is derived from its employment. The sudorific effect of the antimonial is not disadvantageous in this stage of the disease. Ipecacuanha has proved generally beneficial when given in combination with a neutral salt and some narcotic: perhaps the compound powder of Ipecacuanha of the Pharmacopœias, in conjunction with Tincture or Extract of Conium, is the best method of administering it. A slight nausea should be maintained, and the bowels at the same time kept only moderately open. Such a combination, given in conjunction with a drachm or more of distilled vinegar, and from one grain to five grains of Acetate of Lead, I have seen relieve hæmoptysis sooner than any other means. The vinegar prevents the deleterious influence of the salts of lead. *Tincture of Digitalis* is, in my opinion, prescribed in this stage of the disease on the most erroneous principles: it is not a direct sedative, but, like every other narcotic, its first action is stimulant—its second only sedative. When inflammation is present, whether in Dropsy

or in Phthisis, it is never useful; but it produces the most beneficial effects after the inflammatory action has been subdued by the aid of the lancet. If emetics be necessary, either Tartar Emetic or Ipecacuanha are preferable to more direct emetics, such as the sulphates of zinc or of copper; on account of the nausea induced by the former, and the influence which this exerts on the lymphatic and absorbent systems.

The most powerful external contrastimulents are blisters, the warm plaister of the Dublin Pharmacopœia, and the Tartar emetic ointment; and any one of them may be employed with advantage. If blisters be adopted, they should be frequently repeated, and applied alternately between the shoulders and on the fore part of the thorax. I have more than once used the actual cautery with benefit. The action of rubefacients is too transitory to prove permanently beneficial.

The diet of the Phthisical, in this stage of the disease, should be altogether milk, vegetables, and farinaceous matters. When milk oppresses the stomach, it may be diluted with soda water, which renders it more grateful, and aids the digestive powers of the stomach. One of the most important points with respect to regimen in this stage of the disease, is not available in that rank of society to which the patients of this and similar institutions belong; I mean removal, during winter, to a warm climate, or confinement to rooms graduated to a temperature not under 65 degrees. I knew a celebrated professor of the practice of medicine, whose wife, having a tendency to Phthisis, regularly disappeared with the swallows, and was not again visible in general society till late in the following spring; and by this means the disease was effectually warded off. In recommending, however, a graduate temperature, too little attention is paid to the humidity of the warm air: in every climate favourable to Phthisis, we find that the air is not only warm but moist. A hygrometer is as necessary an instrument in the winter conservatory of the Phthisical as a thermometer.

In the latter stage of the disease—that period of it at which most of the patients, who present themselves to us, have arrived—our hopes of cure, in my opinion, are futile; and consequently the object is to palliate—to restrain the frequency and violence of the cough—to improve the quality of the purulent secretion—to mitigate the hectic fever which wears down the patient, and to aid nature, if it be possible, in forming that membrane to which I have alluded, and which seems to be the only prospect of recovery, however rarely it is fulfilled. To attain the first indication, the Muriate of Morphia, in combination with the decoction of *Cetraria Islandica*, or other mucilages, has been found to answer extremely well;

or, where opium in any of its forms cannot be employed, the Tincture of Conium, of the Dublin College, has been used with advantage. Nothing has appeared to improve the quality of the expectorated matter, and to promote its expulsion so effectually, as the inhalation of much diluted Chlorine. I have employed this agent for several years; and, besides being satisfied with its salutary influence in the cases in which it has been used, my confidence in it has been greatly confirmed by the accounts of its influence on the lungs of the workmen employed in the works of Mr. Tenant, of Glasgow, who is the greatest manufacturer of bleaching liquor, perhaps, in the world. Mr. Tenant informed me, that men who came to him with fixed coughs soon lost them when they were gradually brought into the chlorine house. In the cases which present themselves here, I generally order the Chlorine to be extricated from the common materials, a mixture of sea salt, black oxide of manganese, and sulphuric acid, in a corner of the sleeping apartment of the patient; and in all cases they have expressed, in strong language, the comfort it affords them; lessening the frequency of the cough, and greatly improving the quality of the expectorated matter. Nor is this at all difficult to be explained, if we revert to the beneficial influence which it exerts in its aqueous solution, as a gargle in malignant Scarlatina, and as a lotion in fetid and ill-conditioned ulcers on the surface of the body.

With the view of mitigating hectic, I have had little reason to be satisfied with the effects of Foxglove, Myrrh, the Balsams, or tonics in any form, with the exception of vinegar, which I have given in doses of half a fluid ounce three times a-day, in combination with Opium and the decoction of *Cetraria*; whilst, at the same time, the trunk of the body has been sponged with vinegar and water before the patient gets out of bed in the morning. This plan seems to alleviate the hectic almost in the ratio of its influence, in moderately confining the bowels; and when its internal employment has disagreed with the stomach, I have found nearly equal advantage from the use of the Chalk mixture, or solutions of *Catechu* or *Kino*. In proportion as the diarrhœa has been moderated, or rather as the bowels have become confined, has amendment, or rather mitigation of the symptoms, taken place; and, instead of aggravating the febrile symptoms, this state of bowels has almost invariably appeared to alleviate them. The truth of this remark was strikingly verified in one of the cases which was lately before us.

CONTRIBUTIONS

TO

THE PATHOLOGY OF THE BRAIN,
IN INSANITY, EPILEPSY, AND
GENERAL PARALYSIS.

By WILLIAM DAVIDSON,

Member of the Royal College of Surgeons of
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"Quisquis enim artificiosè corpora humana
secare novit, eorumque singulas particulas diligen-
ter inquiri, ex his latentium morborum causas et
sedes facile intelliget, necnon accommodata reme-
dia prescribet."—*J. Riolan, Anthropol. lib. i. p. 15.*

[Continued from page 663.]

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CASE III. Old.—John Evans, ætat. 55, died Sept. 13, 1831. Prominent symptoms, referrible to the mental and corporeal functions, were diarrhœa; locomotive powers destroyed; general paralysis; marasmus; occasional pyrexia and flushings, attended with high excitement; fatuity; automatic life.

Autopsy, September 14, 1831.—Minute pisiform eminences disseminated in great abundance over the convexities; about two ounces of bloody serum in the cavity of the arachnoid; œdema of the arachnoid of both sides, elevated in the form of small bladders containing limpid serum; white, thickened, mucous condition of the arachnoid, from sub-arachnoid effusion confined to the convexities; slightly clouded, opaque, and thickened condition, from albuminous deposit on the inner surface, of that portion of the arachnoid unconnected with the pia mater, and situated behind the fourth ventricle and commissure of the optic nerves; adhesion of the pia mater to the cortical substance over the convexities of both hemispheres, particularly anteriorly; cortical substance red, highly vascular, and, in different parts, in a state of ramollissement throughout its whole depth; punctuated redness of the medullary substance very remarkable. White, pulpy ramollissement of the lower surface of the corpus callosum, and of the fornix; serous infiltration of the pia mater throughout, but particularly in the intergyral spaces.

Abdomen.—Liver in a state of pale induration and considerably reduced in size; mucous membrane of the ascending and transverse colon presented ex-

tensive elevated patches of redness, and was throughout much thickened.

CASE IV. Recent.—Edward Madden, ætat 40. Died Sept. 13, 1831. Prominent symptoms, referrible to the mental and corporeal functions, were typhoid pyrexia; constant agitation, requiring coercion; gradual sinking of the powers of life, without coma or convulsions; maniacal delirium, succeeded by low muttering, and complete insensibility.

Autopsy, Sept. 14, 1831.—Dura mater much injected—adherent over the left hemisphere to the inner table; its serous lining infiltrated with blood, particularly over the left hemisphere. Effusion of about eight ounces of bloody purulent serum into the cavity of the arachnoid; cerebral arachnoid covered at the convexities with a thick layer of consistent yellow pus, which completely obscured the convolutions; white, thickened condition of the arachnoid very apparent over the convexities after the removal of the purulent layer above described; clouded, opaque, and thickened condition (in a trifling degree) of that portion of the arachnoid situated behind the fourth ventricle and commissure of the optic nerves; cortical substance of a reddish lilac colour throughout, and in several places marbled, red, and softened. Excessive red punctuation of the medullary substance, bordering upon uniform redness; the cut surfaces presenting numerous bloody points, which enlarged on exposure to the air for a few seconds, and gave a rosy tinge to the white substance. Veins and sinuses gorged with blood. Pia mater thickened, uniformly red, and highly injected throughout; bloody infiltration, about two inches long and an inch broad, where it lined the anfractuosités of the right middle lobe. The other cavities were not inspected.

The circumstance of finding purulent effusion between the dura mater and the arachnoid, independent of any peculiarity, such as a blow on the head, struck me at the time as one of very rare occurrence. I have since only met with one well authenticated case in the course of my reading, and in referring to the best works on morbid anatomy. The case is detailed by Dr. Southwood Smith, at page 209 of his work on Fever, as having occurred in a man who died of typhus. The following is his description of this morbid appearance:—

"Dura mater thickened and opaque; beneath it gelatinous effusion; *upon its external surface a large quantity of well-formed pus*; a quantity of purulent matter at the base of the brain, surrounding the corpora quadrigemina." It is very common to find infiltration of pus, both in a concrete and liquid state, between the pia mater and arachnoid, in examining the brains of those who succumb to that very insidious, frequent, and peculiarly fatal class of maladies—the general paralysis of the insane, so well described in M. Calmeil's work on that disease. Dr. Bright, whose accuracy as a pathological observer few persons will be disposed to deny, affirms that pus is not effused upon the surface of the arachnoid, but that it fills the meshes of the pia mater. It is impossible to adduce higher authority on any subject connected with cerebral pathology.

CASE V. Old.—William Williams, ætat. 75, died Sept. 17, 1831. Prominent symptoms, referrible to the mental and corporeal functions, were frequent and severe epileptic fits, succeeded by dyspnoea; blindness; coma preceding death; dementia; obtuse sensibility; peevishness and quarrelsomeness; and paroxysms of ungovernable fury.

Autopsy, Sept. 18, 1831.—Cranium remarkably thick and heavy; dura mater very much gorged, and adhering to the inner table, so as to be split when the calvarium was removed. Unusual development of minute pisiform eminences disseminated over the visceral arachnoid, and connecting it with the arachnoid of the dura mater: œdema of the arachnoid elevated in the form of small bladders, containing limpid serum, over the convexities of both hemispheres; white, thickened, mucous condition of the arachnoid, from sub-arachnoid effusion throughout. Adhesion of the pia mater to the cortical substance partial, and confined to a few of the convexities of the convolutions.

Remarkable paleness and firmness of the external layer of the cortical substance, with redness, softness, and granulated appearance of the internal layer when the former was torn off; unusual depth, with remarkable thickness of the convolutions of the anterior and middle lobes; great development of the posterior part of the posterior lobes; medullary substance of a punctuated redness, bordering on a pale rose colour; cho-

roid plexus of both sides fleshy, and unusually large; veins of the pia mater gorged with dark-coloured blood; atrophy of the optic nerves (particularly the left), both anterior and posterior to the commissure of the thalami (particularly the right), and of both nates; general atrophy of the central parts of the brain; the centrum ovale of Vieussens was reduced to a narrow plate of medullary matter, of extreme tenacity, scarcely exceeding half an inch in breadth; much serous infiltration, with increased thickness, and remarkable redness of the pia mater.

It may here be asked whether the morbid alterations, found in this interesting and instructive case, are to be considered as the cause or the effect of the violent and frequent paroxysms of perfect epilepsy observed during life, or whether, in other words, the diseased changes give rise, after short intervals, to that peculiar organic modification, or irritation, in the nervous centres, upon which the disease depended; or whether they are, on the contrary, in all or most cases, themselves the product of sanguineous congestion, which is generally supposed to take place during the epileptic paroxysm, either as a cause or an effect, within the brain, and of the invasion of inflammation, as the result of this congestion; or whether they are to be considered merely as accidental formations; and, if we admit the organic changes as the exciting cause, why, under a permanently existing cause, was the effect so variable and intermittent. These interesting questions I do not at present attempt to answer.

Although we must be guarded in our speculations where it is so very difficult to avoid falling into error, and must satisfy ourselves with attempting to trace with caution, and in very general terms, the modifications of the nervous centres, whether congenital or acquired, which seem to be absolutely essential to the development of epilepsy, and the various causes by which, in an organ like the brain, such derangements in the mechanism of innervation may be supposed to take place, yet I think we may safely assume, in the present state of our knowledge, the following positions as in the highest degree probable:—That, in the act of throwing the muscles of a limb into a state of contraction for the purpose of locomotion, there takes place in the brain a certain organic mo-

dification, whatever it may be, which is transmitted through the nerves to the muscles that are about to contract, and that, immediately after contraction takes place, this modification of innervation ceases;—that when a disease takes place, characterized by preternatural, disorderly, involuntary movements, and by the excess of muscular contraction, a modification, similar to the former, and as liable to sudden cessation, passes within the brain;—that if, as we are induced to believe, there is such an organic alteration, causing epilepsy, its nature will probably for ever escape our means of investigation;—that the local lesion is not, in the majority of cases, the proximate cause of these general phenomena, but that, if it give rise to them at all, it can only be by determining a general modification similar to what we have just mentioned;—that the permanent changes of the cerebral substance itself, or the new formations within the head, morbid growths, from the internal table, giving rise to prominences in the natural projections at the base, thickened condition of the membranes, and other appearances, (the result of slow changes,) so often associated with epilepsy, may, in the majority of cases, be regarded as the ultimate effect of the convulsions which act by determining an afflux of blood to the brain, and consequently congestions, and, as has been said above, their frequent result—inflammation. But I must forbear here from dwelling on various facts bearing upon this disease, which have always appeared to me of very great interest in the pathology of epilepsy, and from urging the subject further than by merely stating, that these speculations comprise the doctrines of epilepsy as taught by Rostan, in his *Cours de Clinique*, at the Salpetriere, and since more fully developed by his pupil, Foville, at Rouen,—doctrines which may, perhaps, be regarded as legitimate conjectures, as strictly consonant with the diseased appearances connected with epilepsy, and with peculiarities in the mechanism of innervation, and in the circulation within the cranium.

This case of Williams's, in which the organic changes in the brain were such as affected chiefly its periphery, is extremely interesting, as tending, in combination with numerous other dissections of epileptic brains, performed at this institution, to confirm a remark

which seems to have been made, with much appearance of truth, by Dr. Bright, in his late splendid and valuable work,—that the organic causes of epilepsy (admitting them to be exciting causes) only connected immediately with the brain, are more frequently those which are situated on its surface, than such as are deeply seated in its substance. Without laying it down as an invariable principle, I could (in viewing these changes as cause and not effect) adduce a number of cases in support of the position maintained by a distinguished writer—that lesion of the cineritious substance, which is considered the most active part of the brain generally with regard to all its functions, gives rise to disordered modifications of innervation in that part, and that such disordered action is transferred to the distant parts of the body, producing involuntary motions.

CASE VI. Old.—Elizabeth Shipley, ætat. 48, died Sept. 19, 1831. Prominent symptoms, referrible to the mental and corporeal functions, were marasmus; ulcers of the sacrum; complete fatuity and insensibility; palsy of the locomotive muscles, and those of the voice; general paralysis of sensation.

Autopsy, Sept. 20, 1831.—Unusual thickness of the cranium; the fibrous tissue of the dura mater, when separated from the internal table, was found covered with blood. About four ounces of serous effusion into the cavity of the arachnoid; dry, lardaceous, thickened condition of the arachnoid, from albuminous exudation, like the buffy coat of inflamed blood, over the greater part of the convolutions, but more remarkable over the intergyral spaces. Remarkably clouded and thickened condition, from albuminous deposit on its inner surface, of that portion of the arachnoid unconnected with the pia mater, and situated behind the fourth ventricle and commissure of the optic nerves; also at the fissure of Sylvius. General adhesion of the pia mater to the cortical substance, by which the whole outer layer, on raising the membrane, was peeled from the inner layer. Cortical substance soft; the torn surfaces bleeding, and pulpy, like rotten fruit; general dilatation of the vessels of the cortical substance, with increased tenacity of their coats. Medullary substance exhibited a sensible increase of density, particularly in both the hippocampi majores; about one ounce of pure serum

within the ventricles. Arteries of the pia mater gorged with blood, and varicose; the capillaries could be distinctly traced. Cerebellum very soft, and having a small portion of the common trunk of medullary substance, immediately surrounding the corpora dentata, in a state of complete ramollissement; pia mater a little thickened. The other cavities were not inspected.

CASE VII. Old.—Margaret Johnson, ætat. 51, died Sept. 20, 1831. Prominent symptoms, referrible to the mental and corporeal functions, were general dropsy; marasmus; tonic contractions of the lower extremities six weeks before death; complete fatuity; orthopnœa; and frequent dry cough.

Autopsy, Sept. 21, 1831.—Very minute lenticular eminences, numerous, and of a soft albuminous consistence, disseminated over the visceral arachnoid, and connecting it with the arachnoid of the dura mater; about eight ounces of serous effusion into the cavity of the arachnoid; white thickened condition of the arachnoid in the intervals of the convolutions, and over the great fissures. Adhesion of the pia mater to the cortical substance confined to a few points on the convexities of the convolutions, where the cortical substance was elevated with, and remained intimately attached to the membrane; cortical substance remarkably pale, thin, firm, and almost of the same colour as the medullary substance, and shewing its three layers very distinctly, except at the adherences just mentioned, where the pulp was of the colour of wine lees. Medullary substance very white and firm. White pulpy ramollissement of the septum lucidum and fornix, of the lower part of the corpus callosum, and of the cerebral matter surrounding the ventricles. With the exception of the corpora striata, the whole surface of the lateral ventricles was in a state of white ramollissement. The left thalamus was reduced to a greyish pulp, to the depth of an eighth of an inch; the surface of the right thalamus was also similarly disorganized. Arteries and veins of the brain, and sinuses of the dura mater, remarkably empty. Pia mater thickened and finely injected.

Thorax.—The inferior lobes of both lungs exhibited the induration of chronic pneumonia.

Abdomen.—The liver was in a state of simple ramollissement, of a natural colour, and, at its convex surface, pre-

sented extensive separation of its peritoneal coat, and several deep, ragged, irregular fissures, apparently from loss of substance caused by absorption. About four pints of pure serum were found in the cavity of the thorax, and about as much in that of the abdomen.

CASE VIII. Old.—Ann Stanfield, æt. 42, died September 23, 1831. Prominent symptoms referable to the mental and corporeal functions were automatic life; constant recumbent posture; ulceration of the sacrum; complete fatuity; frequent screaming; sensibility doubtful; constant agitation.

Autopsy, September 24, 1831.—Cranium very thick. Minute lenticular eminences, of greater size than usual, disseminated in great abundance over the visceral arachnoid, and connecting it with the arachnoid of the dura mater. About six ounces of serous effusion into the cavity of the arachnoid. Remarkably white, thickened, mucous condition of the arachnoid, from subarachnoid effusion diffused over both hemispheres, both at the convexities and base. General adhesion of the pia mater to the cortical substance. Cortical substance soft and reddish in many points; its surface bloody and marbled red; half its thickness torn up with the pia mater; the torn surfaces red and bloody, and in a state of ramollissement. Great dilatation of the vessels of the cortical substance, and increased tenacity of their coats. Medullary substance infiltrated with serosity. The arachnoid of the ventricles was slightly reddish, and had a downy, villous appearance. White pulpy ramollissement of the ventricular surface of the corpus callosum. Velum interpositum adhering intimately to both thalami, corpora quadrigemina, and posterior commissure. A portion of brain, about the size of a garden pea, in the centre of the left corpus geniculatum, in a state of brownish ramollissement. Slight sanguineous infiltration of the pia mater, particularly over the upper surface of the right and middle lobes.

The other cavities were not inspected.

CASE IX. Old.—Charles Dennis, æt. 55, died October 6, 1831. Prominent symptoms referable to the mental and corporeal functions, were great diminution of the locomotive powers; occasional convulsive attacks resembling epilepsy; permanent tonic contractions of the limbs a few weeks before death; marasmus; complete fatuity; automa-

tic life; occasional excitement, with screaming.

Autopsy, October 7, 1831.—Unusual development of the crista galli of the æthmoidal bone. From the right posterior clinoid process of the sphenoidal bone, there arose an exostosis of nearly half an inch in length, and terminating in a sharp ragged point, the pressure of which had greatly thickened that portion of the arachnoid with which it was in contact. Lenticular eminences disseminated over the visceral arachnoid of the dura mater, particularly at the mesial eminences of the hemispheres. Here and there, albuminous deposits, of the consistence of jelly, developed in the tissue of the arachnoid. Considerable œdema of the arachnoid. Complete adhesion of the pia mater to the cortical substance of a few of the convolutions of the convexities of the brain, but throughout the membrane was raised with difficulty. Cortical substance red and soft in its whole depth, and bloody at its surface. Unusual dilatation of the vessels of the cortical substance. Medullary substance highly injected, particularly the centrum ovale, which was remarkably tough and fibrous: it represented throughout, but particularly where it surrounded the posterior cornua of the lateral ventricles, numerous red threads; these were vessels gorged with blood. About four ounces of serous effusion within the ventricles. White pulpy ramollissement of the septum lucidum. Ramiform redness of the pia mater over the convolutions, and diffused redness, with thickening, between the convolutions.

Thorax.—The heart was remarkably flaccid, withered, and soft, so as to be easily torn, and had undergone such diminution in size as scarcely to equal that of a sheep. Its whole apex was converted into an uniform mass of cartilage, as well as those columnæ of the left ventricle, which were immediately attached to the mass.

Abdomen.—The liver was of a white, or rather ash colour, and presented the character of pale degeneration, with slight softening of its texture, but without any alteration in size.*

[To be continued.]

NARCOTICS IN MERCURIAL IRRITATION.

To the Editor of the London Medical Gazette.

SIR,

If the following case deserve any notice, as shewing the influence of narcotics in mercurial irritation, it is at your disposal.

A gentleman, about 26 years of age, placed himself under my care in July last. He stated that, three months previously, he had a primary venereal ulcer on the prepuce, which disappeared in about a week, under the use of strong purgatives and the application of caustic. His symptoms were, a deep ulcer in the posterior part of the right tonsil, with slight difficulty of deglutition. No spot or eruptions on the surface of the body. The appearance of the ulcer indicating the nature of the disease, I directed small doses of the Hydrar. cum creta, with the extract of Hyosciamus, night and morning; put him on the antiphlogistic plan; and enjoined quietness and repose. In about ten days the difficulty of deglutition was overcome, and the ulcer had cast off its white slough, and exhibited a healthy granulating base. I was now suddenly called into the country, where I was forced to remain for some time, but previous to my leaving town I gave him such instructions as I thought his case required. On my return to town he again called on me, so altered in his looks and person that I scarcely knew him. Some short time before, I left him robust, florid, and active; I found him now exsanguined, emaciated, and a cripple. On inquiring, I learned from him, that, dissatisfied with the slow progress my medicine was making, and the recollection of the rapid cure of the primary ulcer being still fresh in his memory, he applied to another medical man, who ordered him to take the oxymercuriate of mercury in pills night and morning, and to use a gargle (the lotio syphilitica lutea of Swiedaur) repeatedly. Under this system he continued nearly a month, the ulcer all this time extending, until it occupied the whole posterior part of the pharynx: he now applied to another medical gentleman, who pronounced the ulcer to be phagedenic, and which, most probably, would compromise life. He was directed to use mercurial fumigations, and to suspend all other forms of mercury, to take anodyne draughts

* In the first part of Mr. Davidson's paper, page 668, for "the anus may still be capable of action, &c." read "the arms may still, &c."

and sarsaparilla. In a fortnight the slough cleared, leaving a deep excavation. His complaints at present were, pains in every part of his body—"from the crown of his head to the sole of his foot"—which completely deprived him of sleep and appetite; difficulty of deglutition; the right knee enormously swelled; the joint immovable; and the tibia bent at right angles on the femur. The size of the knee, together with the pain and general debility, alarmed me for the result, and I advised him to consult an eminent surgeon, which he declined doing, upon which I directed leeching and cupping to a great extent, with the lotio liquoris plumbi externally; for I found all warm applications, even the hot bath, increase the irritation; enjoined quietness, and put him on the use of a combination of extract of conium, Dover's powder, and extract of hyosciamus night and morning, and ordered the compound decoction of sarsaparilla, which latter disturbed his digestion so much that I was obliged to give it up, and confine myself to the conium, hyosc. and Dover's powders internally, and stimulating friction externally. Under this treatment he rapidly improved; his nights became better, his appetite returned, and the swelling and pain of the knee gradually diminished. For two months he adhered to this course, with the exception of increasing the dose as the habit became used to it, until he took a scruple of conium, with a proportionate quantity of Dover's powder and hyosciamus, twice a day. His general health is now quite restored, and the local affection of the knee seems perfectly cured. It is singular that the disposition to sleep was not increased beyond the natural limits, that there was no tendency to vertigo, and that the bowels were moved by the mildest aperients. Towards the close of the case, I found the warm bath agree well. Whether the little sarsaparilla he took could have any effect on the disease I cannot say, but with that exception, he took no other medicines than those mentioned above. That the disease is perfectly and radically cured I will not say—time alone can prove that; but the general state of his health inclines me to hope so. I shall offer no opinion on the rationale of the practice, conscious that any explanation I could give would be unsatisfactory.

I remain your humble servant,

RICHARD BURKE.

4, Bolton Row, May Fair,
23d Jan. 1832.

ON HÆMORRHAGE.

To the Editor of the London Medical Gazette.

SIR,

In your last number is a copious extract from a short paper of mine upon the employment of styptics in hæmorrhage from large arteries, which it might appear superfluous, to those who are acquainted with Dr. Jones's work, to have written at all. But so many persons, who ought to have known better the principles by which hæmorrhage is controlled, were deceived by the supposed efficacy of the styptic of MM. Talrich and Halmagrande, that I was induced to make the experiments which you have related, in order that the pupils of St. George's Hospital, at least, might be better informed upon the subject, and be convinced by their own observation; and I observe, by the Bull. des Sc. Med. that our continental neighbours have been still more recently occupied in considering the claims of a fresh styptic, composed of charcoal, resin, and gum-arabic, to the confidence of French Surgeons. It may, perhaps, be interesting to those who are not convinced, by my experiments, of the small share of credit that styptics can claim in controlling hæmorrhage from large arteries, to be acquainted with the following summary of Dr. Jones's experiments, in which no styptic was employed; the result of which induced me to perform my own, although of a different kind, with perfect confidence in their result. His experiments were performed upon the carotid, axillary, femoral, and humeral arteries of horses, asses, and dogs.

1. Partial wounds, 22; of which lived 19; died of secondary hæmorrhage, 3.

2. Secondary hæmorrhage occurred in 5;—lived, 2; died, 3.

3. The kind of wounds were—

		Lived.	Died.
Longitudinal wounds.....	2	2	none.
Oblique wounds.....	3	2	1
Transverse wounds.....	3	3	none.
Half-circumference cut ...	4	2	2
Considerable (kind not stated) ..	1	1	none.
Punctured ..	9	9	none.
	22	19	3

4. Died of secondary hæmorrhage, 3; one lived for 80 hours; one lived for 6 days; one lived for 25 days.

5. Dr. Jones also divided, or completely tore through, the same sized

arteries in nineteen experiments; tying the integuments in fifteen, and leaving the wound completely open in the other four.

Of these nineteen, thirteen lived; six died, or were killed just before they probably would have died.

Of the six that died, three were open wounds; in three the skin was closed by suture.

In four, the end of the artery nearest to the heart was tied; of which one died, the other three lived.

In fifteen, nothing was done to the artery; of which five died, ten lived.

The result is amply sufficient to prove that, in wounds of *large arteries*, by far the majority will be cured by very simple means, without the employment of any styptic, if the integuments are closed; and the experiments detailed in your last number, I think, prove the same point with regard to *open wounds*.

I am, sir,

Your obedient servant,

CÆSAR HAWKINS.

31, Half-Moon-Street,
Feb. 8, 1832.

MANIA FROM EXHAUSTION.

To the Editor of the London Medical Gazette.

SIR,

Dr. Marshall Hall, in commenting on the interesting case of mania from exhaustion by blood-letting, related, in the 12th Number of the Medical Gazette, (p. 419) observes, that he is "the first who traced puerperal mania to exhaustion as a principal, not the sole cause," and that the late Dr. Gooch had taken a similar view, giving him, Dr. H., the credit for the originality of the remark.

I am not solicitous for the honour of this discovery, had I a claim to it; for surely there would be little merit in one, having had as much experience as myself in puerperal insanity, marking so obvious a characteristic of the puerperal state, as exhaustion.

If my work on insanity, which had preceded both of Dr. Hall's and Dr. Gooch's publications on the diseases of women, be referred to, it will be seen, that I have mentioned mania, or delirium, to be a frequent consequence of excessive hæmorrhage, or of extreme

exhaustion, however produced*; and, in the introductory observations to the medical treatment of puerperal insanity, have premised, that "when a woman is recently delivered, she should be considered as a person who has sustained a profuse evacuation of another kind†."

It must not be understood, however, that I consider exhaustion so common a cause of puerperal mania as Dr. Hall seems to imply; and fearing that too hasty an adoption of such an opinion might lead to evil, I am induced to offer some brief observations on the subject.

As exhaustion is a natural concomitant of parturition, would not delirium be much more frequent among lying-in women, if exhaustion were a *principal* cause in producing it?

Mental aberration, or delirium, may come on at the epoch of conception—during pregnancy—on the commencement, during, or immediately after labour, whether attended by great hæmorrhage or little—whether the woman be in a plethoric or cachectic state—whether, in fact, the system has been much, or little, or not at all reduced.

Hence is it not to be inferred that there is something peculiar in the processes of gestation and parturition that disposes to mental aberration, independent of exhaustion?

A specific condition of the sexual organs has been supposed to exercise an influence; but there is no feature peculiar to puerperal insanity; any of the phenomena apparent in other forms of insanity may occur in this. Nymphomania is not oftener attendant on the puerperal woman than on the virgin.

I have found upon investigation, that above half of eighty lying-in women becoming delirious, had an hereditary predisposition to insanity.

As to the specific cause of insanity in lying-in women, therefore, we can conclude nothing, but that it is more apt to be developed where an hereditary predisposition obtains.

Dr. Hall's case, referred to above, admirably illustrates the possibility of inducing mania by profuse blood-letting, and I hope will prove a check against that indiscriminate depletion which I have known practised, for no other reason, that I could discern, but because delirium had supervened on delivery. It

* Commentaries on Insanity, Part 1, Comm. iv.

† Ibid. Part 2, Comm. vi.

shews, also, that this remedy, adopted to remove delirium, if pushed to excess, is likely to originate it.

As a general rule, I have insisted elsewhere*, and repeat it here, that puerperal insanity is not relieved by venesection: the only exceptions are, when true inflammation, or plethora threatening apoplexy, or inordinate action of the circulation exists.

To the too free use of the lancet I have ascribed the high degree of mortality, viz. above *one in six*—in the synoptical table† I have given of fifty seven cases which, in some stage or other of the malady, had come within my cognizance.

Dr. Gooch, equally impressed with myself with the importance of the inquiry—in what proportion puerperal insanity proves fatal—refers to my table, and states his persuasion, that its results “present a prospect unnecessarily gloomy and discouraging‡.”

With a want of candour resembling that to which Dr. Hall alludes in this author, he never mentions where this table is to be found: nay more, he draws inferences from it which I have in a special manner endeavoured to guard my readers against.

As a corollary to this table, I have subjoined, that “truth compels me to acknowledge, that I do *not* infer this malady to be naturally as mortal as the proportion of deaths recorded in it intimates§.” In the next page, I have even assigned the probable cause of such fatal effects, in these words: “in most of the cases, depletory measures had been pushed to an unreasonable extent; so that the issue was already perhaps determined before I was consulted, and no alternative left but death, or long continued insanity.”

Dr. Gooch is dead: christian charity, therefore, forbids my saying more than is requisite to correct the erroneous impressions his remarks on the results of my experience are calculated to produce. What other liberties he has taken with my opinions and treatment of puerperal insanity, without acknowledgment, may be seen by those who chuse to read, and compare our respective dissertations on this subject.

From what source soever the im-

provement proceeds, is immaterial; but I have subsequently had the satisfaction of finding that more enlightened views prevail respecting the treatment of puerperal insanity, and consequently that, comparatively, such cases are generally of shorter duration, and the recoveries more numerous.

I am, sir,
Your obedient servant,
G. MANN BURROWS.

64, Great Russell Street,
January, 1832.

ANATOMY BILL.

To the Editor of the London Medical Gazette.

SIR,

I WAS induced to put forward the “Observations” which you did me the favour to insert in your last number, from a conviction that the bill as it stands only palliates the mischiefs which it purposes to correct. The difficulty of all legislation lies chiefly in detail; and here that difficulty is increased by the admitted unsuitableness of the subject for discussion, repugnant as it is to the natural feelings, not to mention the prejudices, of mankind. But it is useless thus to treat the subject in the dark; I would have it fairly and boldly met, involving as it does the security of the public, and the credit of the profession.

The absolute indispensableness of anatomy is not now to be argued; the previous agitation of that question has given rise to some able expositions, which are doubtless familiar to your readers. I would especially mention an article first published, I think, in the *Westminster Review*, entitled, “The Use of the Dead to the Living,” and another in the *Quarterly*, attributed to the late Dr. Gooch.

Hitherto the pursuit of anatomy has been stealthy, like an ignominious calling, half checked and half encouraged—one of those anomalies which result from the conflict of man’s real necessities with his abstract likings; and hence the student has been punishable for providing himself with the means of obtaining knowledge, for the neglect of which he would have been punished as a practitioner. Discoveries have lately been made which call for the interven-

* Commentaries on Insanity, p. 403.

† Ibid. p. 391.

‡ An Account of Diseases peculiar to Women, p. 121.

§ Commentaries, pp. 398, 399.

tion of the law, not merely to punish a hideous crime, but to provide for each lighter shade of offence, and remove the temptation. How is this effected by the bill, which opens and sanctions a body market, leaving the price undefined, and subject to the fluctuations, real or fictitious, of all markets? It takes for granted that a plentiful supply will so reduce the price, as to extinguish the temptation to crime. Without questioning the axiom, that a plentiful supply will reduce the cost in this as in other markets, I assert that we are neither warranted by reason or experience to conclude, that a wretch so utterly depraved as deliberately to commit one murder for a sum of ten pounds, will hesitate to commit two, nay three, for the same sum. But, say the advocates for the bill, the certificate required with the body is sufficient security against murder. I reply, that it is in so far a safeguard, that in no instance should it be dispensed with; but taking the numberless instances in which persons of a certain class die in the absence of all medical attendance, the certificate will, in nineteen cases out of twenty, be a mere form; and even were the body opened, a process by which it would be spoiled for sale, the cause of death would often be conjectural. In short, the legal sanction now first granted by this bill to traffic in bodies, only unbars the door which was formerly forced. It is frightful to contemplate the possible effects of an act which renders the human body a legal tender; thus tempting the poor man, in a moment of desperation, to turn an evil eye upon his friendless and even poorer neighbour. Such is the monstrous result of a legal enactment, the great object of which is to prevent the repetition of the crime. Thus much as regards the public.

Now let me ask, how is the profession exonerated, by this bill, from the unmerited degradation of being compelled to carry on this odious traffic?

There are gentlemen, now living ornaments of their profession, who have been in former times obliged to act as their own purveyors, and to join in the perilous and ungrateful service of robbing graves for the means of education. This necessity, painful and disgusting as it was, was honourable in comparison with that to which we have been reduced by the systematic employment of resur-

rection men; whose daring monopoly of their craft, sharpened by its risks and the severity of its penalties, became so gainful as to plunge them into a reckless sensuality; the next step to which was midnight murder. How long the infamous practices of these men may have been perpetrating, to meet the increasing demands and secure the enormous prices paid by rival schools, we have no means of knowing; that the discovery is recent affords no proof that the crime is so. Crimes are committed in all great communities which escape the vigilance of the most active police, and of the existence of which many simple-minded persons would be incredulous, if they were disclosed.

Whatever other provisions may be thought advisable, the profession owe it to themselves to stand out for this—that *the trade in bodies shall henceforward cease, and be declared a violation of the law of the land.* It is an unfounded and gratuitous assumption, that if the direct stimulus of gain be taken away, the supply will fail: the dissecting-tables will be supplied with the same description and class of subjects as heretofore; for surely no one is silly enough to suppose that they have been, or will be, furnished by what are called the better classes of the community. The “unclaimed” dead are the proper, and more than sufficient source; and it is a maudlin sensibility which affects to lament over the condition that renders those useful after death who have been burthensome whilst living. Parishes bury their “unclaimed” dead at an average cost of three to four pounds for each body: let these be transferred to the schools, under the superintendence of the inspector, and the parochial assessment be reduced accordingly.

It is a sophistry to calculate upon any result from the philanthropic examples of A., B., or C., in high or middle life. Nothing will reconcile individuals, who have means and friends, to the custom of bequeathing their bodies for dissection. It may be vastly noble, but it is not natural, and originates in a spurious liberalism—the temper of the time. As society is constituted, there must be a diversity of lots; during life it is real and personal—after death, a reality only to survivors, and painful only to friends.

Strongly as I object to the sale and

purchase of bodies, I am no advocate for the gratuity of anatomical education; what is given, is rarely so prized, or turned to so much account, as what is purchased. Let the price of subjects be stipulated, and constitute a fund strictly appropriated to the salaries of inspectors, and fees to porters employed;—a moderate price will be sufficient to ensure the respectability and trustworthiness of such persons, allowing an inspector for each school.

I do not see how the provisions of this or any bill can be effectual for the security of the public and the credit of the profession, which does not require that dissection should be confined to licensed schools. How are inspectors to perform their duties unless thus circumscribed and defined? I have heard no rational objection to licenses. The alleged hardship upon persons advanced in life, or settled in the country, wishing to refresh their knowledge, is a pure delusion: persons so situated, and in earnest, may resort to the metropolitan or provincial school, and there gain what they desire with the greatest benefit and expedition. And, to obviate the annoyance of publicity, let the schools provide private rooms when they make the discovery of their necessity.

I am, sir,
Your obedient servant,
B. TRAVERS.

Bruton-Street, Feb. 7, 1832.

To the Editor of the London Medical Gazette.

SIR,

EVERY rational person must agree with Mr. Travers in the *general* principles upon which his observations, contained in your last number, are based; but I know not how far he is justified in urging so strongly the necessity of an enactment to restrict the practice of anatomy to *licensed* schools.

The object of such an enactment would evidently be to limit the number of schools; and yet I do not see, very clearly, how any law can be introduced so as, consistently with justice and impartiality, to prevent the establishment of new schools. It has always appeared to me, that an increased number of anatomical schools, or of establishments of

a similar kind, is, if it be an evil, one which would in a very short time remedy itself; and that more effectually than any legislative interference could do. The prerogative, at present very properly exercised by the College of Surgeons and Society of Apothecaries, of recognizing properly qualified teachers, enables them in effect to license schools, and to prevent their increase to an unnecessary extent. It gives those bodies all the *salutary* power which Mr. Travers desires should be possessed by some authority, while it in no way impedes anatomical study.

But it should be remembered that the number of schools must be always proportioned to the number of pupils, and not the latter to the former; or in other words, that an increase in the number of anatomical schools will not augment the number of students. Now the demand for subjects is in proportion to the number of students, whether those students be divided among ten classes, or all included within the precincts of one school: hence, were Mr. Travers to reduce the anatomical schools in London to three, the demand for subjects would not be in any degree diminished.

The proposal of Mr. Travers savours so much of that monopolizing spirit, which has already tended considerably to injure the interests of the anatomical schools of London, that I could not refrain from thus expressing sentiments in which I feel that I shall meet the concurrence of many well-wishers to the profession and science of medicine.

I have the honour to be, sir,
Your obedient servant,
R. B. TODD,

Lecturer on Anatomy and Physiology at the Medical School, Aldersgate-Street.

37, Great Russell-Street, Bloomsbury,
Wednesday, Feb. 8, 1832.

To the Editor of the London Medical Gazette.

SIR,

MAY I beg to intrude on your pages, to make a few remarks on the "Anatomy Bill" brought forward by Mr. Travers; inasmuch as I judge it expedient to call the attention of your readers (and more particularly of country surgeons) to the fact that while Mr. Travers, in his bill,

does away with the "money traffic" by the transfer of dead bodies, he at once renders practical surgery a monopoly, by cutting off all supply of subjects to the members of the College unless they have a licensed school; and however conversant a man may be with his anatomy when he leaves town, as time rolls on he becomes deficient, and incompetent to perform capital operations on the living unless he first execute such on the dead—a "hoon" most desirable, and one, of which (at this time) hospital surgeons do not fail to avail themselves. Now the second clause of Mr. Travers's bill altogether prohibits every kind of supply (save to the licensed schools), to the great injury of every practical surgeon residing in the country; I therefore hope you, as a friend to the profession, will guard against any thing like a monopoly, and endeavour to lay the path open to merit and industry.

I have the honour to be,
Your obedient humble servant,
W. R., Surgeon.

Grantham, February 8, 1832.

P.S.—I think the present bill, now before parliament, far preferable to the one suggested by Mr. Travers; as the transfer of dead bodies is absolutely necessary for the well-being of the profession.

FASTING.

THE author of a work, entitled "*Apologie du Jeûne*," published in Paris, in the year 1795, is a strong advocate for occasional fasting as one of the most certain means of invigorating and prolonging life. One of his arguments is this:—he takes 152 hermits or bishops (French bishops, it is to be presumed, *évêques*.) who are known to have lived a strictly temperate life, with frequent fasting; and he sets them against an equal number of academicians, half from the *Académie des Sciences* and half from the *Belles Lettres*. The joint lives of the hermits amounted to 11,589 years; those of the academicians only to 10,511. Hence he concludes that frequent fasting would prolong the lives of men of letters, in each individual case, by more than seven years, on an average.

ANALYSES & NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

The Cyclopædia of Practical Medicine. Part II. Feb. 1832.

THE early notice we have taken of the successive parts of this Dictionary shews the interest with which we regard its progress. We must confess, however, that the learned physicians by whom it is conducted, have conceived a different idea from ourselves with regard to the *beau idéal* of such a work. Some objections we ventured to make in reference to the first number; and as these were, in part, of a nature not to be remedied (the size and form of the volume), we did not, of course, expect to see any change in these respects; but there is one point to which, with every good feeling, we must seriously direct the attention of the Editors; namely, that however learned and elaborate the essays may be, they yet are not in keeping with the title which has been given to the work. It is not a *Cyclopædia of Practical Medicine*; for the proportion that the practical parts bear to the abstract physiological and theoretical portions, is exceedingly small. This, then, is one fault; another is, that a due balance is not struck between the articles as to length—some are lengthy and unimportant, others, which ought to be important, are short and meagre. It is, of course, within the province of the Editors to control this, and it is essential to the success of the work that they should do so.

Observations on the Medical Treatment of Insanity. By EDWARD J. SEYMOUR, M.D. 1832. pp. 95.

THIS unpretending little volume consists of three lectures, delivered before the College of Physicians last season. One of these was published in our present volume, because we regarded it as containing a very useful and perspicuous digest of what was known regarding "the application of medicine to mental disease," accompanied by numerous original observations. The other lectures, which precede the one alluded to, treat of the causes, pathology, and phenomena of insanity; and are constructed in the same manner as that with

which our readers are already acquainted—*ex uno disce omnes*.

—
Treatise on Cholera Asphyxia, or Epidemic Cholera, as it appeared in Asia, and more recently in Europe, &c. &c. By GEORGE HAMILTON BELL. Second Edition, very greatly enlarged.

IT is refreshing to turn from the numberless ephemeral productions on the subject of cholera to this really useful work, a second edition of which, it is creditable to the profession to find, has already been required. The history of the disease is here brought down to the latest period, and the various reports and documents which have appeared regarding it have been turned to due account. Differing as we do from the intelligent author on the subject of contagion, we are not the less anxious to bring his opinions before our readers; and we are quite sure that none of those who are influenced by our recommendation will regret either the time or money spent on Mr. Bell's Treatise. If we can find space in the present number, we shall give some of the cases of cholera as it has shewn itself at Haddington.

—
Annals of Sir Patrick Dun's Hospital. No. 1. Dublin, 1831.

THIS is the first Annual Report of the splendid hospital, founded by the munificence of Sir P. Dun. How it has happened that so excellent an institution should be without a medical historiographer so long, is more than we can well account for; but we are disposed to give much credit to the present physician in ordinary, for bestirring himself in getting up the work before us. Dr. Osborne has very properly prefixed to his report a sketch of the origin and progressive advancement of the hospital. Few readers, we fancy, on this side of the Channel, know much about Sir Patrick; and this is the more remarkable, as not much more than a century has elapsed since the worthy knight was gathered to his fathers. Like the patron saint, his namesake, he was a Scotchman; of the family of Dun of that ilk, near Montrose. In 1693, we find him president of the Irish College of Physicians; at the same time he occupied a high rank in

his profession, and was State physician for several years previous to his death. By his will he bequeathed lands, which then brought in an annual rent of 52*l*. for the support of one or two professors. And this is the friend from which the School of Physic in Ireland derives its present support;—but it should be added, that owing to the immense increase in the value of landed property in that country since the founder's death, the present income from the estates of Sir Patrick Dun is little short, of 2000*l*. per annum.

There are several curious cases recorded in the Report, of which we may avail ourselves at another opportunity.

—
Official Reports made to Government by Drs. RUSSELL and BARRY, on the Disease called Cholera Spasmodica, as observed by them during their Mission to Russia in 1831. With an Appendix, and other Papers, Extracts of Letters, Reports, and Communications received from the Continent, relating to that Disease.

THE greater portion of these documents have already been for some months before the readers of this journal; they commence with the first report made by the Board of Health, dated August 12, 1831, and conclude with a note from Drs. Russell and Barry, dated January 12, 1832. It is impossible to look over the mass of information thus collected into rather a formidable volume, without being impressed with the conviction that the objects of the mission were most amply fulfilled. The only circumstance which prevents us from extracting largely, is, that we have already, in a succession of numbers, transcribed the most interesting reports into our pages. In looking them over in their collected form, only two passages of importance strike us as either being new or as having formerly escaped our notice; these we shall probably give next week.

—
Journal of a Tour made in the Years 1828—1829, through Syria, Carniola, and Italy, whilst accompanying the late Sir Humphry Davy. By J. J. TOBIN, M.D.

DR. TOBIN was the amanuensis and private secretary of Sir Humphry

during the whole of that last continental tour which terminated so fatally at Geneva; and having kept a diary of the period, he has here given us an abstract of it, in the shape of a very interesting little volume. We should have hoped for more of the conversations, and flashes of the expiring genius, of our great philosopher; but Sir Humphry was broken up, and, what was still more unfortunate, but too conscious of the fact. He avoided company, and chose to spend much of his time in solitude and silence. Yet the habitually active and inquiring mind was not to be so satisfied: Dr. Tobin gives us many curious particulars of the literary relaxations of his illustrious friend. The morning was usually spent in dictating the "Consolations in Travel," or, as he at first called it, his "Vision;" the evening in listening to the perusal chiefly of works of fiction: and, it may be worth mentioning, that the last work he ever heard read was Smollett's *Humphry Clinker*. A little before his death, he derived exquisite pleasure from Sir Walter Scott's critique, in the *Quarterly*, on his "Salmonia." We wish Dr. Tobin had recorded some of these circumstances more at length; but as it is, interspersed with the topographical notices which form so considerable a portion of the volume, we can recommend it as a work of much merit and interest.

MEDICAL GAZETTE.

Saturday, February 11, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

IMPROVED PROSPECTS OF MEDICAL SCIENCE.

THE coldness with which the anatomy question was taken up lately by parliament, and of which we thought we had some reason to complain, has, we are glad to perceive, been exchanged for a mode of reception more cheering and satisfactory. Petitions have been presented from various quarters*, in favour

of a regulation of some sort, and conversations have arisen occasionally, which disclose the favourable feelings entertained on the subject by those members of the legislature who can be persuaded to speak out about it. The Cresset Pelhams and the Percevals have been shamed out of their wrong-headed opposition; but the member for Preston, who has no protective principle of delicacy in his nature, still goes on to blunder out every assertion he can think of, true or false, by which he can hope to retard the progress of bill. It is curious to observe the proceedings of both Hunt and Cobbett, the worthy pair of brethren, who, however they may be prepared to tear each other to pieces on other matters, are yet most ludicrously leagued, like sworn friends, to put down the *inhumanity* of anatomy! There are prejudices, no doubt, among the mass of the people, of which those gentry would be sorry not to avail themselves; though probably we should be wrong in attributing all their earnestness of opposition to this cause, while it may be owing, in as considerable a degree, to that sympathy which an equal proportion of unenlightened prejudice seldom fails to produce. Oh, the tender mercies of Peter Porcupine and Henry Hunt for the poor! There is the member for Middlesex, however, who probably is as good authority as either of these worthies, on what is likely to be for the benefit of the people, and what the people may be likely to take as a wholesome, though at first perhaps a bitter draught—even he has not thought it right or politic to remain silent when such an "inhuman" measure was before the House. Mr. Hume, in truth, could serve the profession much, and at the same time the people, were he only to exert himself often thus, when questions of vital interest to the health and physical wants of the nation are

* We regret that we have not room for the sensible lay petition from Leeds.

under the consideration of parliament. We want enlightened representatives of our profession in the House of Commons, and that want is every day more and more impressed upon us. That a day will come when such an obvious and due provision for the balancing of the different interests of the state, by the appointment of representatives equally chosen from the three learned professions, will be adopted, we have little doubt; but, meantime, many a measure "of great pith and moment" is lost, or treated with cold neglect, through a deficiency of such adequate representation. By the way, Sir James Mackintosh was once adorned with the bonnet of the faculty: has he fore-sworn us, or why have we never had the benefit of his eloquent advocacy in any discussion bearing upon the interests of medical science?

The Duke of Sussex has become now one of our warmest supporters. With a zeal worthy of the successor of Sloane and Pringle in the chair of the first scientific society in England, his Royal Highness has come forward and set an example that ought to have great weight in the removal of prejudice. His declaration lately on presenting the petition of the President, Fellows, and Commonalty of the Faculty of Physic, did honour to his well-known feelings of humanity, and could not have been delivered on a more opportune or solemn occasion; and the minuteness with which he subsequently dwelt on several minor topics of importance, while presenting the anatomy petition of the 388 students of St. Bartholomew's, proves what an anxious interest his Royal Highness takes in the successful issue of this difficult piece of legislative management. Other great names among our enlightened backers might be here set forth with honourable mention; but we shall take another opportunity of paying them due homage.

A word on a collateral topic and we

have done. The other night the honourable member for Preston having told a most absurd story, about an artificial subject of some sort being employed for years back in teaching anatomy in one of the Dublin schools (!), the Irish Solicitor-General was called on his legs to contradict it, and in doing so an important piece of intelligence was let drop. A pair of deputies, the learned gentleman informed us, had come over from the College of Surgeons in Ireland, to procure the extension of the new anatomy bill to that country. Perhaps it may be so: that certain deputies *have* come over, is quite true; but that their business is exactly what Mr. Crampton tells us, is a matter of belief too hard for us to credit. When we have ascertained that one, if not both, these worthy deputies are the same who last session performed such feats before the committee on the grand jury bill, and that the monopoly contemplated by that bill is *adhuc sub judice*, they will excuse us if we seem a little jealous of their movements, and incredulous as to the avowed design of their mission.

COMPARATIVE MORTALITY OF CHOLERA.

By the accounts received in London up to Feb. 10th, the following have been the actual numbers and rates of mortality at the places where the cholera has prevailed for a sufficient length of time to enable us to judge with any degree of accuracy of its comparative degrees of severity. Some places we omit from our calculation, because to speak of the fatality of a disease within two or three days after its first irruption, can only lead to vague, and probably erroneous conjecture. At Earsden, for instance, the number of cases reported is very large in proportion to the deaths; but to suppose this would prove the ultimate result, would be to form an extravagantly san-

guine expectation, inasmuch as a considerable number of those who remain ill, it is to be feared, will yet sink under the complaint.

The grand total of cases is 4146, and of deaths, 1250; but this, it will be understood by the preceding observations, does not enable us to calculate the rate of mortality, as many of the cases are very recent, and have not yet passed the period of danger. Taking places, however, where the disease is either expended, or has prevailed for a considerable time, we find that at Sunderland the numbers are—cases 536, deaths 202,—or rather less than one in three. Newcastle, cases 928, deaths 293; or considerably less than one in three. Gateshead, cases 402, deaths 144; or somewhat above one in three. At North Shields, cases 236, deaths 59. At Hetton, &c. cases 367, deaths 73; or nearly as one in five. At Wallsend, cases 32, deaths 3: and at Ellswick, cases 10, deaths 1; or nearly one in ten!

Again, in Scotland the rate of mortality becomes formidably increased. At Musselburgh, cases 362, deaths 153; or about one in two and a third. At Haddington, cases 112, deaths 55. At North Berwick, cases 15, deaths 6; and at Killingworth, cases 9, deaths 5; or about one death out of every two cases; while by a private letter from a distinguished practitioner in Glasgow, we learn (under date Feb. 6.) that at Kirkintilloch, “out of 30 malignant cases, 20 have died”—only one recovery in three cases; but this calculation does not include the mild cases. It is, however, quite obvious that a considerable difference has been exhibited in the fatality of the disease at different places, and it becomes a natural question—is this owing to any difference in treatment? We can state, from ample sources of information, that it does not; the treatment adopted during the last

two months having varied very little indeed in the places alluded to, and having almost exclusively consisted of emetics, moderate and cautious bleeding, calomel, occasionally purgatives, opiates and stimulants in limited quantity, clysters, either simply emollient or with opium, or with stimulants, such as oil of turpentine, and more recently with mustard. When to the above we add external heat and frictions, we have completed the list. Other means may in a few instances have been tried, by way of experiment, but certainly neither to an extent, nor with results which require to be taken into the account in speaking of the general rate of mortality. We subjoin a short account of the treatment adopted in Edinburgh, from the pen of an intelligent correspondent in that city.

CHOLERA AT EDINBURGH.

To the Editor of the London Medical Gazette.

SIR,

At present there is only one case in Edinburgh, and this may be pronounced convalescent. The patient, a woman, decidedly caught the disease from infection; was admitted into the Castle-hill Hospital in the second stage; was bled to $\bar{\text{xiij}}$.; afterwards took a mustard emetic; subsequently, during the day, grs. xxv. of calomel, with gr. j. of opium, were administered, and spiced wine, together with an enema of beef-tea. The second day vj . of calomel was given, and effervescing draughts; diet, arrow-root and wine. Flat tins, filled with boiling water, covered with flannel, and made to fit the body, were applied externally, and warm turpentine was rubbed on the extremities with much relief to the spasms. Another woman who was admitted into the Castle-hill Hospital, died four hours after admission. She was taken ill the previous evening at five; was bled to about $\bar{\text{viij}}$. prior to admission; the blood drawn was florid; she was in extreme collapse when taken in; pulseless;

and was treated with stimulants, external and internal.

Another case which was received into the Queensburg Hospital, had been removed from too great a distance and when in a state of too great collapse: she died soon after admission. The other three cases were also desperate, occurring in broken-down constitutions, one in a woman between sixty and seventy years old.

All the cases of cholera which have occurred at present in Edinburgh are distinctly traced to have arisen from infection.

I remain,
Your obedient servant,
J. KNAPP.

Edinburgh, Feb. 6, 1832.

DR. CLANNY'S DISCOVERIES.

IT is now several weeks since we announced the claims to certain "discoveries" connected with cholera, which had been advanced by Dr. Clanny, of Sunderland, and his liberal offer to communicate these for the benefit of his suffering countrymen—on receiving a due recompense from the government. From the commencement we exposed his conduct, and treated his pretensions with contempt; because we were able to demonstrate, by a reference to the official returns, that he had not diminished by one iota the rate of mortality, and because we regarded the proceeding alluded to as a most impudent piece of humbug, which imperatively required castigation, if not more severe, at least more public, than it received at the hands of the noble Lord in Downing-Street, to whom the Doctor's last appeal was addressed. Since our denunciation of this unprofessional attempt, it has been stated in the *Sunderland* newspaper—we will not venture to guess at whose instigation—that Dr. Clanny had received a letter from the council-office, thanking him for his exertions; but though the re-

ceipt of this document has but now been brought forward, and though it be intended to rebut by implication the statement in this journal, the fact is, that the letter of which he boasts was sent to him in November, and had no reference to the correspondence to which we alluded, and which did not take place till a later period. We repeat that Dr. Clanny wrote to the government, and endeavoured to make a bargain for the sale of certain discoveries which he alleged that he had made with regard to the treatment of cholera; that his first application was met with silent contempt, and that being put forward a second time, he received such a letter as effectually prevented him from repeating his solicitation. If we misrepresent the tenor of the correspondence, we challenge Dr. Clanny to publish it.

But Dr. Clanny persists in saying, that when we stated, on the 24th of December, that the rate of mortality at Sunderland remained nearly one in three, we made a "*lying accusation*;" and, in order to prove this, our courteous disputant throws aside the official documents which had constituted our data, and quotes the *Sunderland Herald*. Now by this it appears, that during the week from December 16th to the 22d (the latest day to which our information on the 24th could reach), there occurred—new cases, 50—*recoveries*, 32—*deaths*, 31! It is useless to argue with such an antagonist, who is evidently incapable of appreciating the force of his own statements. The only circumstance, indeed, which has induced us to recur to the subject again, is, that Dr. Clanny, finding nothing was to be made by the sale of his "discoveries," has been pleased to give them to the world. What admiration will strike the members of our profession when they learn that emetics, bleeding, opiate clysters, and warm

frictions, are the new and peculiar remedies recommended by Dr. Clanny! But new, and till now unheard of, as these discoveries may appear, another, not less curious, remains behind. Every one knows how troublesome is the purging in this complaint, and how even enemata are often speedily returned. This seems to have been the great subject of the Doctor's meditations, and constitutes the great triumph of his genius. Well can we imagine the exultation of his *heureka* when the bright idea struck him, amid his contemplations on the fundamental nature of the malady. Others had wasted their efforts in attempting to control the action of the bowels by various medicines; *he* resolved to go directly to the point—to *bung up his patients*, and fasten the plug with a bandage,—as we wire down the cork in a bottle of ginger beer! Such, in sober seriousness, is the only proposal in Dr. Clanny's mode of treatment which has the slightest claim to novelty, and therefore we presume that it formed the subject of his correspondence with the Lords of the Council. Blind and ignorant government! not to purchase such important novelties—not to reward so great a discoverer!

ROYAL INSTITUTION.

Friday, Jan. 27, 1832.

GEORGE MOORE, ESQ. F.S.A. VICE-PRES.
IN THE CHAIR.

Mr. Faraday on the Planariæ.

THIS evening, according to announcement, Mr. Faraday proceeded to lay before the literati assembled an account of Dr. R. Johnson's investigations into the *restorative, productive, and reproductive* powers of the Planariæ, a genus of small animals allied to the leech, and of which there are several known species, viz. *P. torva*, *lactea*, *hasitata*, *arethusa*, *felina*, &c. the three first of which are to be found abundantly in a pond near the Red-House, Battersea-fields.

From Dr. Johnson's experiments it ap-

pears, that if an incision be made longitudinally into the head of the animal, so as to separate its eyes from each other, if the cut has not been carried very far down, it will heal in the ordinary manner; but if the head be absolutely cleft in twain, then, according to the extent of the fissure, there will be a mass of new matter formed by each half of the head, which will either join the two halves together, forming a head of extraordinary size, and bearing in it one or two additional eyes; or each old half, thus cleft, will form the new matter into another half, with an eye, and so the animal have two complete and entire heads. If the fissure be carried farther down through the body of the animal, then not only will there be two heads, but two bodies also formed, joined together only by the tail, and when this is the case, so little unanimity does there exist between these *siamoid* twin-planariæ, that they never pull or swim the same way, and so violent are their efforts that they frequently, in the course of two or three days, tear the only remaining bond of union, their tail, in sunder, and then two distinct and perfect animals result.

If in a common planaria the head be cut entirely off, a new head will be formed; and if their lower extremity be removed, it will produce a new tail. In a planaria, which, by the operation above described, had been invested with two heads, these "*nova capita*" were successively severed for three several generations, and were immediately and perfectly renewed, and subsequently the animal was cut through just below the artificial bifurcation, and then only a single head was produced, so that in this more than simple "*capital*" operation, a single-headed animal became a biceps, and after having had the use of six heads in succession was subsequently reduced to the possession of a single one.

When one of these animals is cut in half, the head or anterior extremity swims away as if nothing had happened, and speedily re-tails itself; but the tail swims to the bottom, and remains torpid for two or three days, by which time it has formed for itself a head. If a planaria be cut into three pieces, the head will form a new body and tail, the tail a new body and head, and the middle section or body will produce both head and tail. If a quarter be removed by making a longitudinal incision through the head, and half down the body, and then a semi-transverse cut to remove the upper quarter, not only will the three remaining quarters speedily reproduce a new fourth, but also the separated fourth will form to itself three new quarters. Indeed a planaria has been cut into as many as ten pieces, and each piece has become an entire and perfect animal. In fact this mode of propagation, which physiologists artificially institute,

seems to be frequently resorted to by the animal itself. The planaria felina has been seen to throw off pieces of its body to form new animals, and these are not diseased but healthy parts, and not only parts of its tail, but often offsets from its sides, &c. Indeed, the planaria felina and *P. arethusa* have been never known to lay eggs, whilst the torra, lactea, &c. lay them in abundance, both the original animals, and those artificially produced. It would seem that those species which inhabit springs and running waters propagate only by division; but those which dwell in ponds and ditches, where the water is occasionally exhausted, are oviparous, as well as viviparous.

The above facts are physiologically curious, as they shew a still closer affinity than had previously been supposed to exist between the propagation of plants and animals by cuttings as well as seeds, for they have shewn that this mode of propagation can be carried to an almost equal extent in the one as in the other—an extent to which the experiments of Trembley and others on polypi, starfish &c. &c. did not reach.

On Friday, 3d of February, Mr. Griffiths gave some account of the Chemical Signs of the Ancients.

WESTMINSTER MEDICAL SOCIETY.

Saturday, February 4, 1832.

DR. SIGMOND IN THE CHAIR.

Dr. W. Philip's Paper—M. Azoux's Anatomical Model.

A good deal of conversation having taken place, at the two last meetings, on the subject of Dr. W. Philip's paper on Cholera, we think it right to insert the two following letters which were read by the Secretary, and by which the matter was set at rest to the satisfaction of the Society.

SIR,

At a meeting of the Westminster Medical Society, held January 28, 1832, the following resolution was agreed to, in consequence of an advertisement having appeared in the Times, &c. &c. stating that Observations on the Nature of Malignant Cholera, &c. were drawn up by Dr. W. Philip, at the request of the Westminster Medical Society.

Resolution.—That the Secretary be requested to write to Dr. W. Philip, to state to him, that, a misunderstanding having arisen as to his being requested to draw up a paper on cholera for the Society, an advertisement to that effect appearing in the public newspapers, he has to inform him

that it was understood that permission was given him only to submit the same to the Society.

EDWARD STODART, Secretary.

28, Jermyn Street, St. James's,
January 30, 1832.

To Dr. Wilson Philip, Cavendish-Square.

To this letter, Dr. W. Philip made the following reply:—

SIR,—I regret extremely that any misunderstanding on the subject of your letter should have arisen, and am wholly at a loss to comprehend how this has taken place; because one of the Presidents of the Society stated to me that he was desired, at a meeting of the Council of the Westminster Medical Society, to request me to state to the Society my opinion respecting the nature of malignant cholera; in consequence of which request alone, I drew up the observations presented to the Society. I viewed this request in no other light than as one which would have been made to any other person who had happened to have been engaged in the same course of experiments. On the same principle, I have received a similar request from the Professor of Anatomy, of Edinburgh—Dr. Monro.

Being unacquainted with the laws of the Society, I was not sure how far I was entitled to publish the observations in question without asking permission of the Society, and therefore inquired of the President whether it was necessary to request this permission. He said it was not. I therefore abstained from troubling the Society with a request which I supposed, from the nature of the Institution, to be unnecessary; and, in publishing my paper, I naturally referred to the circumstances from which alone it originated, and without which I had no intention of publishing any thing on the subject.

The last part of your letter I am at a loss to understand:—"He has to inform him, that it was understood that permission was given him only to submit the same to the Society." Now I can assure you, sir, that I never presumed to offer any of my opinions on the subject to the Westminster Medical Society, and therefore I do not see on what principle a permission to present them to the Society could have been voted.

I am, sir,

Your obedient servant,

A. P. W. PHILIP.

Cavendish Square, Jan. 31, 1832.

The attention of the Society was then directed to an ingenious model of the human figure, capable of being taken to pieces, and affording a curious and interesting specimen of mechanism. Mr. Azoux, the inventor, was present—Mr. Costello acting as his spokesman. Such

models may be looked upon as improvements on mere plates, and as calculated therefore to facilitate the acquisition of anatomical knowledge to a certain extent. The exhibition at the present time, however, is rather unfortunate, as it tends to confirm the popular error, that anatomy may be learnt without dissection. A good deal of complimentary *parlance* passed between the parties, which was rather fatiguing, and somewhat lessened the satisfaction which the model itself had excited. Reports, we perceive, have been got into the newspapers—the chief purpose, we presume, of the exhibition.

It is said that the model has been purchased for King's College.

ON DR. AUZOUX'S ARTIFICIAL OR "ELASTIC" ANATOMY.

By MARSHALL HALL, M.D. F.R.S.E. &c.

To the Editor of the London Medical Gazette.

SIR,

I THINK it important that the public attention should be drawn to the ingenious preparation of Dr. Auzoux for displaying the anatomy of the human body. Of these preparations may be said pretty nearly what Celsus observed of old in regard to the utility of philosophy in medicine*—although they will not make an anatomist, and far less a surgeon, yet they will materially assist in attaining and communicating anatomical knowledge. The student who goes from the preparation to his dissection, and runs from his dissection to the preparation, will obtain a knowledge far more readily, and even more correctly, in my opinion, than he who dissects by the aid, or without the aid, of a mere description, or even of plates. There is one point especially which is admirably taught by these preparations—viz. *relative position*. The different parts are made to retain their wonted magnitude; whereas, in actual dissection we see and touch parts which have lost their natural turgescence, and become flaccid for want of the living circulation, as is obvious in the face.

Such a preparation should then, I think, exist in every dissecting room, as an aid, not as a substitute, for dissection. It will form the best chart, the best guide for the young anatomist, and will greatly assist the demonstrator.—I am, sir,

Your obedient servant,
MARSHALL HALL.

14, Manchester-Square,
Feb. 9, 1832.

* — "Itaque ista quoque naturæ rerum contemplatio, quamvis non medicum faciat aptiorem tamen medicinæ reddit."—*Prof.*

CASES OF CHOLERA SUCCESSFULLY TREATED.

[From the Appendix to Mr. Bell's excellent Treatise on Cholera, a second and enlarged edition of which has just been published.]

Cases of Cholera Asphyxia treated at Huddington by Drs. Barton and Lorimer of Huddington, and Messrs. Meikle and Stevenson, Surgeons, Madras Establishment.

CASE I.—TREATMENT: Mustard Emetic—Laudanum and Sulphuric Ether—Sinapisms—Nitric Acid, Calomel, and Colocynth—Hot spiced Brandy and Water—Multigutany Soup.—RESULT: Recovery.

Margaret Thomson, ætat. 15, a delicate girl of spare habit, niece of the man Pearson who died this forenoon, and who occupied a closet adjoining her uncle's bed.

Dec. 31st.—Was affected this morning at six o'clock with slight sickness at stomach, followed by vomiting and purging of serous fluid; heat scarcely diminished; pulse strong. Some calomel and colocynth was given in the morning. At two p.m. she became much worse, and had slight spasms in the legs; she was now bled to fourteen ounces. The blood was dark-coloured, with scarcely any serum. Four p.m. she was visited by Dr. Morries, and Messrs. Meikle and Stevenson, from Edinburgh; found her tranquil, with neither vomiting nor purging. Pulse very feeble at the wrist; moderate heat of surface, which feels natural; countenance dejected; orbits surrounded with a dark circle, exhibiting that expression so characteristic of Asiatic cholera. Has no pain anywhere; is urgent in her demands for cold drinks. The evacuations were inspected and found of the colour of whey, with albuminous flakes interspersed; has passed no urine since the commencement of the attack. At the recommendation of Dr. Morries, a mustard emetic was given, and the following draught:—

Tinct. Opii, gr. xx.
Æther. Sulph. gr. xxx.
Aquæ, ℥j.

It was instantly rejected; another was given, which was retained ten minutes.

Tinct. opii, gr. xx., and calomel, gr. v. were given, and ordered to be repeated in half an hour. A mustard cataplasm applied to the epigastrium.

Seven p.m.—Appears rather worse; very restless, and tossing about the bed; spasms in the legs; frequent retching of white glairy fluid, and vomits every thing fluid that is given.

Tinct. Opii, gr. xxx.
Acid. Nitric. gr. x.

Aquæ, ℥ij. M. ft. haust. statim sumend. et repetatur si opus sit. Brandy and water for drink.

Nine P.M.—Much worse; constant jactitation and retching; spasms in the legs; pulse not perceptible at the wrist; at the middle of the arm it is 120, and small.

Enema ex Tinct. Opii, ʒij.

Aquæ, ʒiv.

Emp. Epispast. Epigastrio.

Calomel. gr. x.; Tinct. Opii, gt. xxx. in a tea spoonful of gruel. This latter and acid for drink.

1832. Jan. 1st, eight A.M.—Had no sleep, and has been vomiting frequently; had only one stool of a white milky colour; pulse now felt at the wrist, weak and tremulous, 120; skin moderately warm; expression of countenance more favourable.

Tinct. Opii, gt. xxx.; Aq. ʒj.

Enema purgans. Cal. gr. iiss.; Ext. Colocynth. gr. v.

Ten A.M.—Still vomiting, but improved in other respects; the injection was retained half an hour.

Repetantur Pilulæ; Ol. Ricin. ʒj.

One P.M.—Two stools, somewhat feculent and bilious; much better.

Repr. Enema.

3½ P.M.—Comatose; still retching; vomited the oil.

Seven P.M.—Three dark feculent stools; has had some sleep; pulse 115; still very weak; retching diminished.

Ten P.M.—Rather worse; much exhausted; circulation very languid, and skin of extremities cold; eyes suffused; blister of epigastrium was displaced, and did not rise.

To have a table spoonful of mulligatany and hot spiced brandy and water occasionally.

Ol. Ricin. ʒj.

Emp. Lytta Capiti, necnon Epigastrio.

Jan. 2d, nine A.M.—Much improved; heat diffused all over, but the pulse is feeble; three very dark bilious stools, and made urine last night.

3d.—Still doing well. From accounts up to the 5th instant, this girl was recovering.

CASE II.—TREATMENT: *Bleeding—Calomel—Opium—Castor Oil.*—RESULT: *Recovery.*

1831. Dec. 31st, seven P.M.—Dunbar, a shoemaker, residing in the same house as the former patient, has been affected since eleven A.M. with purging and pain of abdomen; great oppression at the præcordia; pulse 115, full and strong; skin hot.

V. S. ad ʒx.; Cal. gr. x.; Opii, gr. iiss.

Tinct. Opii, gt. l.

1832. Jan. 1st, eight A.M.—The bleeding produced a tendency to syncope, and the blood drawn appeared very dark, and without serum; one very dark pitchy stool this morning; much pain of abdomen below the umbilicus.

Ol. Ricini, ʒiss.

Ten A.M.—Much the same; to apply a blister to the belly if not relieved.

Calomel, gr. x.; Opii, gr. j.

Seven P.M.—Much better; pain of abdomen gone; pulse 102, softer; two or three very dark olive-coloured offensive stools.

Repr. Pilulæ.

Jan. 2d.—Much better this morning, and is now sitting up; two or three dark bilious stools.

Accounts from Haddington up to 5th represent this man doing well.

CASE III.—TREATMENT: *Bleeding—Calomel, Opium, and Hyoscyamus—Turpentine frictions.*—RESULT: *Recovery.*

James Wingate, ætat. 37, a nailer.

1832. Jan. 1st, eight P.M.—Complains of severe spasms of the legs, which seized him only half an hour ago; his hands are tremulous, with starting of the tendons of the arms; pulse 115, and strong; skin warm; reported to have been drunk last night, and this forenoon vomited once or twice some fluid described to resemble saliva, or the liquor he drank. Bowels said to be unaffected.

Venæsectio ad ʒxx.

Cal. gr. x.; Opii, gr. j.; Hyoscyam. iij.

Turpentine frictions to the legs.

Eleven P.M.—Spasms have ceased as well as the vomiting, and he seems composed; pulse 96; heat natural; a mustard emetic was given at the commencement, and is still retained.

2d.—Considerably better; no stool; has had no return of the spasms; pulse 88; heat natural.

Calomel and Jalap.

3d.—Improving.

5th.—Doing well.

CASE IV.—TREATMENT: *Opium, Whisky, and Brandy—Bleeding—Sinapisms—Camphor combined with Opium—Calomel and Extract of Colocynth—Cupping, and Blister to Abdomen.*—RESULT: *Recovery.*

Treated by Mr. George Steele.

Adam's Row, Parish of Newton.

Jan. 6th, two P.M.—Mrs. Ross, ætat. 35. Complains of severe pains in the bowels, coming on at short intervals, with contrac-

tion of the muscles at the umbilicus; has been vomiting a fluid, resembling in its appearance barley-water, and evacuating per anum a fluid more nearly resembling perfectly pure water, with a small quantity of mucus diffused through it; complains also of sickness and vertigo, with feeling of weight and burning heat at the præcordia; severe spasms in the feet, legs, thighs, hips, and hands; great prostration of strength, with thirst, and an urgent desire for cold water. Features sunk, livid, and death-like; eyes dim and heavy; hands and feet cold; the other parts of the body not cold, but considerably below the natural warmth. The mouth inside is warm, but the breath is cool—nearly cold; respiration unaffected. Pulse 116, very small, at the wrist scarcely perceptible. Tongue white and moist. Has voided no urine since the attack came on.

For the last eight days she has been troubled occasionally with diarrhœa, accompanied with pains in the bowels: last night, however, on going to bed, she felt perfectly well, but was awoke at four this morning with griping pains in the belly. She arose from bed at six, for the purpose of commencing her daily domestic labours, and felt a call to go to stool; the evacuation was copious and natural, but rather loose. Before having time to dress, she became so sick, that she was under the necessity of immediately returning to bed; her feet, legs, and hands now became cold, and affected with spasms; she had another alvine evacuation about two hours after the first, which was dark-coloured and watery, and since that time has had constant sickness, with vomiting and purging of the peculiar watery fluid, as stated above—the spasms at same time becoming more severe, and extending to the thighs and hips. She had, about an hour ago, an opium pill of a grain and a half, with a glass of whisky, and at present feels rather easier.

I immediately removed about fourteen ounces of blood from her arm, which was all that could be got away, and even that with much difficulty. The blood was thick and dark-coloured; and during its flow she became very sick, and vomited about eight ounces of the whitish watery fluid before mentioned. Her pulse at the same time sunk, and could not be felt at the wrist. When the retching had subsided, she swallowed a pill, consisting of two grains of camphor and half a grain of opium, washing it down with half a glass of brandy mixed with water; was ordered to repeat the same every half hour; to apply a large sinapi-m to the belly, with bottles filled with warm water to the feet, legs, and other parts of the body.

Five p.m.—She has had no evacuations per anum since last report, and has vomited but little; what has been ejected from the stomach, however, has still the same ap-

pearance, but is imbued with something of a brownish colour, probably the brandy or dissolved opium. Has taken six pills; has had severe cramps confined to the feet, with constant sickness and thirst; feet and hands are now warm; pulse a little improved.

Ordered to take one pill every hour.

Nine p.m.—Three pills taken; has vomited only once since last visit, which was about two hours ago; felt a call to evacuate the bowels, but without effect; no urine; some cramps occasionally in the toes; complains much of sickness, thirst, and headaches. Lips blue, eyes sunk, and whole appearance of countenance *very cadaverous*. Pulse 116, a little stronger. Surface of body warm and clammy.

She was again bled to about nine ounces, when she became sick, as in the morning; her pulse *fell*, and the flow of blood stopped. Blood still thick and dark-coloured.

Pills and brandy to be continued.

7th, ten a.m.—At one in the morning she had taken six of the camphor and opium pills as above, when they were discontinued, and one-half ounce of the mist. camphor, with five drops laudanum and a little oil of cloves given every hour. She is now improved in her appearance; has had no spasms; body of natural warmth, and covered with a clammy perspiration. Pulse 112, soft, and of natural strength; tongue white; has vomited none, nor has she had any alvine discharge. Took some tea and biscuit for breakfast; she is at present giving suck, but the breasts have become quite flaccid. Complains of no pain except some degree of headache, which she describes as not very severe.

Intermitt. Mist. Camphor.

R Submur. Hydrarg. gr. xxx.

Ext. Colocynth. gr. x.

Syrup. simp. q. s. ut ft. mass. in Pil. x. dividend; quarum sumat i. omni hora.

Seven p.m.—Continues to improve; countenance resuming its natural appearance, but seems a little tinged. Only three of the pills taken, and no evacuation either upwards or downwards; voided about fourteen ounces of high-coloured urine at eleven a.m., and again a smaller quantity in the afternoon. Had beef tea for dinner, and tea and biscuit in the evening.

8th, ten a.m.—All the pills taken; has been vomiting and purging copiously since six this morning; the fluid ejected from the stomach is tenacious, and of a dark yellowish-brown colour; that per anum is stated to have been "black and green," and in smell highly offensive. She complains of pains in the epigastric and umbilical regions. Countenance anxious and clammy; eyes heavy; conjunctiva of a yellowish tinge. Skin of

natural warmth, dry; pulse 132, thready; tongue brown and moist; voids urine occasionally. States that on the afternoon of yesterday her milk was abundant, but that to-day it flows less freely.

Eight p.m.—Has had several very dark bilious evacuations from the bowels; and twice since the morning visit an attack of retching, with the discharge of a little frothy mucus. Urine flows freely; breasts flaccid; pulse 138, thready; tongue whitish, moist; thirst urgent. Complains of considerable pain in the epigastric and lower part of the right hypochondriac regions, where may be felt a diffused hardness, tender to the touch. She also feels it uneasy when she moves her body, and lies easiest on the left side. Appearance of countenance as in the morning; head uneasy, but not pained. Has had hiccup twice during the afternoon, and had an attack of it at the time of visit.

9th, ten a.m.—Countenance resuming its natural appearance and expression. Complains principally of weakness, thirst, and the abdominal tenderness. No appetite for food; pulse 128, soft; tongue whitish; slept occasionally during the night. No vomiting; two dark-coloured alvine evacuations, bilious, but not feculent; urine voided in much the same quantity as when in health, and of same appearance; has no milk in the right breast.

Eight p.m.—Was cupped this afternoon over the pained part of the belly, but with little success, not more than two ounces of blood having been got away; pain still continues; pulse 112, soft and weak; no alvine evacuation; other symptoms as in the morning.

Applicet. Emp. Lyttæ parti dolenti abdom.

13th.—She is now nearly well, debility being her only complaint.

NOTE FROM DR. LINDSEY.

To the Editor of the London Medical Gazette.

London, Feb. 8, 1832.

SIR,

You will oblige me by giving insertion in the next number of your journal to the following remarks, in reference to a letter from Dr. Haslewood, Physician to the Sunderland Infirmary, published in the Medical Gazette of the 21st January.

With regard to the remark of Dr. Haslewood respecting mustard emetics, it is only necessary for me to observe, that in my communication of the 7th January I stated, "it had been recommended by Dr. Gibson previous to my arrival."

Referring to Dr. Haslewood's interpretation of that part of my report which speaks of the preliminary diarrhœa, I beg to disclaim any allusion to the profession in Sun-

derland, and to express my surprise that a meaning has been attached to my observations which was not intended by me; the paragraph, in reality, being an allusion to the neglect of the unfortunate patients themselves, in not reporting the preliminary symptoms.

I feel pleasure in acknowledging that I obtained much useful information relative to the disease from Dr. Brown, and I perfectly recollect his having, soon after my arrival in Sunderland, pointed out to me the importance of attending to the early diarrhœa, and which I had afterwards ample opportunities of ascertaining. I also feel grateful to the profession in Sunderland generally, for the facilities afforded me of seeing the disease prevailing there, and in some cases of practising myself.

Your obedient servant,

OWEN LINDSEY, M.D.

DR. COLLIER'S CELSUS.

We have received a note from Dr. Collier, thanking us for placing his Celsus first on the list of those noticed in a recent number, and informing us that the inaccuracies we alluded to, as appearing in some parts of the text, had been corrected in a subsequent edition, which he states that he sent us at the time of its publication. We beg to assure him, that, if it ever reached us, it has been mislaid,—a thing very possible, amid the number of books which we are in the habit of receiving.

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.

Feb. 1832.	THERMOMETER.	BAROMETER.
2	from 34 to 47	from 29.22 to 29.18
3	26 43	29.40 29.70
4	37 53	29.74 29.86
5	38 53	29.87 29.95
6	35 52	29.79 29.60
7	32 45	29.73 30.06
8	22 48	30.24 30.26

Wind variable, S.W. prevailing.

Except the 2d, 5th, and 6th, generally clear; rain at times on the 2d, 4th, and 6th.

Rain fallen, '1 of an inch.

CHARLES HENRY ADAMS.

NOTICE.

The Report from the Whitechapel Board of Health has come to hand; but as it contains nothing more than symptoms, treatment, &c. with which the public have long since been made familiar, we must decline publishing it.

W. WILSON, Printer, 57, Skinner-Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, FEBRUARY 18, 1832.

LECTURES

ON

THE THEORY AND PRACTICE OF
MEDICINE;

Delivered at the London University,

By DR. ELLIOTSON.

PART I — LECTURE XX.

Melanosis.

THE next new formation, gentlemen, of which I have to speak, is called *melanosis*, or *melanodes*, in which is deposited a mass of black substance, giving an unctuous smooth section, which is either uniformly dark-coloured, or has certain shades, so as to be mixed up with patches, or streaks, of paler coloured substance. Sometimes it is deposited in lobules, or a mass, and sometimes in plates. If you macerate a mass of this description, the black portion is separated, and this readily mixes with water, and stains the hand, linen, and paper, just like Indian ink. It has neither taste nor smell: I never tasted it myself, but this is said to be the case, and this is worthy of notice for considerations which I shall presently mention. In every respect it resembles the pigmentum nigrum of the eye, or the dye of the cuttle-fish; and the colouring matter of the hair, of the cutis of nerves, of some parts of the brain, bronchial glands, or ganglia of the lungs of old people, and the placenta of some carnivora, may be similar. When you have washed it out by water, the structure which is left after the separation of the black substance is circumscribed, and more or less firm. When I say circumscribed, I am supposing that you take a piece of an organ, that you wash the part that is healthy, and you wash out the black portion, and then there will remain a circumscribed, more or less firm, substance, different from the rest of the organ in which the black matter lay.

Although the character of this disease is blackness, yet you do not always have a decidedly black colour; there are shades of

brown, and even of a yellow hue, as well as the black. The masses, or depositions, are not only of all sizes, but also of all shades. Sometimes a black mass of this description is thought to soften down, but that must be a rare occurrence. You have seen that scirrhus softens down, that tubercles soften down; but this very rarely, if ever, softens. It is, of course, a totally inorganic substance. It is like a tubercle in this respect—that it never becomes organized; but it is unlike a tubercle, in rarely, if ever, softening. A dark-coloured fungus will sometimes arise from the part in which this black deposit has taken place, and so far it resembles fungus hæmatodes. The neighbouring glands will become affected, at least when I say the neighbouring glands, I mean the glands which are connected with the absorbents of the part. Sometimes we have melanoid tubera, or tubercles, using the word in its common acceptation,—melanoid deposition in remote organs, so that the disease appears like fungus hæmatodes or scirrhus, in a primary or a secondary form, beginning in one part, and being found afterwards, though not so advanced of course, in distant organs. There appears to be an analogy between this disease and fungus hæmatodes as to the organs which are primarily and secondarily affected: there is the same order observed with regard to both circumstances. You might sometimes commit a mistake when you see an absorbent gland, or a ganglion affected with this disease, and take it for a mass of melanotic substance. The absorbent glands frequently have this deposited in them, and become blackened, and of course it is a gland blackened with a secretion of melanotic matter, and not a real mass of new matter which is there. If you do not consider that the part which you examine is one in which an absorbent gland actually lies, you might sometimes mistake a mere blackened gland for a mass of this peculiar melanotic matter.

This is a disease which does not occur early in life, like fungus hæmatodes, but rare

ther resembles scirrhus in this respect, seldom occurring till the individual has passed the middle age. It has been seen, like the other deposits, in brutes, and particularly in the horse. Of all brutes, I understand, it is most frequently found in the horse; and it is said (you observe I do not speak from observation of my own, for I have not dissected many brutes, and I am not willing, of course, to intrude upon the province of other persons, but there can be no harm in speaking incidentally in this way, at least I presume not) it is said to occur most frequently in cream-coloured and spotted grey horses, and more often in horses in the south of Europe than here. But it is likewise seen in dogs, cats, rabbits, mice, and rats; so curious have some people been in the investigation of melanosis. It not only attacks so many creatures, but it is said to attack all parts of the body, chiefly, however, the lungs and the liver. So large are the masses sometimes in horses that they have occasionally been found to weigh thirty pounds in the abdomen. Dr. Halliday mentions one instance of most intense melanosis in the human subject, where it existed almost universally in the subcutaneous and inter-muscular cellular membrane—in the cellular membrane under the skin, and among the muscles—in the peritoneum also—in the pericardium, the pleura, the ovaria, the sternum, and the bones of the cranium—so extensive sometimes is this deposition.

Sometimes, though rarely, this substance—I do not use the word matter, because that conveys the idea of pus—is enclosed in a cyst. Most other deposits have a cyst, but it is only sometimes that this is found to be so enveloped. Occasionally, instead of being collected into a mass, it is completely diffused, not merely in small scales and plates, but generally diffused along a membrane, mucous or serous; and I presume, when this is the case, it really is deposited, not upon the membrane itself, but rather in the cellular membrane immediately underneath it. I should imagine so, because it is the cellular membrane that is liable to become cartilage and bone, and which is particularly liable to other diseases. This disease, too, is not unfrequently seen in new membranes of the body—I mean false membranes. When a serous membrane has been inflamed, or has had lymph deposited which has become organized, then the false membrane becomes cellular membrane, which I mentioned before, and these new false membranes are sometimes seen to contain melanoid deposit. However, sometimes it is said to be found on the surface of a serous membrane: it is right that I should mention I have not seen it, but it is said to have been found, not underneath the membrane, but lying on the surface. The parts around probably do not undergo

any change except softening. Around this black deposit the real natural parts of the body are found softened, but in general that is all, unless this disease co-exists with another; and it is a very common thing for it to co-exist with scirrhus, with fungus hæmatodes, and even with tubercles. Where, however, it exists alone—where it is conjoined with no other disease, it seems itself to occasion nothing more than a mere softening of the surrounding parts.

Occasionally this same substance would appear to exist in a fluid form; occasionally we have a cyst filled with a liquid perfectly black, in which no difference has been observed from the mass that I have now mentioned except that the one is liquid and the other solid. When this melanoid matter has been found in a fluid state, there is every reason to believe that it was formed originally fluid, and not softened down, because the usual course of the disease is, however long it may last, for the mass to remain solid. When we see black fluid contained in a cyst, we must generally conclude that this substance had been originally formed with a disposition to remain fluid.

Now we continually see upon a serous membrane,—indeed, one can hardly open many bodies without seeing upon certain serous membranes, particularly the peritoneum towards the pelvis, the uterus, the bladder, and the rectum,—spots of melanosis, that is to say, perfectly black spots, which appear to be quite analogous to this affection. These appear to do no harm; they are perfectly innocent deposits; and sometimes we see very extensive deposits locally, superficially, that is to say, diffused, and in persons who are known not to have suffered at all by such a circumstance.

It is seen frequently in mucous membranes after they have been injured. After a mucous membrane has ulcerated and healed, which is very common in the intestines, the cicatrix is frequently blackened more or less with a deposit of this description. The deposition sometimes, as I mentioned before, is not exactly black, but merely greyish, sometimes brownish, or even yellow, and this we see in the skin after an eruption—after ulceration of various kinds. After an eruption, the skin will sometimes remain black, but more frequently brownish; and after an ulcer, it is very common for the legs to remain of a dark colour, sometimes black, but more frequently of a brownish yellow. These, I presume, may be considered various shades of a similar deposit. The same is seen in the brain after paralysis from the effusion of blood into the substance. A deposition of this sort is sometimes seen in the mucous membrane of the intestines after chronic diarrhæa. Occasionally in the intestines you see points not so large as a scale, of black deposit, as if a very fine powder had been sprinkled in the finest possible way upon the surface of

the intestines; I do not know that it indicates any thing particular, but every now and then you will see such points extensively disseminated. When I first saw this, I imagined it was some dirt or soot, but I found that I could not wash it off. It is not any thing exactly upon the surface; you see it through the surface; but you cannot rub it off, unless you destroy the membrane itself. I have sometimes seen the centre of the tongue of a jet black, without an unhealthy state of the rest of the organ, and without sordes, or any dangerous symptom.

I think there is a very curious analogous circumstance to this. I have never seen it; but there are, perhaps, the greater part of twenty cases on record within my reading, and beyond it there may be far more, of white persons who have become black. A change will now and then take place over the whole of the skin, so that a white person shall become entirely black. I have seen portions of the arm become black, but I have never seen an extensive change throughout the body. Others suppose, and I coincide with them, that this is a melanotic disease on a very extended scale,—that melanoid substance is diffused solely upon the skin, and in the most extensive manner. When we consider that if the disease be formed in a mass, it always bears a resemblance to the pigmentum nigrum of the eye, we must suppose that it is nearly the same affection when a white person is converted into a black.

Melana.—Persons occasionally discharge from the stomach, or the intestines, a black liquid, and the discharge of this is called by the peculiar name, in old authors, of *melana*, the black disease. Now this stuff, when perfectly black, has, I believe, no smell; at any rate, when it happens to be discharged pure, without feces, it has no smell; and cases have been noticed in which a large collection of precisely similar black liquid matter has been found in the peritoneum. Some such cases are upon record. I know that Dr. Prout has seen two or three instances, but I have not seen any myself, in which the urine was perfectly black,—not bloody, but black: The black secretion of the bronchiae, in some people, is probably dependent on the same dye. When we come to consider yellow fever, we shall find that persons vomit stuff of a black colour, and which is called *black vomit*. It is found to be tasteless, so that some have indulged themselves in drinking it, others have put it into their eye to see if it were acrid, and others have made an extract of it and formed it into pills and taken several in the course of a day, and they all concur in stating, that it is just like this melanotic matter which I have spoken of as deposited in solid masses, tasteless, inodorous, and perfectly innocent, when taken into the stomach.

Now I believe with respect to the discharge from the intestines, you may have it

entirely black, or you may have it a little reddish, so that you observe a slight hue of red in the black; and you may have it of various kinds, down to absolute blood. I think that when these discharges take place from the alimentary canal, that at any rate they are blood, which has undergone a certain change. Persons are exceedingly exhausted by this black discharge; but as it comes away through the intestines, it produces no pain, in general no irritation, nothing but exhaustion. It is admirably remedied by small and frequent doses of oil of turpentine, just like hæmorrhage from these parts. It appears to be—and I believe we must consider black vomit the same—an exceedingly mild substance, as nearly like the blood as it can be constituted, excepting the difference in colour. What the change is that has been undergone, I do not of course pretend to know; but the black urine, the black discharge from the intestines, this blackness of the skin when a patient is converted into black, partially or generally, the blackness on the peritoneum, blackness of the mucous membranes, blackness of cicatrices in various parts of the body, and black masses and substances in various organs, all appear to be the same affection in different forms, and in different degrees, and it would appear, in many instances at least, to be merely blood which has undergone only a very slight change. As analysed from the horse, it consists of the elements of the blood—fibrine, albumen, &c., but nearly one-third is highly carbonized substance—probably altered cruor: every thing else is in far less proportion. In the cells of the ovaria we often see black substance—often blood; and all shades may be traced between the two, so that blood here appears changeable to this black substance.

Melanosis appears to be perfectly harmless, excepting from the quantity of the substance which is formed. There may be an inconvenience arising from its bulk, its pressure, and its hardness, and of course when it is discharged, in a liquid form, there may be exhaustion from its quantity; but independently of that, I believe that the disease is not one of any malignity: still it is to be remembered, that it is continually united with malignant disease,—continually in a mass of fungus hæmatodes, you will see black deposit. This is not to be wondered at when you consider, that in fungus hæmatodes there is blood effused into the different cells. It is also seen in scirrhus as well as encephaloid disease; and you continually see all these diseases mixed together in the same individual—scirrhus, melanosis, and encephaloid disease, in one mass; but I believe, when confined to itself, it is thought by the greater number of persons not to be a malignant disease.

Kirronosis.

There is another disease in which a sub-

stance is deposited in spots or patches in the substance of the viscera, or upon the skin or different membranes, of a yellow colour. A German author, Lobstein, says that he has seen various parts of a membrane so coloured, and he calls this *kironosis*; but it particularly seems to affect the membranes of the head, chest, and abdomen: and when jaundice occurs in young children, Andral imagines it is really this particular disease, and not true jaundice. I can give no opinion about it. There is a species of jaundice which occurs in young children, and which is speedily got rid of by castor oil, or would often cease spontaneously in a day or two if no measures were adopted; but besides that there is sometimes jaundice, the disposition to which is congenital, for many families have died of it in succession. There are many children who are said to have jaundice, but which is supposed to be an instance of this yellow disease. Laennec calls it *cirrosis*, and says it is deposited in masses, or layers, or sometimes in a cyst, such as we occasionally see in melanosis, and conceives that a hard tuberculated liver, such as is seen in gin drinkers, is an instance of this affection. I recollect having once seen the liver of a child, who had scrofulous tubercles of the lungs, contain a cyst filled with this peculiar yellow matter. It is a rare disease altogether, except the small brown tubercle of the liver be it, but of course it is right that I should mention it. I should imagine that it was an innocent affection; that the deposit was as innocent as that of melanosis.

When you find these new formations co-existing in the body, it is nothing more than a fact analogous to the circumstances I have stated respecting transformations. I mentioned that the various transformations sometimes co-exist in the same organ; that you will see cartilage in one part of an organ and bone in another; and so with respect to mere changes of size and consistence; you will find these co-exist in the same organ, so that one part of an organ will be indurated and another softened; one part hypertrophied—over-nourished, and another atrophied—wasted. You see the diseases of consistence, and size, and the diseases of transformations, all blended together, just as you see the diseases of new formations; and not only so, but you see in the same persons new formations, and diseases of change of consistence and size, continually blended together.

These diseases, gentlemen, of which I have now spoken, are those which may affect almost any part of the body, viz. inflammation, scrofula, changes of consistence and of size, transformations, and new formations. I have therefore spoken first of diseases which will affect any part. I do not mean to say, each of these diseases will affect any

one part, but they may be situated here or there.

Besides these, however, there are diseases which may be called *general*, not from their being common to any part of the body, so that they may affect one person to-day in one part, and another in a different part to-morrow, which is the case with inflammation, but there are some diseases which are general in another sense—they appear to affect the whole body together. Whether these general diseases have their origin in some one spot, I do not know; but, so far as I can observe, they exist more or less throughout the whole body. Now one of these is a disease called *bloodlessness*—*anæmia*, a very curious disease; and there is another called *scurvy*. I do not myself know whether these diseases spring from the state of any one part of the body more than another, for we see a person bloodless throughout, without any local affection that can be discovered. You see him become pale, blanched throughout, and excessively weak; and we cannot say that any one organ is labouring under disease rather than another. So in *scurvy*, the whole mass of blood, as well as the solids, is affected; so that these are general diseases in another sense, or we ought perhaps rather to say, *universal* diseases, those being properly general which are able to exist here or there. Those which I shall next speak of are therefore rather universal diseases; and perhaps the distinction *general* and *universal* would seem to be proper.

Again, there is another set of diseases which may be local, but which produce effects so universal, that if they have a local seat, it is at one spot in one patient, and another in another, and that is fever; and, indeed, I may say fevers, because there are various kinds of what is called *fever*, to say nothing of eruptive diseases. Many persons speak of fever, be it intermittent, remittent, or continued, as having a particular locality; but I do not think they have proved the correctness of their assertions; and according to our present state of knowledge—but of course ready to change my course when more knowledge is imparted to us—I shall prefer considering these also as universal diseases.

Besides those general and these universal diseases, there are other affections, which consist of a mere affection of function, so far as we know, in which there is no structural disease, at least in many cases. It is possible that all diseases may be more or less structural; some persons say so; but I cannot help believing that some diseases are entirely *functional*. With respect to diabetes, I have frequently opened persons who have died of it, and the disease appeared to be entirely functional. So in cases of insanity, no

disease of the brain has often been discovered, and it appears to have been entirely functional. Of course the disease is corporeal just the same, but still the word *corporeal* is not *structural*. A disease may be corporeal and yet merely functional, for there may be no change of structure.

There are other diseases, entirely *mechanical*. Hernia is, of course, disease, but it is a mechanical affection altogether. I mentioned that even fractures and luxations are classed in systems of nosology as diseases; but at any rate hernia is called disease, and that is entirely mechanical.

There are other diseases arising from another animal having taken up his abode with us; such is the case with worms in the alimentary canal, and the existence of various insects upon the surface of the body.

Thus you may have *general* diseases—that is to say, diseases attacking any one part; and you may have *universal* diseases—diseases which, according to our present state of knowledge, appear to be diffused throughout the whole body without any particular locality: again, we shall have to speak of diseases which are *functional*, affecting particular functions—not organic at all, but entirely functional, and affecting only special parts: (diabetes, as a disease of the kidney, can affect no other part than the kidney; insanity can affect no other portion than the brain: spasmodic asthma, also, is often a functional disease; I have opened persons who have died of or with the disease, and others have done the same, where no trace of disease could be found.) You may have *mechanical* affections likewise; and the existence of parasitic animals, *parasitical* diseases.

But I shall content myself with the general account already given of *inflammation* and *structural diseases*, and consider *functional*, *mechanical*, and *parasitical* diseases, not in general, but particularly only, when speaking of the diseases of each part of the body. Previously, however, I must give you an account of the *universal* diseases.

UNIVERSAL DISEASES.

I shall now proceed to speak of one of these universal diseases. I have no doubt that the fault resides in some one particular spot in the function of forming blood, but in our present state of knowledge we can only speak of it as an universal disease.

Anæmia.

The affection to which I shall now direct your attention is called *anæmia*, a word which explains itself—the want of blood; blood is present, but there is a great deficiency of it. When a person labours under this disease, we might state, *à priori*, what the symptoms must be. In the first place

there is great debility: in the next place the skin is of a deadly pale wax colour; soft; in the next place the white of the eye is bluish, the inner part of the mouth is colourless, the lips are exceedingly white, and even the tongue is pale; the pulse is about 80 in general, but exceedingly feeble, and very easily excited—the least stimulus, the least mental emotion, as well as corporeal movement, produces a great momentary acceleration; respiration is hurried also on the least exertion; the appetite is bad, and it is said that there is constant thirst; there is œdema of the legs at last; and, finally, sweating—sweating induced by the great debility. After death the colour is much the same as it was during life—the corpse is not paler than the living subject; for when this disease is intense persons are really corpses in appearance. So far, I think, one might before-hand pourtray the disease merely upon reflection, without ever having seen or read about it.

But in certain cases of this disease, the stools have been observed to be dark-coloured and fetid, the appetite to be bad, and almost every thing at last to be vomited.

Morbid Appearances.—When persons have been opened, there is universal internal paleness, softness, and want of blood, and more or less excess of fluid in the serous membranes; a fact corresponding with œdema of the legs, and with sweating in the last stages.

Treatment.—This disease, in this peculiarly marked form, occurred formerly, in France, to a great number of workmen in one particular gallery at a coal-pit situated at Anzain, near Valenciennes. The disease was preceded by tormina, retching, green stools, thirst, and wasting, for ten or twelve days; and then the affection appeared as I have now described it. It lasted for six or twelve months, and then ended in death. At length the proper treatment was discovered. You will find a description of this endemic disease (for so it might be called, from having been confined to a particular mine, and not only so, but to a particular gallery in a mine, as though it arose from some effluvia) given by Professor Halle. You will find a similar case described by Dr. Combe, in the first volume of the Edinburgh Medico-Chirurgical Transactions. The patient was a corn merchant's son, and no evident cause for the disease could be discovered. Mercury was given to the French patients, and did them a great deal of harm; for it is a state of the system in which mercury is very injurious. Opening one patient, and finding the internal vessels almost bloodless, they no longer gave mercury, but iron,—carbonate, or rather oxide, of iron, in considerable quantity, with opiates and tonics, and good food, and the patients got well. They exhibited signs of improvement

in the course of eight or ten days, and from that time the greater part, I believe, recovered. A similar occurrence took place at Dunkirk, and the treatment there was exactly the same—the exhibition of iron—and the patients recovered equally well. It was found, however, that relapse was very usual, and therefore that the remedy had to be continued for a considerable time. I believe the patients at Dunkirk were persons who had been sent from the mines to which I before referred; but the same treatment was adopted, and was followed by the same success. I must mention that Dr. Combe used both the sulphate and carbonate of iron, but he does not mention the dose nor the length of time it was given. Mercury also was exhibited, which appeared to be injurious in France, and a number of other drugs were exhibited; so that the iron did not appear to have a fair chance.

Chlorosis.

Now the state of chlorosis appears to be exceedingly similar. There is a disease common to young women, either about the time they ought to menstruate or soon after they have begun to do so, and which in them is called *chlorosis*. It occurs occasionally to persons more advanced, and a similar affection occurs also to men. It strictly is not chlorosis in men, because we say that one of the symptoms of chlorosis is an absence of the menses—the menses not appearing at the proper time of life, or retiring just after they have presented themselves; of course that is not the symptom of the disease in men, and therefore we cannot say to quibblers it is chlorosis when we see it in men; but less us consider things more than words—consider things as pathologists; and it is a fact that men will sometimes fall into a state of *anæmia*, bloodlessness, and debility, very similar in all appearance to that state which in women is called chlorosis.

Symptoms.—When a woman has this disease—and so common is it that you will see instances every day—there is a general paleness; there is a swelling of the face and ankles, weakness, a great sense of tension of the legs and feet; dyspnœa; palpitation; the pulse is either quick or easily rendered so; and of course there is a deficiency of menstruation. Now these are the symptoms of chlorosis mentioned by authors, and they are, for the most part, exactly the symptoms that occur in the disease called *anæmia*. I have no doubt the pathology or proximate cause of these two diseases is exceedingly similar. In women who have chlorosis, the alimentary canal is often very much disturbed, and that was the case with the Frenchmen whom I have mentioned.

In women labouring under chlorosis there is sometimes a great derangement of the sto-

mach, or, if the cause of hunger is cerebral, we must say a derangement of a mental feeling; they will long for all sorts of things—chalk, sealing-wax, brown paper—and they are not always the worse for eating them; their stomach manages them pretty well, there is such a departure from the natural state of things. Very frequently, too, they are costive. They will sometimes eat the most filthy things—things that one would never think any human being would touch, except cannibals. They will even take pleasure in offensive smells, and long to eat what they do eat in a privy. That, however, is an intense state of the affection.

Treatment.—Now I believe the best remedy for chlorosis is the same which was found useful in France, viz. iron. You will find no medicine whatever act so beneficially as iron. I am not sure that the carbonate of iron is better than any other preparation of that medicine, but with that I know you will generally succeed. It is a disease not to be treated by bleeding, or profuse purging; the bowels are only to be maintained in a regular state—you must not allow a collection of trash to be formed in them; but beyond that, purging does harm. All evacuations were found to do harm in the disease of *anæmia* in France; and I know from my own experience, which is necessarily very extensive in so common an affection as this, that iron is by far the best remedy. Persons will lose their puffiness under it, lose their difficulty of breathing, and will gain strength, and soon gain colour.

You will find an affection similar to this when I come to treat of diseases of the heart. Persons will have violent palpitation, and become pale, and the state is not one indicating bleeding and purging; in such a complaint as this I know they will be made worse by these things; but to the surprise of myself, and frequently of others, but very much to my own surprise when I first ventured on the practice, iron has remedied this morbid condition of the system to a great extent. It will not cure organic disease; but when the heart, and body at large, has thus become almost bloodless, and extreme faintness has been induced by it, so that you have a quick, irregular, and sharp pulse, you will find that the patient will experience the greatest mitigation from the exhibition of iron.

You will observe that there is no confusion in all this. These diseases, although they are not spoken of by authors as analogous affections, are all of the same family; there is a want of the production of blood, but wherein the defect consists, I do not know. In the cases which occurred in France, one would suppose that some deleterious substance affected the functions of the body. In chlorotic women no cause can frequently be discovered. In organic dis-

ease of the heart, patients will continually fall into a similar condition of the system to that which we observe in chlorosis, but with palpitation ten times greater than ever occurs in that affection. In various visceral diseases, especially of the spleen and stomach, anæmia occurs. In anæmia from loss of blood, not from deficient formation of it, iron also is one of the best remedies.

[At the conclusion of the lecture the learned professor exhibited three drawings, executed by Dr. Carswell; the first illustrative of melanosis of the brain; the second, of melanosis mixed with open cancer of the stomach; and the third, of melanosis situated in the interior of the intestines.]

CLINICAL LECTURE

ON

EFFUSION OF URINE,

Delivered at St. Bartholomew's Hospital,

By MR. EARLE.

MR. EARLE observed that the case of John Shepherd (Powel's ward) was very interesting in several respects, although the true nature of the affection under which he had laboured was not ascertained until after his death. Both the history the patient gave of his complaint, and the state he was in at the time of admission, (not many days before death) led every one to suppose he was suffering from the effects of extravasation of urine into the perineum. He stated that he had long laboured under a very bad stricture of the urethra, which caused him great pain and difficulty in passing his water, and occasionally produced almost a state of retention. A week before he came in, something seemed to have given way in the urethra: suddenly a hard lump had formed in the perineum, which had since burst. Such was the account he rendered of himself, adding, that since the above-named time some water had passed through the opening, and that his distressing symptoms had been much relieved. At the time of his admission, the whole perineum was in a foul sloughy state, emitting the most insupportable stench. The fæces passed involuntarily; and upon inquiry, it appeared he had some time back undergone an operation for fistula. The sloughing process had quite extended to the nates, and the sloughs were in the act of slow separation, so that all that could be done was to make a few incisions through them, in order to expedite their removal, and allow of the free exit of the urine and pus. This patient was an old man, of debilitated and worn-out appearance, and his strength evidently fast sinking. Attention was particularly directed to supporting his system, and poultices, with chloride of soda, were applied to the part. It was, however, of no

avail; he became gradually weaker and weaker, and died yesterday, worn out with the irritation caused by the separation of the sloughs. The postmortem examination proved that it was not a case of extravasation of urine at all. The urethra was found sound and healthy in its whole extent, with the exception of some enlargement of the third lobe of the prostate. The rectum was in a very diseased state, part of the internal lining in a state of ulceration, and from it projected large hæmorrhoidal excrescences, also in a state of ulceration. Numerous sinuous openings led from the gut towards the perineum, in various directions. The edges of the incision, which had been made some time back for the cure of the fistula, were united. In this case we must suppose that portions of feculent matter passed through some of these numerous openings into the perineum, and there excited the irritation, which proceeded on to sloughing of this part.

There being, however, other cases in the house, shewing the effects resulting from effusion of urine, Mr. Earle took this opportunity of expressing his opinions upon the subject generally, describing the usual seat of the disease, and nature of the affection, when connected with primary affection of the urethra. He next described some more rare cases of effusion in unusual situations; then cases of effusion from rupture of the urethra, from violence; and, lastly, cases of effusion complicated with fracture of the pelvis.

There are no cases in surgery, he observed, requiring more prompt or energetic treatment than those in which a sudden effusion of urine has taken place. In such cases you must act at once. There is no time for temporizing or consultation if you wish to rescue your patient from the disastrous circumstances attendant upon delaying the application of the proper means. Every moment is of consequence, for there is no fluid of the system, (excepting, perhaps, the bile) so irritating and so rapidly destructive to the parts, into which it may become effused, as the urine. It soon extends itself throughout the perineum and scrotum, distending them immensely, and in many instances the whole of these parts perish, and the testes are left exposed. No boundaries are set up to the extent of the mischief unless it be arrested by prompt and decisive surgical means. There is always in these cases the worst form of irritative fever present, and Mr. Earle knows of more than one case where the constitutional affection was treated as a typhus fever, the medical men being ignorant of, and not suspecting, the local source of irritation, which was extensive extravasation of urine.

The most frequent case met with arises from ulceration of an old stricture. The

patient, perhaps, has long been the subject of a very bad one, passing his water in a very diminished stream, or even only guttatim. Upon some occasion he takes a violent degree of exercise, or gives way to some excess in wine, or venery, and a fresh attack of inflammation is induced in the part, causing complete retention of urine: ulceration of some point of the urethra, either at or behind the stricture, takes place, and the urine becomes effused into the surrounding parts. In the first stage of this effusion a hard circumscribed tumor is felt in the perineum, which has appeared suddenly, and increased rapidly. There is also felt in the surrounding parts, upon slight examination being made, a peculiar crackling sensation, very like that produced by emphysema, especially about the scrotum, while the neighbouring cellular membrane, not yet infiltrated with urine, has become very œdematous.

The patient who has before this been suffering from retention of urine, as soon as this hard tumor appears is relieved of all the more urgent symptoms, and often expressed himself as feeling infinitely better. This arises from the ulceration in the urethra having given vent to the urine, and relieves the state of spasm of the parts, upon which so many of the distressing symptoms of stricture depend. We must, however, in all such cases act with equal decision, and if a painful hard tumor present itself in the perineum, we should immediately cut down upon it, although in many instances no fluctuation be perceptible. Upon making an incision into it, a quantity of urine and pus, of a most offensive description, will be discharged. It will often happen, however, that you may not be called to a case until it has made considerable progress. The parts may have been enormously distended, and sloughing to a great extent already taken place. These cases have a most terrific appearance, but even when in this way left entirely to the efforts of nature, it is surprising what she will eventually effect. Thus the whole scrotum and perineum have in many cases sloughed away, leaving exposed the testes, which, however, become covered with lymph, which granulates, and eventually cicatrizes. Indeed, these cases where so much mischief is at once effected, and the parts are cast off by the natural efforts, are frequently infinitely less difficult to manage than those in which the effects have been both less severe in extent, and more gradual, causing enlargement and induration of the integuments.

Occasionally, but very rarely, such effusion takes place into the corpus spongiosum of the urethra, without farther extending itself around, and thus forms a very formidable case, giving the penis a semi-erect appearance. Mr. Earle never himself met with a case of this description, but referred to an

instance in which sloughing of the whole urethra and glans penis took place, and the case terminated fatally.

Extravasation of urine may occur from violence done in the passage of bougies and catheters; and, considering the rough manner in which these operations are often effected, it is surprising this accident does not occur more frequently. In the case of strictures of long standing, there is generally more or less of previous inflammatory action from time to time, which in some measure forms a boundary to the extent of the mischief; but, of course, in sudden lacerations of the urethra no such effect could have been produced. In some cases the inflammatory action which precedes the effusion of urine has so limited its extent, by condensing the surrounding cellular tissue, that small abscesses, or pouches, containing pus and urine, are formed, which gradually make their way to the surface, if not opened. This recurs again and again, and thus numerous sinuses are formed, with great induration of the surrounding parts. This has happened in the case of Massey, a patient at present in Pitcairn, who has laboured for the last fifteen years under difficulty in passing his water, so that occasionally he is only enabled to void it guttatim. He says that six weeks ago pain and swelling attacked his scrotum, and that an abscess burst externally, through which water has since passed. The scrotum is very much enlarged and indurated, in that peculiar manner producible by the irritation of urine. Great induration exists also in the course of the urethra, and the smallest catheter can only be passed a very short distance down the canal. There are several sinuous openings leading from the part. The man states that this state of things has only existed six weeks; but, judging from the much-increased size of the scrotum, and its cartilaginous hardness, one would be inclined to suppose that it must be the production of many months, or years. Although in these cases there are usually many openings in the perineum, yet it generally happens that only one of these communicates directly with the urethra—a fact of great importance in their treatment: such opening being generally situated on the under side of the urethra. The parts thus indurated and altered in structure by the extravasated urine, are very frequently capable (by causing them to suppurate, and giving an unobstructed exit for the urine,) of being brought into a much softer and more pliable state, and thus rendering any future attempt at restoring the urethra much more easy of accomplishment. This was the case with John Boys, who was a patient in Baldwyn last year; he had a most enormously enlarged and indurated scrotum, being four or five times its proper size, nodulated, and quite altered from the natural structure of the parts, the testes be-

ing, as it were, buried within a deep mass, and all the surrounding parts were in a most hardened and diseased state. He passed his water out at half a dozen apertures in the perineum and scrotum, voiding none by the natural passage. The complaint was of five years' duration. The urethra, in a great part of its course, had become impervious. By incisions of large extent the urethra was laid open, and a catheter introduced; the parts healed around it; the huge masses of new growth about the scrotum lessened much; and the man left the hospital, being able to pass his water in a very free stream, which he had not been able to do for many years before. Mr. Earle, in the case of Massey, proposes laying open the urethra, at its obstructed portion, in the same manner; and has, as a preparatory step, made some free incisions through the indurated scrotum, in order—by inducing free suppuration, and giving as free an egress to the urine—to soften the parts, and reduce their bulk. If the operation were performed without first doing this, a much more severe and extensive one would be required; for the mere removal of this indurated state of parts, and obtaining a free exit for the urine from below, will often enable us to pass an instrument, which we could not do before. In all these cases there is, besides the actual obstruction, a degree of spasmodic irritation present; and thus we often find in cases of very bad stricture, requiring puncture of the bladder, the state of quietude afforded by the operation permits an instrument to be passed after a few days, which could not before be introduced. Moreover, we might very often cut down unnecessarily upon the anterior portion of the urethra, for generally the part affected is just at, or beyond, the bulbous portion; and therefore any operation higher up is unnecessary.

Sometimes abscesses form in the perineum, through which urine does not pass, and which, indeed, have no communication with the urethra, but which have been caused by the irritation of the disease in the urethra, just as abscesses form around diseased joints, without having any communication with their interior. However, after several successive formations of this kind, urine is found at last to flow from the abscess. Sometimes the abscess may burst internally, the discharge of the pus being preceded by great pain, and attended with great relief. Sooner or later these cases all terminate, if not properly treated, in troublesome fistulous openings, which sometimes extend themselves a long way around. There are some cases of old stricture, where ulceration and effusion take place without any increased difficulty of passing the water, or any other premonitory symptom. These cases come on in a very insidious manner. A gentleman, sub-

ject for many years to severe stricture, felt after a day's active hunting a severe pain at the lower part of the abdomen, which was accompanied by shivering. Shortly afterwards a swelling appeared in the groin, in the situation of common inguinal hernia, which was very painful. It was deemed to be a hernia, and after several attempts being made to reduce it, Mr. Earle was sent for to perform an operation for its relief. He found, however, that the patient had a free passage through his bowels. There was an erysipelatous redness about the tumor, which much resembled that which is so characteristic over the seat of effused urine; added to this there was a feeling of crepitation, and a good deal of surrounding œdema. These circumstances induced him to inquire into the state of the patient's urinary organs, who told him that he passed his water much better than he had done for many years previously, which confirmed Mr. E. in his suspicion of effusion of urine. The patient, however, refused to permit him to puncture the tumor. The next day it spontaneously opened, and urine was freely discharged from it. The case terminated very favourably, in consequence of Mr. E. passing a full-sized catheter, which was retained until the abscess granulated and healed. In this case the strictured part of the urethra had no doubt ulcerated. Such cases are very deceptive; the previous inflammatory action having prevented the urine from pursuing its ordinary course into the perineum and scrotum, it becomes extravasated backwards between the abdomen and bladder; and thus we may have it also effused within the pelvis, without any external mark whatever. Mr. Earle mentioned another case, where effusion of urine took place nearly in the same direction as in the last. The case was further illustrative of another source of effusion—namely, obstruction in the urethra from the lodgment of a foreign body. A boy was admitted into the hospital with a compound dislocation of the ankle-joint. Shortly after he was seized with retention of urine, which was supposed to depend upon a spasmodic state of the urethra; and he was treated accordingly. He was not at all relieved; but the urine made its way upwards behind the triangular ligament, in front of all the lower part of the abdomen. There was none effused into the scrotum or perineum. The calculus was cut down upon and removed, and the patient recovered. An abscess of the prostate may burst, and urine become effused into the cellular tissue, between the rectum and bladder, to a very great extent. In one case where this had occurred, the whole of the cellular tissue behind the peritoneum, as high up as the diaphragm, became in a state of slough, no external mark of the mischief being visible. There was a case, however,

lately in Powell's ward (Vesey) which terminated in a much more favourable manner—the abscess bursting into the rectum—the man afterwards becoming better than he had been for years before. Abscess in the prostate sometimes arises in the course of very acute gonorrhœa, and thence makes its way into the perineum, forming fistulæ of the most intractable kind, requiring very large and deep incisions, and the maintaining a large external wound during the whole treatment. In some very obstinate cases of this description, Mr. Earle has divided through the prostate, as in the late-ral operation for the stone. In one case where Mr. Earle performed this operation in the hospital, a fistulous opening remained until the patient was discharged for bad conduct; but in another case the operation was perfectly successful. It is unattended with danger, and we know how readily the parts heal up after the operation for stone. This is, however, by far the most difficult description of case to manage.

We may have the urethra ruptured from external violence, such as blows or falls; and in these cases the effusion is very rapid. It is generally in these accidents the bulbous portion of the urethra which suffers, and great hæmorrhage often accompanies the effusion of the urine. A large tumor will present itself filled with blood and urine. The extent of the mischief will greatly depend upon the degree of distention of the bladder at the time of the accident, as also whether it were or not in a healthy state before the accident. These cases require the most active and prompt measures. They are of a very dangerous nature, and become much more so when complicated with fracture of the pelvis. The whole urethra may be ruptured, and no bounds are then set to the effusion of urine which may take place. The only chance will be in immediately making a free and deep incision into the perineum, and thus securing an outlet for the urine.

In most cases it is only the under portion of the urethra which is destroyed by ulceration or accident, but Mr. Earle has seen two or three cases where portions of the whole calibre of the canal have been wanting, and he related some particulars of a case in which he had repaired the part by a sort of taliacotomy operation. The case was published in the Philosophical Transactions for 1819.

In cases of effusion of urine from ruptured urethra we should very rarely pass instruments. When we do use one, we should keep it along the upper surface of the canal. A solid one is preferable to a flexible one, as it is impossible to judge accurately of the direction the latter is taking; whereas we may often direct a solid body safely past the lacerated parts. Where we find any decided deviation from the course of the canal, we should at this point cut down upon it, and secure the exit of the urine.

In nearly all cases of effusion from ruptured urethra, we are obliged to have recourse to incisions sooner or later, as abscesses are certain to form from the effused blood and urine; and much suffering to the patient may be saved by the immediate performance of an operation which must eventually be required, and which at once places the patient's life in safety. Having made a free incision, it is better to wait until all inflammation has subsided before making any further attempt at restoring the canal.

CASES OF EYE DISEASES, WITH REMARKS.

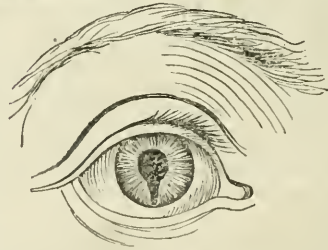
BY MR. WILLIAM MACKENZIE,

Lecturer on the Eye in the University of Glasgow.

No. II.

Coloboma Iridis.

On the 31st of January, 1831, I was requested by Mr. Roy to examine the eyes of Boyd Armour, a recruit, in the Barrack Hospital, Glasgow. In both eyes of this individual the pupil was prolonged, as is represented in the annexed sketch, towards the lower edge



of the cornea. This was the congenital form of the pupils, and though it certainly gave to the countenance a very peculiar and somewhat wild expression, it had not been noticed at the time of enlistment. The vision, as far as could be ascertained, was perfect. Both the natural pupil, and the cleft proceeding from it, contracted, on sudden exposure to light; but the cleft slowly, and not so as to close, at least in a moderate light. The fact of such a cleft in the iris contracting and expanding appears by no means favourable to the notion of Maunoir and others, that the contraction of the natural pupil depends on the action of the sphincter muscle.

The above is the only instance of this malformation of the iris which has

fallen under my observation. That it had been observed by others, I learned from Dr. Savenko, who accompanied me on my visit to Armour, and had in his portfolio a drawing from nature of the very same formation of the pupils.

Walther, as early as 1810, had recorded* the occurrence of this form of the pupil in one eye, with a natural pupil in the opposite eye; and in 1821, having by this time had opportunities of examining six different cases of this malformation, he made it the subject of a paper, in the *Journal der Chirurgie und Augen-Heilkunde*†, characterized, like all his writings, by great elaborateness and ingenuity. He observed that in general the two edges of the cleft ran in a perpendicular direction, but in two cases they divaricated as they approached the lower edge of the cornea, a disposition exactly the reverse of what existed in Armour. In general, Walther observed the lower half of the eye to be less convex than the upper, and apparently less developed. He also noticed that the whole eye was in some cases smaller than natural, the cornea flatter, the pigment deficient, and the eyeball ascillatory. Only in one out of the six cases did this malformation exist in both eyes of the same individual. In none of the cases could he discern any thing like the ciliary processes at the lower extremity of the cleft. To this malformation he proposes to give the name of *coloboma iridis*. As for his notions regarding its origin, I shall defer their consideration to a subsequent part of this paper.

From a communication by Dr. Ammon, in the first number of his *Zeitschrift für die Ophthalmologie*‡, I gather that this malformation of the iris has been the subject of observation, not only to Walther, but also to Wagner, Helling, Erdmann, Heyfelder, and others. Dr. Ammon himself, however, appears to be the first who has enjoyed an opportunity of examining this state of the eye anatomically, in any case of this malformation; and his observations are so interesting, that I cannot refrain from giving some account of them.

The dissection by Dr. Ammon was of the eyes of a woman of the name of

Schönherrin, the mother of three children, none of whom had any malformation of the eyes; neither had her father, mother, nor either of her grandfathers nor grandmothers. The irides were of a greenish colour, with brown spots. The cleft of the irides descended nearly to the lower edge of the corneæ. There was nothing else unnatural in the external appearance of the eyes. Schönherrin died of phthisis on the 24th of June, 1830. She had told that her mother, when pregnant with her, had been frightened by a cat. During her life, Dr. Ammon observed that the pupils moved in a lively manner, and that the edges of the clefts separated, and again approached one another, along with the expansion and contraction of the regular portion of the pupils. The woman saw well, both near and at a distance, in twilight and in bright sunshine; only when she turned the eyeballs very much down, so that the clefts came to be hid behind the lower eyelids, all external objects vanished, nor could she keep her eyes long in this direction.

On dissection, after the muscles and cellular substance were removed from the eyeballs, Dr. Ammon observed at the inferior-posterior part of the scleroticæ, in the course of the middle line of the eye, and at the distance of a quarter of an inch from the edge of the cornea, an elevation of a bluish colour, about five lines long and from two to three broad. This elevation rose about a line and a half above the level of the scleroticæ, and bore the appearance of a staphyloma scleroticæ. Behind this, another peculiar, almost glandular, elevation presented itself, in connexion with the neurilema of the optic nerve. The scleroticæ in the whole course of these elevations was softer, thinner, more yielding, and bluer than elsewhere.

On dividing the left eye into halves, an anterior and a posterior, Dr. Ammon observed on the posterior aspect of the anterior half, that the corpus ciliare had nearly an oval form, terminating inferiorly in a white, elevated, and pointed fold, destitute of any ciliary process. The lens lay in its natural situation, but was also somewhat oval. The uvea was very black; the edges of the pupil, viewed through a glass, appeared flocculent.

On the anterior aspect of the posterior half of the eye, at the distance of a few lines from the connexion of the retina

* Abhandlungen aus dem Gebiete der practischen Medicin, p. 72, Landshut, 1810.

† Vol. ii. p. 598, Berlin, 1821.

‡ Page 55, Dresden, 1830.

with the optic nerve, Dr. Ammon observed in the retina and in the choroid a cleft, seven lines long, posteriorly two and anteriorly three lines broad. The edges of the cleft in the retina, and also of that in the choroid, were sharply defined, and with a fine probe they could be raised up all round. In this cleft lay the thin white sclerotica. In other respects, the retina was pretty natural, presenting its usual folds, and its limbus luteus, but without any foramen centrale. The halo signatus on the hyaloid was somewhat oblong, and pointed at the place corresponding to the cleft of the iris and choroid, as was also the edge of the lens. The examination of the optic nerve led to the discovery of nothing remarkable. The opposite eye presented the same results.

This dissection, then, establishes the extraordinary fact, that a congenital cleft or coloboma may extend to the choroid and retina, as well as to the iris; and that the membrane of the vitreous humour, and even the lens, are not exempt from this malformation. Future research must determine whether coloboma iridis is always or only occasionally attended by a similar deficiency in the choroid and retina. The analogy between this defect in the eye and hare-lip is not undeserving of attention, and the fact that the latter exists in many instances without any deficiency extending to the soft or to the bony palate, affords ground for supposing that coloboma may also be confined in some cases to the iris alone. It would be interesting to know whether coloboma palpebræ has been observed along with coloboma iridis.

A remarkable fact in Dr. Ammon's case is, that the corpus ciliare still formed a complete circle, although placed between the cloven iris and choroid.

The question naturally occurs—does coloboma of the interior parts of the eye throw any light on the genesis or organization of this part of the body; or, on the other hand, does any thing that is known of the original mode of growth of the eye, at all illustrate this curious malformation? I believe it is the general, and I think it is the true notion regarding hare-lip, and similar deficiencies, that they are in no instance produced in the fœtus, but arise entirely from want of regular organization; that they are not diseases nor

effects of injury, but simply abortions in completion. The process of genesis is checked, and hare-lip and the like are the consequence. Walther adopts Meckel's opinion* regarding all congenital deficiencies or divisions in the middle line of the body—as hare-lip, fissure of the soft and bony palates, spina bifida, imperfect bladder, &c.; but extends the doctrine even to those organs of the body which exist in pairs. He supposes that the law of formation for the trunk of the body is also the law for each individual organ; that each consists originally of two lateral halves, which, after a certain degree of development, coalesce in the middle line of the body, or of the particular organ. Dr. Ammon observes, (without any intention of attacking Meckel's theory, that, through partial deficiency of the mesial edges of the lateral halves of the body, numerous malformations arise), that he thinks Walther has pushed the idea too far, when he applies it to congenital cleft of the iris; as, not even in the earliest period of the fœtal existence, has any appearance of cleft been observed in the iris, sclerotica, or any part of the human eye. It is a curious fact, however, that in the fœtal eye of some of the lower animals, a cleft or fissure has actually been observed. Kieser† discovered this in the eye of the chick, and Carus‡ in the eye of kittens and calves. The former supposed that the fissure, which in the chick is found on the fifth day of incubation, existed only in the choroid; but, to the latter of these observers, it appeared to affect the iris, the choroid, and even the sclerotica.

Dr. Ammon remarks, that, in the genesis of the human eye, nothing is seen approaching to lateral halves of a sphere; but that, on the contrary, the organization of the organ seems to proceed in the direction of its axis, from behind forwards.

Along with the lens and vitreous humour, which latter is at first but small, the retina is observed, very flocculent, enclosed within an external covering, the sclerotica. Some time after these forms, close behind the anterior part of

* Pathologische Anatomie, vol. i. p. 93. Leipzig, 1812.

† Oken und Kieser's Beyträge zur nergheiehen-den Zoologie. Bamberg, 1807.

‡ Introduction to the Comparative Anatomy of Animals, translated by Gore, vol. i. p. 329. London, 1827.

the external coat, a narrow ring, the inferior edge of which is occasionally observed to be narrower than the rest. This ring is the iris, lying close to the transparent part of the external coat, or cornea, so that there is no anterior chamber at this period. The choroid, about the same time, is gradually formed between the sclerotica and the retina. The eyelids, which appear mere duplicatures of the common integuments, are at this period not sufficiently extensive to cover the bulb of the eye. To form the anterior chamber, the pupillary membrane is next developed; the pupil is shut; and thus a second hemisphere is added, as it were, in front of the first. The conjunctiva follows, covering the cornea and lining the eyelids; and thus a third hemisphere is added to the former two. The lids, at the same time, close. They remain closed, and the pupil also, till the eighth month, at which period the fissura palpebrarum is formed, apparently by increased absorption, and the pupillary membrane is ruptured and shrivelled away.

According to this account, the human eye and eyelids are developed by the super-position first of one and then of another hemisphere to the original bulb of the eye, in a direction from behind forwards. It follows, that congenital clefts of the organ do not arise from the parts persisting in their original state, or in the state in which they were produced at an early period of the fetal existence.

Feb. 1832.

EFFECTS OF SWALLOWING PINS.

To the Editor of the London Medical Gazette.

SIR,

IF you think the following case of inflammation and gangrene of the integuments of the abdomen, in an individual who was in the habit of sleeping with pins in his mouth, worthy of a place in your pages, you will oblige, by its insertion,

Your obedient servant,
J. O.

Dublin, Jan. 1832.

Peter Kearns, a gardener, aged 56, of robust frame, was admitted into Sir

P. Dun's hospital, under the care of Dr. Osborne, on the 16th of August. It was ascertained, from the testimony of his wife, that he had a singular predilection for keeping pins in his mouth, which he was in the habit of putting there almost every night on lying down to sleep. This circumstance was, however, not communicated until after they had appeared in an ulcer produced in his groin, and he persisted a long time in the belief, that it was *impossible* he could have swallowed them.

On his admission he had pain in the right iliac region, increased by pressure or by straining at stool: there was fullness and tension of the part, together with swelling of the inguinal glands. It had lasted two months, having commenced from the cavity of the ilium, and gradually increased till within a few days, when the pain and tension forced him to seek the aid of an hospital. Repeated applications of leeches were used, with water dressings, and subsequently, poultices, and he got draughts of turpentine and castor oil, with hip-baths at nights.

23d.—The tumefaction and inflammation are increasing. The occurrence of flatulence moving in this direction causes a peculiar pain. Bowels confined during the last thirty hours. A dark spot has formed at the centre of the tumor, a wheyish-coloured fluid oozes from around it, and there is a boggy feel, with crepitation, on pressing towards the centre, with induration at the circumference extending towards the pubis and a few inches down the thigh. He was delirious in the night, but there has been neither hiccup nor vomiting. Pulse 90; thready. A continuance of the poultices was directed, with a turpentine enema; and a table-spoonful of port-wine, in an effervescing draught, every hour.

24th.—The tumor has now been opened, and a large quantity of foetid sanious pus has been discharged; had a motion this morning, which contained a small quantity of sanious pus; fermenting poultice to be applied; to continue the wine, and take a quinine pill four times in the day.

30th.—During the last four days there has been a copious discharge of foetid yellowish pus, mixed with sanies, from the wound, and also some coagulated blood, but no repetition of purulent discharge by stool. In order to meet

the tendency to sinking, which became very apparent, he got an increased allowance of wine, with porter; and, as his appetite was not much impaired, he was able to use meat diet. The sulphate of quinine was taken in doses of three grains, thrice in the day.

On the 4th of September, the inflammation having now subsided, and the ulcer having assumed a much healthier aspect, a large-sized pin, crooked to a right angle, and oxidized of a purplish black colour, made its appearance at the bottom of the ulcer. A few days afterwards this was followed by another, and in some time by a third, previous to his departure from the hospital. After his return to the family with which he lived, the opening continued unclosed and without secretion for two or three months, during which time he acquired the name of the "*Pincushion*," from the exit of pins which take place at various intervals, most of them crooked, and all covered with the purplish black oxide above mentioned. On one occasion he was much alarmed by having "broken wind," as he expressed it, through the opening; but, with this exception, no remarkable occurrence took place after his dismissal from the hospital.

The passage of pins and needles from the intestines, through various and sometimes distant parts of the body, has often been observed*. In the present instance, however, there were some peculiar circumstances worthy of notice. There can be but little doubt that the place where the pins were collected, was the cœcum, and the passage of purulent matter by stool, in connexion with the highly inflamed state of the parts about the cœcum, which at the same time began to secrete pus, denotes a communication taking place between the cavity of the intestine and the surrounding integuments. In connexion with this may be mentioned, that in a patient of Dr. Osborne's, who died of fever, there was found, in the vermicular appendix of the cœcum, a pin covered with large incrustations, which consequently must have remained there some considerable time, and yet did not appear to have excited any disturbance. The crooked figure of the

pins in this case necessarily produced much of the pain and irritation, inasmuch as, whatever position they could be placed in while proceeding towards the exterior, they must have acted like barbed arrows. Once, however, an opening was effected by the passage of the first, the subsequent pins followed in the same track with the greatest facility, and in fact unperceived by the patient, who was as much surprised as his medical attendants could be, when, on taking off the dressings, the head or the point of a pin was described, peeping up among the granulations. With respect to the crepitus perceived in the surrounding integuments, the same is described in the majority of the very interesting cases of inflammation of the right iliac region, related by Mr. Ferral, in the Edinburgh Medical Journal, and is usually ascribed to the commencement of gangrene. In this instance, however, the air effused under the integuments was not to be ascribed to that cause, for the gangrene was principally superficial, and confined to one spot, while the crepitus was diffused through a considerable space, and in the body of the cellular tissue; and again, long after the inflammation had been subdued, he passed air through the opening, as has been mentioned. In such cases as have been described by Mr. Ferral, it will be necessary to examine carefully as to the origin of the emphysema, and not hastily to ascribe it to gangrene. The adhesions of the cœcum with the cellular membrane outside the peritoneum are so intimate, and this portion of the intestine is so liable to ulceration, and consequently to perforation, that the passage of air from the latter into the former may very readily occur, and thus produce an appearance which, if mistaken for gangrene, will give rise to a very inappropriate course of treatment. It is evident that, in our case, the pins must have passed through the cœcum in the part now alluded to, and not through the peritoneum; as, in the latter case, the symptoms of peritoneal inflammation would infallibly have been present.

* See Med. Gaz. vol. i.

OBSERVATIONS ON THE INCUBATION OF MORBIFIC GERMS.

To the Editor of the London Medical Gazette.

SIR,

THE study of the *specific* causes of fever may be dated from about the commencement of the last century. In the days of Sydenham, and prior to his time, pathologists were chiefly occupied with discussions concerning the *proximate* causes of fever, such as acidity of the juices, fermentation, putrefaction, or degeneration of one or more of the humours, &c. They described, indeed, with much accuracy, those direct causes of fever which are obvious to our senses, such as the variations of the atmosphere in respect to its heat and dryness, the qualities of the ingesta, irregularities of exercise, and deficient cloathing. It was reserved, however, for Lancisi, Sir John Pringle, Huxham, Lind, Dr. Russell, Dr. Withering, and other eminent writers of the last century, to investigate those *occult* sources of fever which have their origin either in the soil, or in the animal frame itself, and which we now designate as the infective, or febrific germs. Throughout their writings many observations will be found upon the interval which elapses between the application of the morbid agent and the subsequent development of symptoms; and it cannot be doubted but that, in all ages, this subject has been considered as deserving of attention. It does not appear, however, that these authors devoted themselves specifically to its investigation. For obvious reasons, little could be looked for concerning it from the writers of an earlier æra. But even in our own times, it has been comparatively neglected. I know indeed but of one author who has made it the subject of specific inquiry. In the fourth volume of the Dublin Hospital Reports, there is a paper by Dr. Marsh, entitled, "Observations upon the Origin and Latent Period of Fever." In this Essay Dr. Marsh has advanced many very profound and philosophical views regarding the origin of disease generally; and no one can rise from its perusal without a conviction of the advantages which may be expected from a more diligent attention than has hitherto been paid to the laws which regulate the in-

terval between the reception and the development of morbid germs. With reference to sanitary or police regulations, this subject is obviously of the highest importance. It assists also materially in the study of diagnosis; and it will be found to involve many questions of *general* interest, and not a few of a directly practical tendency.

That this branch of study has not hitherto been more diligently pursued, need not surprise us. To establish the many minute details which it requires, investigations must be entered into for that specific object, and continued for years with a degree of assiduity such as few men engaged in actual practice could afford to bestow. But it must also be confessed, that the experience of one individual, though devoted exclusively to this object, would be insufficient; and the glory of discovery, therefore, must be shared with many fellow labourers. Constituted as human nature is, this can hardly be expected, while so much remains to reward the unassisted labours of the ambitious pathologist.

Under this impression, I do not venture to offer myself as a guide to the profession, on a topic requiring such extended research, and such accurate and varied personal experience. My aim is only to make a beginning in so important a work, by contributing the few results of my own observation, and collecting the recorded opinions of others, so far as they are known to me. The experience of my cotemporaries may serve to fill up some of the blanks which I may leave, while to other points hitherto unexplored, the labours of posterity may be advantageously directed. One principal object, in fact, which I had in undertaking this task, was to direct the attention of observing physicians to this neglected branch of pathology, — the ascertaining, with all reasonable accuracy, the usual periods of incubation of the various morbid agents or germs, as well as the anomalies which they present.

To the general subject of investigation I have given the title of the "*Period of Incubation*." This term was originally suggested by the French; and it appears well deserving of admission into the nomenclature of English medicine. The terms "latent" and "dormant periods," which will be found in most of our standard works,

carry with them this objection,—that they presuppose the absence of all symptoms during the incubative stage; a position which, though true in some cases, is far from being of general application. Indeed, the kind of symptoms present during the incubation of the several diseases propagated by infective germs, is a legitimate subject of inquiry. Dr. Marsh has several useful observations on this topic.

There is every reason to believe, that the interval between the application of the exciting cause of a disease, and the manifestation of its peculiar phenomena, must be liable to many variations. The causes of these variations, and the extent to which they go, are points which it is of great importance to determine. This subject has also been touched upon by Dr. Marsh, in the Essay above referred to. "From numerous observations," he says, "I have been led to think that the latent period may be shortened, and the accession of constitutional symptoms accelerated, by the occurrence of what are technically called the *exciting* causes of disease." To illustrate this he cites the following case:—"A boy, twelve years old, was playing with a favourite dog, and was bitten by him on the nose. The injury was slight. A few days afterwards the dog died rabid. The wound soon healed, and the circumstance made no impression on the boy's mind, and was wholly forgotten. Four weeks afterwards he was thrown by his companions into a ditch; he went home wet, chilled, and complaining that he felt ill. That very night, unequivocal symptoms of hydrophobia manifested themselves, which proved speedily fatal." In this case, the usual period of incubation of the hydrophobic germ was (probably in consequence of this accident) *anticipated* by twelve days.

By a comparison of numerous cases with each other, the pathologist endeavours to ascertain the *average* periods, and the *maximum* and *minimum* periods. To the maximum period it is difficult to attach any precise limit; yet common sense teaches that some such must be set. Tales are gravely told of hydrophobia occurring *twelve years* after the infliction of the wound. These cases are undoubtedly fabulous. It may indeed reasonably be questioned whether any infective germ whatever can exist

in the body *twelve* months, retaining the power of producing disease. The utmost limit to which my own experience, and that of the professional friends on whose accuracy I can rely, would warrant me in extending this period, is *nine* months. In the case of a lady, who died in Edinburgh of hydrophobia, under the care of the late Dr. Gregory, nine months elapsed between the bite and the development of the disease. A physician of great eminence in this town related to me the case of an officer, who first suffered from severe ague nine months after leaving the island of Walcheren, the interval having been passed in the healthiest parts of London.

The diseases in which it appears of most importance to determine the periods of incubation of their respective germs, are the following:—1. *Ague and Remittent Fever*; 2. *Typhus*; 3. *Plague*; 4. *Epidemic Yellow Fever*; 5. *Cholera Spasmodica*; 6. *Small Pox*; 7. *Measles*; 8. *Scarlet Fever*; 9. *Purpural Fever*; 10. *Hooping Cough*; 11. *Hydrophobia*; 12. *Gonorrhœa*; 13. *Syphilis*.

I shall offer a few cursory reflections upon each of these topics of inquiry.

1. *Ague and Remittent Fever*.—I am indebted to the kindness of Sir William Franklin, Principal Inspector of the Medical Department of the Army, for the following document, illustrating the incubative period of Malarial or Endemic Fever.

"Notes relative to the movement of a detachment of De Roll's Regiment in Sicily, in the year 1810.

"On the 12th July, 1810, the regiment of De Roll marched from Milazzo, and encamped on the heights of Cureuraci, near Messina. A detachment was subsequently sent to occupy a large house called the *Casa del Corso*, at a short distance from the body of the regiment. This mansion is situate on an eminence, between which and the sea there is a swamp, distant from the house at least half a mile. The north and north-westerly winds, very prevalent in Sicily, blow, often very strongly, over this swamp, directly on the *Casa del Corso*. During the prevalence of these, no one ever slept there, or in the open air near the house, without suffering from intermittent or remittent fever." This information Sir William Franklin obtained from a man who had managed

the vineyard belonging to the farm for fifteen years, and who had himself contracted fever, from sleeping with open windows.

At this house the troops took up their quarters on the 7th July, 1810. They consisted originally of eighty-three men, who were subsequently joined by eight others, making a total of ninety-one, exposed to the germs of disease. On the 31st of July, (thirteen days after exposure to the malaria,) the first case of remitting fever was sent to hospital. On the 1st of August five others were reported. The following day the detachment moved from the Casa del Corso, and encamped near the rest of the regiment, which was then healthy. Yet the men continued to drop; and the admissions into hospital from this detachment took place in the following order:—

Number sent to Hospital.	Date of Admission.	Interval since exposed to Malaria.
1	July 31, 1810	13 Days
5	Aug. 1,	14
1	2,	15
8	3,	16
5	4,	17
6	5,	18
7	6,	19
11*	7,*	20*
12	9,	22
4	10,	23
8	11,	24
2	12,	25
3	13,	26
1	15,	28
2	20,	33
1	26,	39
Total, 77 Men.	27 Days	

Strength of the Detachment, 91.

From this return it appears that the *average* period of incubation of the paludal febrile miasm is twenty days, the *minimum* thirteen, and the *maximum* thirty-nine. Of the whole number exposed (for a fortnight) to the action of the morbid germ, fourteen only escaped an attack of the disease. Whether a further exposure would have brought those fourteen men under the influence of fever, must remain a matter of doubt. I am inclined, however, to think *not*, and that the chief danger, in all cases,

is from *first* exposure. As a proof of the *intensity* in which the poisonous effluvia existed in this instance, it may be remarked, that of the total number attacked (77), twenty-three died, being in the exact ratio of thirty per cent. This is the usual rate of mortality in small pox, and it approximates also to that of the Epidemic Cholera, as it has recently shewn itself in Northumberland.

Dr. Marsh remarks (p. 493), that many of the Irish labourers who are employed during the harvest in the fenny counties of England, have their *first* fit of ague *after* their return from Ireland. This assertion is established by numerous cases of ague, which, at certain seasons of the year, are admitted into the ward of Steevens's Hospital. The period of incubation is thus extended to several months. The maximum I would venture to place at nine months.

2. *Typhus Fever*.—Frequent as is the contagious typhus in this country, it is by no means easy to meet with cases so circumstanced, as to offer determinate data for fixing the period of incubation of its infective germ. The commonly received opinion, I believe, is, that such period is subject to great variety. Dr. Haygarth, from his own observations, deduced the conclusion, that the minimum period was seven days, and the maximum seventy-two days. Dr. Bancroft's opinion coincides very nearly with this. From observations made on the hospital attendants, on occasion of the return of Sir John Moore's army from Corunna, he inferred, that thirteen days formed the minimum, and sixty-eight the maximum period. Sir W. Burnett, in his "Account of a Contagious Fever at Chatham," relates the history of a party of men belonging to the St. George, at Spithead, sent on the 3d July, 1811, to assist in navigating the Dolphin troop-ship, whose crew were affected with typhus. On the 10th January (seven days after exposure), fourteen cases of fever were sent to the hospital ship from the St. George, and many subsequently, up to the 21st January (the eighteenth from exposure), after which period no cases occurred.

Dr. Marsh has made it one principal design of his paper to shew, that, in the Irish epidemics, the apparent period of incubation of the typhoid germ was very short indeed. In several instances which he relates, the febrile rigor suc-

* Average.

ceeded so *immediately* to the application of the contagious effluvium, that it is scarcely possible to conceive that the germ of disease could have operated through the medium of the absorbents. He adopts, therefore, the notion of its *direct* agency on the sentient extremities of the nerves. An obvious objection, however, is open to all the cases cited by Dr. Marsh. During the prevalence of epidemic fevers so widely diffused as those adverted to by this author, there is every probability that the infective germ had *previously* been received into the system.

The following series of cases, which are not open to this source of fallacy, occurred under my own observation in the year 1819; and to those who have doubts on the subject, they may be adduced as undeniable proofs of the spreading of typhus fever *by infection* in private houses, with every advantage which ventilation, personal cleanliness, and all the comforts of domestic life can afford. The cases were all seen by my friend Mr. Johnston, of Mortimer-Street, and they occurred in the family of a medical gentleman, resident in that neighbourhood.

Case 1.—A. B., maid-servant, recently received into the family, sickened with typhus fever May 16, 1819. Died May 21.

Case 2.—C. D., the mistress of the house (whose attention to her servant during her illness had been unremitting), sickened with typhus May 24. Died June 3, under the care of the late Dr. Latham.

Case 3.—E. F., maid-servant, who had attended A. B. during her illness, sickened with typhus May 30. Came under my care June 7. Died June 9.

Case 4.—G. H., sister of A. B., residing in Clare Market; she visited her sister during her illness; sickened with fever May 26; was delivered of a dead child June 2. June 11, seen by me and found convalescent, though still in bed.

Case 5.—J. J., cook in the same family, sickened with typhus May 29, and removed to her own home. I found her on the 10th June, convalescent, under the care of Mr. Smith, of Red Lion-Street.

The periods of incubation in these cases, may be stated at seven, nine, twelve, and thirteen days from *first* exposure to the infective germ. I have every reason to believe, that ten days

may be viewed as the *average* period in the case of the typhoid miasm.

3. *Plague.*—From the concurrent testimony of numerous authors, we are warranted in saying, that the period of incubation of the true pestilential germ is very short. We may even lay it down as a maxim in the pathology of fever, that the more malignant the disease, the shorter is its period of incubation. Two have been stated as the minimum, and fifteen days as the maximum period. Five days may be looked upon as a fair average.

When the plague has been received by inoculation, constitutional symptoms begin on the 4th day.

4. *Epidemic Yellow Fever.*—I am not now in possession of any series of facts, by which to determine the period during which the infective germ of this disease lies dormant. From the inquiries, however, which I had formerly the opportunity of making, through the kindness of W. W. Fraser, Esq. Inspector-General of Hospitals, I was led to believe, that the interval between exposure to contagion, and the development of symptoms, varies from *two to ten* days.

5. *Cholera Spasmodica.*—The Central Board of Health have given the following as the results of their extended inquiries into the incubative period of this singular disorder.

1. Out of 171 cases of Spasmodic Cholera at Berlin, 159 occurred within five days from exposure to the infective germ.

2. At St. Petersburg, in the cases where *single* exposure to infection was best ascertained, the period of incubation ranged between one and five days.

3. In the Austrian territory, according to the reports of the Genöese Medical Commission, it was observed, "that those who had absorbed the germs of the disease, were generally attacked before the third, and not later than the fourth day."

6. *Small-Pox.*—At the Small-Pox Hospital, abundant evidence has been afforded that the period of incubation is usually about twelve days. I select a few cases, in illustration.

Case 1. Mary Argent was discharged convalescent from the Small-Pox Hospital, July 6, 1830. She slept that night with her sister, Susan Argent, who sickened for the small-pox July 19th; the eruption shewed itself July

21st; she was admitted into the hospital July 22d. The period of incubation, counting (as I always recommend should be done) from exposure to the *appearance of eruption*, was in this case fifteen days.

Cases 2 and 3. Elizabeth Hall, aged 3 years, residing in Field-Place, St. John's-Street, was attacked with small-pox Oct. 25, 1829, and left that house for the hospital, Oct. 28.

Henry Hall, aged 5 years (brother of the above), was, after two days of fever, attacked with small-pox Nov. 5, and received into the Small-Pox Hospital Nov. 7: period of incubation, *eleven* days.

Alfred Taylor, aged 5 years, living in the same house, was, after three days of fever, attacked with small-pox, Nov. 14: period of incubation (presuming that he took the disease from the second child), *nine* days.

Cases 4, 5, 6, and 7. Sarah Harman, seventeen months old, left Clerkenwell workhouse (having had the small-pox for five days) on Jan. 28, 1828.

Feb. 6. Another child in the same workhouse, after three days of fever, took small-pox: period of incubation, *fourteen* days.

Feb. 7. Another child attacked with small-pox, after six days of fever: period of incubation, *fifteen* days.

Feb. 8. A fourth child attacked, after two days of fever: period of incubation, *sixteen* days.

Feb. 10. A fifth child attacked, after four days of initiatory fever: period of incubation, *eighteen* days.

All these cases were received into the Small-Pox Hospital.

Case 8. Elizabeth Foster was attacked with small-pox, Nov. 26, 1830, after four days of fever. She was exposed to small-pox, Nov. 14, when her sister, Lucy Foster, sickened with it, and was sent to the Small-Pox Hospital: period of incubation, *twelve* days.

Case 9. A young medical friend (Mr. Tebbs), some years ago, accompanied me to the Small-Pox Hospital on a Thursday. On leaving the wards, he expressed to me his firm conviction (from the peculiar feelings he experienced at the moment) that he had received the germ of small-pox. He soon after became languid, and his appetite fell off. On Saturday, in the ensuing

week, rigors supervened, and two days afterwards the eruption of small-pox. In this case the period of incubation was *twelve* days.

In the London Medical Gazette, vol. iii. p. 282, a *series* of cases is given by Mr. Caesar Hawkins, in which the period of incubation was very short—in none exceeding ten days. In one of them, rigors supervened within a few *hours* after exposure to contagion, and the eruption shewed itself on the third day, or, at furthest, at the very commencement of the fourth. This corresponds with what has sometimes been observed by Dr. Marsh, in regard to typhus. A feeling of disgust was experienced at the time, so strong that the gentleman dreamed of it the following night, and the impression frequently recurred to his mind.

The source of contagion was, in all these instances, a dead body brought into the Windmill-Street Theatre, for dissection. The body had been probably dead fourteen or sixteen days before two of the gentlemen came near it. Mr. Hawkins has justly remarked, that this occurrence of the disease in several persons, from exposure to the effluvia of a body so many days after death, is well deserving of notice, and seems to confirm the suspicion recently entertained with regard to the same liability in the pestilential cholera.

From the same series of cases we may learn, in regard to the origin of disease, how much depends upon the *susceptibility* of the recipient of the infective germ. One of the gentlemen who took the small-pox from this source, had been in the habit of frequenting the Small-Pox Hospital; and, three months previously, had made drawings of the disease from patients both before and *after* death.

From the results of my own experience in small-pox, six days is the *minimum* period of incubation which I should be inclined to admit. I would place the *maximum* period at twenty-one days.

It would be improper to pass over, without some comment, that peculiar sensation recorded in two of the preceding cases, as having been perceived at the moment of imbibing the infective germ. I have heard of the same thing in many other instances, and the patient often expresses himself as being

frightened. Dr. Marsh has alluded to the circumstance of a highly disagreeable and peculiar odour, as characterizing the reception of the *typhoid* miasm, and founds upon it a train of ingenious reasoning concerning the *modus operandi* of the infective germ. I would beg leave to point attention also to the *languor*, loss of appetite, and impaired rest, which attended the incubation of the variolous germ in the same case. This sometimes proceeds to so great an extent, that, in the patient's judgment, six or seven days, instead of two, elapse between the attack of disease and the appearance of eruption. Hence arises the necessity of counting the period of incubation, from the reception of the germ, to the *occurrence of eruption*.

It is worthy of remark, that, of the numerous cases received into the Small-Pox Hospital, not one in twenty is able to trace the disease to any source of infection; but it is believed by the patient to arise from cold, fatigue, change of air, or some similar circumstance. While so much difficulty is thus experienced in tracing the origin of a well-known disease, which spreads, I believe, exclusively by infective germs, it need not create surprise if we encounter similar difficulties in developing the origin of a disorder so little known to us as the spasmodic cholera.

When the small-pox is received into the system by inoculation, seven days elapse between the insertion of the virus and the establishment of the fever. In this case, the appearance of eruption is cotemporaneous with that of the fever. An opinion has long prevailed, that, from the difference of the incubative periods, the inoculated small-pox would take precedence of the natural disease. I believe this notion to be destitute of all foundation. My reason for saying so is, that, with very few exceptions, it will be found, when small-pox is "*in the blood*," that vaccination will not advance.

7. *Measles*.—My own experience furnishes me with no precise data for determining the period of incubation of the rubeculous germ. It is, I believe, generally understood to vary from eight to fifteen days, counting from the reception of the germ to the first attack of rigor. Dr. Bateman says, "from ten to fifteen days." But as the catarrhal fever ought undoubtedly to be

viewed as constituting part of the incubative stage, so this period will be found to extend, in many cases, to twenty-one days. Dr. Marsh relates the following case*. "A single exposure to measles took place on the 23d March. On the 3d April, the catarrhal symptoms began; on the 5th the eruption shewed itself." Here the full period of incubation was *thirteen* days.

8. *Scarlet Fever*.—The latent period of this miasm has been the subject of frequent inquiry. Dr. Withering's words are, "I have repeatedly had reason to observe, that it is upon the third or fourth day after exposure to contagion, that the patients begin to complain." Dr. Heberden extends the period of incubation to five days. Dr. Blackburn says it varies from four to six days. Dr. Willan gives six days as the *maximum* period.

Dr. Maton has described, in the Transactions of the College of Physicians (vol. v. p. 161), a peculiar variety of scarlet fever, in which the latent period varied from seventeen to twenty-six days; average, twenty-one days. The difference in the periods of incubation constitutes one of the strongest diagnostic marks between this and the common scarlet fever. Dr. Maton mentions the cases of eight members of one family, attacked by this disease. They all terminated favourably.

9. *Puerperal Fever*.—I have been given to understand that the late Dr. Gooch considered three days as the average period of incubation of the puerperal peritonitis spreading by contagion. This disease, when fatal, terminates (according to the same author), for the most part, on the sixth day.

10. *Whooping Cough*.—I have never been able to ascertain, by any facts, the period of incubation of the germ of this disease; nor do I know that any author has hitherto directed his attention to it.

11. *Hydrophobia*.—Some years ago I made notes of a series of cases of hydrophobia (taken indiscriminately in the course of my reading), with the express object of determining the minimum, average, and maximum periods of incubation of the hydrophobic virus. The cases were thirty-one in number. The following was the result.

* Dublin Hospital Reports, vol. iv. p. 495.

Case.	Period of incubation.	Authority.
1	21 day	Mr. Gray, Duncan's Comment. vol. 12.
2	26 ...	Dr. Dickson, Med. Obs. and Inq. vol. 3.
3	31 ...	Marshall on Hydrophobia.
4	35 ...	Dr. Chambers, St. George's Hospital.
5	36 ...	Dr. Pinckard's Cases of Hydrophobia, (seen by myself.)
6	36 ...	Dr. Plummer, Ed. Med. Essays, vol. 6.
7	38 ...	Dr. Babington, Med. Comment. vol. 1.
8	38 ...	Mr. J. Scruton, Duncan's Com. vol. 17.
9	38 ...	Dr. Johnstone, Memoirs Med. Soc. of Lond. vol. 1.
10	39 ...	Attended by myself. Med. Chir. Tr. vol. 13.
11	40 ...	Mr. Sawrey, Marshall on Hydrophobia.
12	40 ...	Dr. Lister, in Dr. Bardsley's Reports.
13	40 ..	Dr. Munckley, College Trans. vol. 2.
14	42 ...	Mr. Parkinson, Marshall on Hydrophobia.
15 (Average.)	44 ...	Dr. Pinckard's Cases of Hydrophobia.
16	47 ..	Dr. Babington, Medical Records.
17	60 ..	Mr. O'Donnell, Med. Comment. vol. 2.
18	60 ...	Mr. R. Simmons, Med. Facts, vol. 5.
19	60 ...	Dr. Marcet, Med. Chir. Trans. vol. 1.
20	63 ...	Dr. Wavell, Medical Records.
21	72 ...	Duncan's Commentaries, vol. 17.
22	73 ...	Dr. Pinckard. (Case of W. Waters.)
23	74 ...	Ditto. (Case of W. Rogers.)
24	77 ...	Mr. A. Battue, Duncan's Com. vol. 3.
25	3 months.	Dr. Satterley, College Transact. vol. 4.
26	4 ...	Dr. Fothergill's Works.
27	4 ...	Dr. Dickson, Med. Obs. and Inq. vol. 3.
28	4 ...	Ditto. (Two cases, bitten by the same dog.)
29	8 ...	Acta Norimb.
30	9 ...	Dr. James Gregory, of Edinburgh.
31	9 ...	Mr. Gaitskell, Memoirs Med. Soc. Lond. vol. 5.

From this table it appears that the average period of incubation of the hydrophobic germ is forty-five days; the minimum, twenty-one days; the maximum, nine months.

In the largest proportion of these cases (that is to say, twenty-four out of thirty-one), death took place within three days from the manifestation of symptoms. Six died between the fourth and seventh day. One, only, lived eight days.

From this document we may learn to distrust those alleged cases of hydrophobia occurring *within* three weeks from the infliction of the wound, and which ended favourably.

12. *Gonorrhœa*.—According to Mr. John Hunter, the latent period of gonorrhœa is subject to as great variety as the diseases strictly constitutional. He believes that in some instances the complaint has shewn itself within a few hours from exposure to infection; but he acknowledges that the interval usually ranges between six and twelve days. He states the maximum period at six

weeks. In almost all the cases which have fallen under my own observation, the urethra being previously healthy, the latent period was one week. How far we are justified in adopting John Hunter's notions regarding the maximum and minimum periods, I am not prepared to say.

13. *Syphilis*.—The same distinguished author was of opinion that the interval between the application of the syphilitic poison and its *primary* effects, was *very uncertain*, but on the whole longer than in the case of gonorrhœa. He considered that the variations of the latent period depended, in some degree, upon the kind of structure implicated. In some instances he believes that chancre have appeared twenty-four hours from exposure to the virus. But he admitted of a latent period extending even to two months. Seven days is usually, and I believe justly, considered as the *average* period. My own experience in this disease is too limited to enable me to add any thing to the observations of John Hunter, on the extreme periods;

and, unfortunately, I am not acquainted with the opinions of our best surgeons on these points.

The interval usually assigned for the development of the secondary symptoms of syphilis is six weeks, corresponding closely with the latent period of the hydrophobic poison. Cases, indeed, have been related, wherein secondary syphilis never manifested itself until many years after the disappearance of primary symptoms. The authority on which such cases rest is undoubted, but they are always open to suspicion*.

Your obedient servant,

GEORGE GREGORY.

Feb. 6, 1832.

BOTANY OF NEW ZEALAND;

BEING A

Description of Trees, Plants, &c. indigenous to that country.

By G. BENNETT, Esq. F.L.S. M.R.C.S. &c.

[With an Engraving.]

Kaetutowa Tree of the natives of NEW ZEALAND.

THIS tree is of the natural family Myrtaceæ, but the specimens collected in June were neither in fruit or flower. It is probably a species of *Leptospermum*. It is found abundantly at New Zealand, both on elevated and low land, and is named Kaetutowa, or Manuka, by the natives. It attains the elevation of from twenty-five to thirty feet, but seldom more than three or four feet in circumference. The wood is very hard and heavy, and is used by the natives in the manufacture of their pattu-pattu, or war-club, paddles, &c.

Rata Tree of the natives of NEW ZEALAND.

This tree is of the natural family Myrtaceæ, and probably a species of *Metrosideros*, but the specimens collected at New Zealand, in May and June, were neither in fruit or flower. It is named Rata by the natives, and attains the height of thirty or forty feet; but, being a very branchy tree, the length of timber below the branches seldom exceeds twelve feet, and it is not usually found of straight growth. In circumference, it is from eight to ten feet.

The wood is hard, but not so heavy as the preceding; it is, therefore, less estimated by the natives for the manufacture of the pattu-pattu, or war-club, paddles, &c. to which purpose it is also usually applied by them.

Maire Tree of the natives of NEW ZEALAND.

From the specimens of this tree which I collected at New Zealand, in May and June (1829), being neither in flower or fruit, I am unable to determine the generic or specific character. It is named Maire by the natives of New Zealand, attains the height of twenty-five or thirty feet, and the circumference of three or four feet. The wood is hard and heavy, and is used in the manufacture of their war-clubs, paddles, &c. &c.

The three trees just described (the Kaetutowa, Maire, and Rata) are those preferred by the natives (on account of the wood being heavy and hard) for the manufacture of the pattu-pattu, or war-clubs; of them, the Kaetutowa and Maire are preferred, the Rata being in some degree lighter than the others.

Phormium Tenax*, or FLAX PLANT.

Korudi of the natives of NEW ZEALAND.

Natural Family, *Liliaceæ*.

Class, *Hexandria*. Order, *Monogynia*.

This valuable plant is indigenous to New Zealand, where it is regarded as sacred, that is, more an object of veneration for its value, for it is not employed in religious or other ceremonies. It grows in moist, marshy soil, but I have often observed it growing also on the declivities of hills. The leaves are ensiform, of a bright green colour, with a rim of orange along the margin: the foliage attains the elevation of five to seven feet, and resembles in mode of growth our water flags: the flower stalk rises to the elevation of four or five feet beyond the foliage, and bears a profusion of flowers of a reddish yellow colour, succeeded by triangular capsules, filled

* The original draft of this paper was sent to the Central Board, and appeared in the *Cholera Gazette*. The author now lays it before the readers of this journal in its more complete and developed form.—ED. GAZ.

“ * *Phormium*.—*φορμιον* est nomen plantæ ex qua storeas mattasque plectunt, apud Aristotelem Hist. An. lib. v. Id huius generi dedimus, quoniam incolæ Novæ Zeelandiæ ex ea lini speciem fabricant, unde earum mulieres storeas pro variis vestimentis plectunt.”—*Forster*.

with numerous oblong, flattened, black seeds. The leaves grow perfectly erect, but are figured incorrectly in Cook's First Voyage and other works, as they are delineated bending towards the

ground, which from their rigidity they are, unless broken, unable to do. The accompanying figure, drawn from a plant at New Zealand, shows its mode of growth:—



The flax procured from this plant is situated (unlike all other kinds with which we are acquainted) in the leaves, where the fibres run in a longitudinal direction, covered by the epidermis. There are several varieties of this plant indigenous to New Zealand, from some of which the flax is procured of much finer quality than others: I collected much finer specimens of the flax from the vicinity of the river Thames, New Zealand, than from the Bay of Islands.

The flax is used by the natives of New Zealand for a variety of purposes: from it they manufacture very durable and excellent fishing lines, and also a variety of handsome and durable mats, which are used by both males and females as clothing.

The method adopted by the natives of New Zealand for the separation of the fibre from the other parts of the leaf, is as follows:—The leaves, when full grown, are cut down, the most perfect selected, and a lateral incision is made with a shell on each side of the leaf, so as simply to cut through the epidermis: the shell is then, with a gentle pressure, drawn from one of the incisions rapidly down the leaf, and is afterwards repeated on the other side: by this, the whole of the external epidermis is readily removed; the internal epidermis, which is of very thin texture, usually remains, but sometimes it is in the greatest part removed together with the external: the internal seems to unite the fibres of the leaf more intimately together, and if not cleaned from the flax when in a recent state, is removed afterwards with great difficulty, and, when it is suffered to remain, renders the flax less valuable as an article of commerce.

In the preparation of the flax, as well as in the manufacture of it into matting, the females are employed, and custom renders them very expert; before, however, the flax is manufactured into matting, it is previously soaked in water, and afterwards beaten, by which it becomes more pliable and soft.

The plant is named Koradi by the natives; and, when the flax is prepared, it is named Muka.

This plant alone would render New Zealand a valuable colony to the British nation: at present, the flax is used in England only in the manufacture of cordage, &c. but if the best varieties

indigenous to New Zealand were selected, (for they differ materially in the fineness of the fibre,) and proper care and attention bestowed on the cleaning, when in a recent state, it may, there can be no doubt, employed in the manufacture of linen of a very fine quality*.

PAPERS AND CORRESPONDENCE

ON CHOLERA.

AN INVESTIGATION OF THE EFFECTS OF TERRESTRIAL RADIATION, WITH REFERENCE TO MALARIA IN GENERAL,

*Indicating precautionary measures against
Cholera in particular.*

BY WILLIAM ADDISON, F.L.S.

Great Malvern, Feb. 2d, 1832.

THE diseases arising from atmospheric impregnations have long formed important topics of inquiry among medical men, because they are oftentimes the most fatal to which the human frame can be subject.

These atmospheric impregnations, not being cognizable by the senses, afford scope to various speculations regarding their origin, nature, propagation, and mode of operation: all writers seem agreed, however, in the existence of some subtle effluvia developing themselves with peculiar force under conditions not at present, perhaps, well understood.

Entering, then, upon a somewhat speculative investigation into subjects of acknowledged difficulty regarding the diseases of mankind, I deem it incumbent upon me to state fairly the facts from which it originates, and to declare my belief that it will indicate more clearly than has hitherto been

* In December 1829 I introduced some of these valuable plants at Oahu, Sandwich Islands, where I hope they will succeed: they were planted in Capt. Charlton's plantation at Wyderri, and had every prospect of succeeding when I left the Sandwich Islands on the 26th of January, 1830.

done, the *modus operandi* of MALARIA, and lead to precautionary or preventive measures worthy of mature consideration.

If we refer to the several histories afforded us by those who have had any experience in the disorders occurring from atmospheric miasmata, the reader must be struck with the fact that the baneful influence of these miasms is exerted peculiarly during the night season, whereas in the day-time they are comparatively harmless. This fact is so thoroughly established, that it would be unnecessary to refer to authorities for its substantiation.

Not only is this point clear and satisfactory, but another, apparently somewhat at variance, rests also upon the sure foundation of truth, viz. that all miasmatic effluvia are extricated from the surface of the earth by the heat of the sun.

From these two well ascertained facts, therefore, it would appear that mankind suffer incomparably less during the extrication of miasms than at some period posterior to their ascent from the ground into the air, and that this period is at night, when they descend into the lower regions of the atmosphere.

Neither is it at all difficult to understand why this should be the case, nor to reconcile the apparent variance between these two facts; for under many circumstances (marked by the occurrence of some peculiar fever, or other severe endemic) the miasms which have arisen in diluted portions during each successive hour in a hot day, and been carried upwards with the warm currents of the atmosphere, descend again at night with the cold air, and become concentrated at the surface of the earth.

Having thus noticed the two principal facts upon which the present investigation is founded, it may be advantageous, before proceeding further, to state the order of succession in the phenomena presenting themselves in relation with this subject, so that we may the better understand the *modus operandi* of malaria, and apply our remedial resources where they are calculated to be most useful and effective.

We have, then—

1st. The existence of certain matters upon the earth, capable of affording noxious effluvia.

2dly. The heat of the sun liberating these effluvia.

3dly. Their ascent into, or solution in the warm air; and,

4thly. Their descent, or precipitation at night, in a concentrated and highly dangerous form.

The first three points offer little for observation or remark here, because beyond the reach of our remedial resources. It is out of our power to effect much towards the removal of miasmatic matters from the earth, because in a very vast majority of instances we are ignorant of their nature or situation; or even should we possess that knowledge, they are spread over so great an extent that it is impossible to do so except in detail, and after a lapse of many years, by tillage and cultivation. Nevertheless, occasionally something may be done by strict attention to the clearing away of filth from sewers, to the draining off stagnant waters, and to the removal of noisome matters contiguous to our habitations.

So, I fear, we have too little controul over the powerful influence of a noon-day sun to adopt successfully any plan with a view to counteract its effect upon the various substances exposed upon the ground. The solar rays are too general, and will defy all our efforts to restrain them, or to obstruct the emanations they give rise to from the soil.

It remains, therefore, only to prevent the descent or precipitation of miasms at night in a noxious or virulent form.

Before we can attempt to accomplish this indication, it will be necessary fully to consider the phenomena which precede or promote a decline in the temperature of the air, particularly the terrestrial radiation of caloric, a process exercising a great influence upon the economy of nature, not merely with reference to vegetable but animal life, and giving rise to important modifications in local salubrity.

I have elsewhere entered somewhat into the details of this interesting branch of medical philosophy, which I deem it unnecessary here to recapitulate. I shall, therefore, on the present occasion confine myself to a few additional facts, illustrating and confirming the views already made public*.

* For a full account of the effects produced by the terrestrial radiation of caloric, in a medical point of view, the reader is referred to a "Dissertation on the Malvern Waters," &c. 2d Edit. Wilson, Prince's-Street, Soho; and also to the "Annals of Philosophy," Oct. and Nov. 1828, edited by Taylor and Phillips, where many interesting and important circumstances, having reference to local salubrity, are fully pointed out.

The effects of terrestrial radiation are—a decrease in the temperature of the air—a specific local temperature—and a specific local atmosphere.

When any portion of the atmosphere, or surface of the globe, is fully turned towards the sun, it receives more radiant heat than it projects, and becomes heated; but when this portion is turned from the source of heat, the radiation into space still continues, and being uncompensated, the temperature declines.

The declension of temperature resulting from radiation is accelerated in a still and clear atmosphere—diminished by a canopy of cloud—neutralized by currents of wind—and greatly modified by surface and situation.

Clouds materially lessen the force of radiation—prevent the escape of caloric—and are consequently desirable at night in situations exposed or liable to malaria: on the other hand, their presence during the day, obstructing the warmth of the sun, is not desirable. Aerial currents, and particularly high winds, act favourably, because they must dilute and remove all otherwise stagnant depositions.

It is on clear and calm nights that radiation proceeds with rapidity, producing specific local temperatures, and consequently, specific local atmospheres, the latter becoming dangerous in proportion as the former decrease.

These specific temperatures and atmospheres are owing to the various degrees of radiation, possessed by different surfaces, and exhibited in different situations: water, in any shape—lakes, rivers, ponds, marshes, and damp places—are all excellent radiators of caloric; so, also, are grassy, (or what might not improperly, perhaps, be termed *filamentous*) surfaces, as jungle and low underwood. In these places, therefore, the specific temperatures will be lowest, and the local atmospheres liable to fog, mist, and malaria.

To enable us to fulfil the important indication we have proposed, viz. to guard against, or prevent the descent or concentration of injurious miasms at night, it is obvious, from the foregoing remarks, that we should raise the specific temperature, and destroy the local atmosphere of a situation affected with malaria.

This, I conceive, may, in most cases, nay, perhaps, always be accomplished by

lighting LARGE FIRES, which, warming the atmosphere, and promoting aerial currents, will prevent the formation of a cold specific local atmosphere, and the consequent concentration of noxious miasms.

Experience here fortunately throws weight into the scale, and gives a colouring of truth to the views I have taken.

The lighting of fires to ward off the poisonous effects of malaria, is no new idea: it has been practised with success, though the principle upon which it acted has not, perhaps, been thoroughly understood, and consequently it may not have been employed to the best advantage. Dr. Macculloch tells us of a superintendant, engaged in directing the cutting of wood in Africa, who erected thirty furnaces on the spot where his men were employed. Before this he had always from forty to forty-eight of his workmen sick; but after using his furnaces, they were reduced in a short time to twelve, then to four, and finally to one. Napoleon adopted the same expedient very largely, and with success, when his armies were occupied in the worst districts of Italy. It is reported by Pliny that the Athenians were cured of the plague by lighting fires near the houses of the sick. “A friend of mine, (observes Dr. Burne) in travelling over the Pontine marshes, was struck with the rude health of the postmaster at one of the inns, as compared with the squalid creatures about him, and inquired how it was he preserved himself from the malaria? He replied, that it was his custom to kindle a fire every evening, and not to quit the house before the sun had risen some time, nor to remain out of doors after it had set.”

At this important period, when CHOLERA is progressing amongst us, it becomes highly interesting and very important to inquire how far the disease is dependent upon atmospheric peculiarities, and to determine whether the views I have taken regarding the circumstances favourable to the diseases of malaria, and the precautionary measures which they indicate, are beneficially applicable to this violent scourge.

So much has lately been written upon the subject of cholera, that one may well hesitate to hazard any opinion regarding it, and I intend to enter into the discus-

sion so far only as it may be connected with those precautionary measures arising out of the present investigation.

With regard to the atmospheric origin of the disease, few, I should think, will be disposed to deny—whatever may be the circumstances which have latterly attended its propagation—that the early history of cholera is quite sufficient to establish its connexion with local atmospheric peculiarity, and to shew that its attack is solicited by exposure to miasmatic air and nocturnal depositions.

The present epidemic seems to have originated in Jessore, the capital of the Sunderbunds, distant from Calcutta about sixty English miles. Jessore, from all accounts, would appear to be a crowded and filthy place, surrounded by impenetrable and marshy jungles, and consequently exposed to all the injurious influence of an unhealthy and malarious atmosphere. Speaking of this disease after its extension to the capital of Bengal, Dr. Young says, “the native inhabitants suffered so severely from its ravages, as almost to threaten the depopulation of the city and its crowded suburbs. In some cases, *nearly whole families, that had retired to rest in apparent health, were found in the morning either dead or dying; and in many others, several individuals of a household had perished between sun-set and sun-rise.* Many of the villages about Calcutta are placed in low swampy situations, and in many parts of the city are pools and ditches of water, on the margins of which, in the hot season, it is the custom of the natives to sleep during the night. In these situations the cholera raged with double and dreadful fury.”

I have been at some pains to look over the various documents referring to the circumstances attendant upon the origin and appearance of cholera in Europe, and (setting aside, for the present, any doctrines regarding contagion) it is evident, not only that the disease propagates itself with peculiar violence along the banks of rivers, in low marshy districts, and other places where malaria is known to prevail, but that, in almost all the cases I have seen reported, the first symptoms (where the precise period has been noted) have made their appearance during some part of the night. Whether this last circumstance, so fully esta-

blished as regards the tropical disease, is merely an accidental occurrence in this, further experience and a greater degree of attention to the point, must decide*.

Now, whatever may be the fate of my views respecting terrestrial radiation, and whether they are entitled to any consideration or not, I feel called upon to urge, at the present crisis, the attention of the profession to the facts which have long been before it—viz.

1st. That the diseases of malaria occur chiefly at night.

2dly. That cholera is essentially a malarious disease, coming on very often (most frequently?) between sun-set and sun-rise.

3dly. That large fires have been found beneficial in warding off the diseases of malaria.

Whence I infer that the same means resorted to at night will be found to mitigate the symptoms, and prevent the spreading of epidemic cholera.

Experience alone can decide whether this inference is founded in truth or whether it is not; let it, therefore, be brought to this test, and decided. In doing so, there may be many important particulars to be considered, which I have failed or omitted to mention, and which I shall be gratified to see pointed out: all I would ask is, that the want of any such may not prejudice a trial. The end is worthy of the means.

It remains now only to indicate the method by which the precautionary measures I have pointed out would be rendered most efficient. This is by arranging them into *public measures*, to be instituted by the proper authorities, and *private measures*, or those which should be adopted by families and individuals.

The public authorities of a town or place where cholera is raging (I would propose)—

1st. Should prohibit, as far as they are able, any traffic along the streets after night-fall; or, at all events, *recommend* its cessation, except for particular and urgent purposes.

* Mr. Searle has informed us, that the Polish army, having slept *one night* in a marsh, had immediately fifty cases of cholera. Again: after some rain and stormy weather at Vienna, the cholera broke out with the most alarming violence *on the night* of the 13th September, and swept off, in the course of twenty-four hours, eighty persons; having prevailed in a milder degree previously.—Vide Medical Gazette, vol. i. 1831-32, p. 94.

2dly. They should cause *large furnaces* to be lighted at night, in those situations most obnoxious to the disease—even in the streets. These furnaces ought to be shallow, exposing as large a heated surface as is consistent with their removal in the day-time. They should be constructed for the especial purpose, and kept burning from sun-set to sun-rise. The fuel consumed might be coke. These fires would be most required in the ill-lighted, low, and close streets; in places on the banks of a river, and on the confines of the town. The inhabitants of the better streets would, I conceive, be in some measure protected by the numerous gas lights, the heat of manufactories at work during the night, and by the columns of heated air ascending, during a great portion of the night, from the chimneys of the more wealthy.

Individuals living in a place where cholera has made its appearance, should be very cautious of exposing themselves, except in cases of necessity, to the night air; every window should be closed an hour or two before sun-set, and fires kept burning in the different apartments during the night. These will not only be measures of protection to themselves, but, if generally promoted and put in force, will be of great public benefit; for, as I have before observed, the columns of heated air, ascending from a great number of chimneys throughout the night, must have a powerful influence in checking the deposition of injurious nocturnal miasms.

ACCOUNT OF THE CHOLERA AS IT APPEARED AT SMYRNA.

To the Editor of the London Medical Gazette.

SIR,

THE accompanying is a translation I have caused to be made of a report sent by the Belgian Consul at Smyrna to his government, relative to the cholera which raged there last autumn. As the subject to which it refers now engrosses a great share of the public attention, you may perhaps not think it unworthy of a place in your highly-valuable pub-

lication, in which case it is quite at your service.—I remain, sir,

Your obedient servant,

H. CASTELLAIN.

Belgian-Consulate,
London, 7th Feb. 1832.

Remarks on the Cholera which raged at Smyrna from the 25th Sept. to 10th Nov. 1831.

During the whole summer of 1831, the cholera ravaged the middle and eastern parts of Russia; in June and July it reached the mouth of the Danube; and in August, after the famous fire at Pera, it disappeared from Constantinople, and keeping along the coast of the sea of Marmora, it at length in September reached the Dardanelles and Enos.

We learned at Smyrna, in the month of September, that the great caravan of Mecca had been attacked and dispersed by a current of cholera coming direct from India, and that having entered Egypt, after having broken through the sanitary cordon established at the Isthmus of Suez, this current had reached Cairo and Alexandria in an incredibly short space of time, and was making frightful ravages in those places. We also heard that several vessels coming from Alexandria, and on board of which several deaths had occurred, were in the neighbourhood of Chesmé. An American corvette of war, which had taken the cholera at Constantinople, landed her sick, and performed quarantine at some islands in our gulph; in fine, a report was spread that a new disease had appeared at Brutia, and at Magnisa of Sypila.

Surrounded, in a manner, by the disorder, it would have required little less than a miracle to enable the town of Smyrna to escape it, even if its germ had not already existed, for since the month of July several cases of sporadic cholera had been observed. Persons apparently in good health suffered daily attacks of colic, and it was impossible not to perceive a general predisposition to diseases of the bowels and of the stomach. From that time, the Europeans of the better class commenced using the precautions recommended in the German newspapers, and although the weather was still very warm, they left off eating fruit, and covered themselves entirely with flannel.

At length, on the 25th Sept. a man died of a disease which was recognised as the true epidemic cholera by Dr. Clark, formerly physician of the British army in India. The day afterwards, and the following days, an immense number of persons emigrated into the country, certainly not less than 40,000, or about one-third of the population of the town. This diminution was of considerable service, as it exempted from the disease those who had fled, and enabled those who remained to be less confined.

From the 25th September to about the 4th or 5th October, may be called the first period of the disease. It seemed principally to attack the brain and nervous system. Those who were attacked fell down deprived of all feeling, and often even of life, and they neither experienced colic, purging, or vomiting. The patient either died, if assistance were delayed for a few minutes, or he was out of danger in a few hours if he had been warmed and rubbed with a mixture, the recipe of which was given in the German newspapers, and which was composed of spirits of wine, vinegar, pepper, garlic, essence of mint, &c.; but bleeding preceded all other remedies, and the following mixture was administered internally in one dose.

No. 1.—Sulphuric ether, 1 drachm; essence of mint, 10 drops; laudanum, 50 drops; tincture of capsicum, 30 drops; cogniac brandy, 1 ounce; water half an ounce.

The following prescription had likewise very good effects, but was not so much used, owing to the difficulty of procuring cajepout oil in most of the chemists' shops. It was composed of—

No. 2.—Tincture of capsicum, half a drachm; laudanum, 2 drachms; cajepout oil, half a drachm. Mix, and give 60 drops every half hour while the coldness continues. The patient being placed in a warm bed, if the extremities were not warm in five minutes, 3 pills were administered according to the following recipe, and continued every four hours.

No. 3.—Opium, 16 grains; calomel, 80 grains; cayenne pepper, 24 grains. Mix and divide into 24 pills.

About the 4th or 5th October, the nature of the disease was altered; bleeding was less frequently necessary, and friction was scarcely used any longer. The attack commenced with sudden ex-

cruciating pains. Incessant vomiting and purging reduced the patient in a few hours, to the brink of the grave, but assistance speedily rendered produced the most beneficial effects. Still, from the 19th to the 23d October, both inclusive, during which period the disorder was at its height, out of *eleven* cases, *nine* were mortal. This second period of the disease terminated about the 26th or 27th October. During its continuance the following remedies were generally and effectively administered:—

Bleeding and leeches on the epigastric region, according to the constitution of the patient.

No. 4.—Brandy, 2 ounces; laudanum, 50 drops; essence of mint, 10 drops. Mix, and give the entire dose every half hour, and every four hours the pills No. 3.

Or else—

No. 5.—Laudanum, 20 drops; mixture of camphor and peppermint water, of each half an ounce. Mix, and give the entire dose every three hours.

Or else—

No. 6.—Prussic acid, 2 drops; laudanum, 20 drops. Mix, and give the entire dose every three hours, and in case of very violent vomiting—

No. 7.—Nitric acid, 1 part; water, 3 parts. Mix, and apply to the pit of the stomach.

N.B.—No. 5 for slight attacks.

Then commenced the third period, which lasted until the 10th November, after which time we may say that we were free from this terrible scourge. In this third period the attack commenced with slight diarrhœa, which was very easily stopped by the following mixture:—

No. 8.—Conserve of opium, 3 drachms; prepared chalk, 1 drachm; powdered gum arabic, 2 drachms; peppermint water, 4 ounces. Mix, and administer one quarter every three hours, and also one or two *lavements*, composed of—

No. 9.—Starch, 12 ounces; laudanum, 60 drops.

The same *lavement* was also administered when it was found impossible to keep the opium on the stomach.

But should the patient have neglected these remedies, the insignificant diarrhœa was, in the course of four or five days, suddenly converted into an attack of cholera perfectly incurable.

During the second and third periods of the disease, when the patient experienced the general symptoms of exhaustion and suffocation, and when the whole system was deranged, the two following prescriptions were productive of great benefit:—

No. 10.—Liquid acetate of ammonia, 4 ounces; tartrate of antimony, 2 grains; laudanum, 80 drops; peppermint water, 4 ounces. Mix, and administer one-fourth part every three hours until the fever and vomitings cease.

No. 11.—Laudanum, 100 drops; prussic acid, 12 drops; carbonate of ammonia, half a drachm; tartaric acid, half a drachm; syrup of lemon, half an ounce; water, 6 ounces. Mix, and give two table-spoonful every hour until the fever and vomitings cease.

These two prescriptions always produced profuse perspiration, which was very beneficial.

Having had the perhaps dangerous honour of being named one of the five directors of the society which was established at Smyrna at the commencement of October, for the purpose of assisting the poor of all nations, who might be attacked with cholera, and not having quitted the town a single day during the seven weeks in which it prevailed there, I have gathered the following facts: they are not *medical* observations, but in the hands of active and zealous medical men, they may perhaps not be entirely useless to humanity.

At Smyrna the cholera made the round of the town, proceeding in a manner by *nations*. First, the Jews, who form the poorest, most timorous, worst fed, clothed, and lodged class, were attacked, and among these it was chiefly the females who perished. May not this be attributed to the circumstance of their walking with their throats, and part of their chests uncovered, while the men wear a large girdle, and a long waistcoat buttoned up to the chin? The number of Jews who died was not less than from 800 to 1000, out of a population (remaining in the town) of about 12,000 persons.

The disease then passed to the Greeks, of whom 2000 out of 30,000 died.

It then extended itself to the Turks and Armenians, who lost 2500 or 3000 out of 45 to 50,000.

At length it reached the Franks, and although in a population of 9 to

10,000 souls there are of course many persons very badly off in every way, still only 60 died, and of more than 7 or 800 individuals belonging to the families of merchants and persons in easy circumstances among these Europeans, only four died!—two of them in consequence of not having called in a physician until 12 hours after the commencement of the attack, and these were, besides, aged persons; a third, also an old man, in consequence of having neglected a diarrhoea brought on by fright; and the fourth, a lady in the family way, who had indulged her appetite too much, from an idea that nothing was hurtful to one in her situation.

Our diet, during the continuance of the disease, was broth, and boiled or roast beef, and our beverage a small quantity of wine, or brandy and water. Many persons did not entirely abstain from eating cooked vegetables. We also wore flannel on the head and feet, and a large girdle of the same round the stomach.

If I might be permitted to express an opinion on a disease so extraordinary as cholera, I should say that it was an affection of the body existing through, or brought on by, a defect of regimen, by checked perspiration, or by fear,—all of which produce on the stomach the effect of indigestion,—and that it was not in itself either contagious or infectious; for, whilst not one of us was attacked, a number of our servants, who would not submit to the precautions recommended, suffered more or less, and yet they slept under our own roof, and were very frequently attended by ourselves alone.

Disinfecting substances do not appear to have had any efficacious results. The house which I inhabited was washed with chloride of lime, and all the apartments filled with the odour which proceeds from it, when one of the maids was attacked; but this woman was a Greek, and had submitted to a fast of three days ordered by her priests, contrary to the advice of the physicians, in order to appease the Divine wrath.

This fast, during which it was not permitted to take any thing but aniseed tea, was extremely prejudicial, and furnished an immense number of victims to the disease.

Dr. Clark attended very strictly to regimen, and his house was filled with disengaged chlorine. His wife was at-

tacked, nevertheless,—merely, as it appeared, in consequence of terror.

Again, we may suppose that cholera is not contagious, since not one of our medical men died, although they all saw, touched, and attended to, during seven weeks, thousands of persons who were attacked. Candour, however, compels me to state, that two among them were slightly attacked, but one had been eating pomegranates the evening before, and the other was suffering from severe domestic affliction.

It was generally remarked, that pregnant women, or those who were labouring under the derangement of the system peculiar to them at times, were extremely predisposed to cholera, and therefore, under either of these circumstances, they require to take particular care of themselves.

Although the disorder may be said to have left us now for more than a week, there is still much to fear, and the regimen must be for the present strictly adhered to, for yesterday evening two Aleppo merchants, having supped on bread and nuts, had an indigestion, and this morning one of them died in six hours, and his companion is in the greatest danger. Nuts have been fatal to many persons before the last-mentioned accident.

Wine, mixed with Peruvian bark, has been recommended; say two small glasses in the course of the day. One should also take particular care to avoid night or early morning air, and all sorts of damp.

Fear being a great exciter of cholera, the inhabitants have been advised to carry about them different odoriferous substances, such as peppermint oil, camomile, camphor, chloride of lime, solid opium, &c. But, in fact, the physicians do not really believe in the repelling power of these ingredients; they merely look upon them as a species of anulet, fit for tranquillizing the minds of the timid, and for keeping persons in as much security as possible.

On feeling the least pain in the stomach or bowels, or the slightest internal cold, it is prudent to swallow two or three drops of peppermint oil on sugar.

In fine, cholera morbus is not after all so terrible as it appears, as it is always curable if taken in time, and only attacks those who give way to fear,

commit any excesses, or infringe on the measures of precaution already taught by experience.

(Signed)

A. DETHIER.

Smyrna, 18th November, 1831.

ON THE
USE OF ALKALINE SALTS
IN THE

CURE OF TYPHUS FEVER, CHOLERA
ASPHYXIA, AND OTHER IMPOR-
TANT DISEASES;

*Tending to prove the Identity of the Causes of
these Diseases. Illustrated by Cases.*

*To the Editor of the London Medical
Gazette.*

SIR,

IF in the following very brief and imperfect sketch of the immediate or proximate causes and effects of *gastric irritation*, I should presume to treat the opinions of some learned physicians with a very superficial notice, I beg to premise, that it will not be from an insensibility to the importance of their opinions, but because it seems inappropriate in me to occupy your pages with any discussion that has not for its immediate object the elucidation of my principles of practice, which I now, with every feeling of respect, submit to the consideration of the medical profession. When I come to treat of the cases that have occurred in my practice, it will be seen that the *same class of medicines, by relieving gastric irritation*, have removed a series of disorders, ranging from simple headache to impending apoplexy, and from the slightest febrile excitement to the worst form of typhus, and that too although the disease had advanced to its last stages before the treatment was commenced; instances of which I have preserved in the cases of Scholes's son, and Harsof's son.

Debility is one of the first symptoms of an attack of typhus fever, then anorexia, an inclination to vomit from slight causes, headache, giddiness, the appearance of objects floating before the eyes, pain of the epigastrium and hypogastrium, together with great tenderness of the intestines, as may be readily as-

certained by gently handling the parts; added to these symptoms there is frequently an unpleasant taste in the mouth, and the fecal evacuations are sometimes liquid and frequent, with some similitude to harm. Now what are all these symptoms, which are always more or less the precursors in *typhus*, but an assemblage of those we so often meet with from acidity in the stomach and bowels; or, in other words, from *gastric irritation*? Let these symptoms be more intense, and thereby the circulation and sensorium more influenced, and a continued action induced, and we have *typhus fever*.

Now if we glance at the *treatment* of typhus fever, as well, indeed, as of inflammatory fever, after paying especial attention to local congestion, we shall find that *that which is most effective in relieving gastric irritation* is the most successful practice; and, on the other hand, *that which is not calculated to relieve gastric irritation*, as well as that which is calculated to increase it, is the least successful practice—as witness the revolting Brunonian system.

That the most effectual practice is precisely in proportion to the relief that is afforded to gastric irritation, is abundantly evidenced by what we witness in the more successful practice of all physicians; of whom a very large majority give an emetic if they see the fever on the first attack, by which matters offensive to the stomach are ejected; the relief being in proportion to the effectual action of the emetic. I do not forget that Cullen imagined that the extreme vessels were affected with spasm, and that the emetic by its shock relieved this state. This I never understood; but I can readily conceive how the stomach, and the system at large, as sympathizing powerfully with it, can be relieved by being unloaded of irritating matters. So is calomel, judiciously administered, an excellent medicine to allay gastric irritation; indeed, it is frequently of the first importance; and therefore, in skilful hands, we find it very effective in the cure of fevers, especially where the inflammatory character predominates, as contra-distinguished from that peculiar loss of energy in the genuine typhus. Thus, in the plague, we are told by Dr. Millingen that the greatest benefit was experienced by the use of the aqua lauræ cerasi, and the acid.

hydrocyan., medicines of all others that have been effectual in curing certain diseased states of the gastric system, when all others have failed. I am aware that some facts seem, on a superficial view, to disprove the soundness of my inferences; and I will instance one of the first that occurred in my professional career. About twenty-seven years since, the typhus fever was very fatal in some parts of London. At that period, the death of the surgeon of the parish of Lambeth threw a great deal of the parish practice into my hands, being at that time a senior apprentice, and we had a great number of typhus cases in the workhouse, assuming in the onset chiefly the type of the synocha of Cullen. We gave to young strong patients a solution of Ant. Tart. in such proportions that it vomited briskly at first, but after a few doses it generally remained and acted on the bowels; and I do not recollect that we lost a patient; yet at the first view the Ant. Tart. seems an unlikely medicine to allay gastric irritation; but if we bear in mind that this irritation was occasioned by the (morbific?) contents of the stomach and bowels, and that these contents were by this remedy speedily and effectually removed, we shall see that it had a direct effect in allaying the peculiar irritation. The evidences hold good, too, as to the ingesta: in typhus fever food of all kinds is loathed, and we find that no beverage is at the same time so grateful and refreshing as good cold water, or toast and water; lemonade, and other beverages acidulated with vegetable acids, so very generally given, delight the palate of the patient equally, or perhaps more than the water; but he who has anxiously watched by the sick one's side, cannot fail to have remarked that this gleam of enjoyment and renovation is quickly succeeded by the prior, or a greater languor and restlessness. At first the languid sufferer seems to revive, his look is grateful, perhaps somewhat cheerful; but the faint gleam dies away rapidly, and the eye of the unconscious sufferer becomes again quite void of intelligence, and a state of restlessness succeeds; but this does not occur when good cold water, toast and water, or, I may add, weak and cold black tea is given. Less gratification may be manifested on drinking, but the effect is to tranquillize. Why is this?

Do not the vegetable acids augment the mischief by increasing the acid mass in the stomach, and thus for a time allay the gastric irritation? On this principle, in the treatment of typhus fever, I always prohibit all vegetable acid beverages, and all preparations of fruits. Before I adopted this practice, I used to give cold water, acidulated with the acid. muriat. which appeared to me to be of permanent benefit in many cases; and this admission I take to be no weakening of the main object of my argument, but on the contrary, as supporting it, since it is a fact well ascertained that the mineral acids, properly administered, act as tonics, and correct the tendency of the stomach to acid formations; and taking this into consideration, and in the faith that what has been said in another place of the use of the muriate of soda in the more formidable fevers, I should not hesitate to give the remedy I chiefly rely on—the Carbon. Sodæ, as a medicine, and the Acid. Muriat. as a beverage, at the same time, in typhus fever.

From this slight pathological notice let us turn and briefly contemplate the astonishing arrangements of the nervous system between the stomach, the brain, and the spinal marrow, the lungs and the heart, and the whole abdominal viscera. We cannot fail to be forcibly struck by the large dépôts of nervous energy evidenced by their numbers, size, frequent unions, and *perhaps* by the series of ganglions that we find in succession from the neck to the solar plexus on the diaphragm; their frequent connexion, not only with nerves from the brain, but also from the spinal marrow. Is it possible to survey this wonderful and beautiful apparatus and not perceive how readily any offensive agent, any excess of stimulus acting on any one of this great family of nerves, may throw the whole into disorder? and is it possible not to perceive that the stomach is the most palpable point to which obnoxious matters may be applied; and that, consequently, on every account it must be the most frequent source of disturbance and injury? If any one doubts this, let him witness the intimate and direct connexion of the ganglionic nervous system, but more particularly the nervus vagus with the stomach, the direct connexion this nerve has with the great arteries going to the heart, as well as

with the arteries of the liver; its indirect connexion with the great arteries going to the head, &c. &c. evincing at once whence arises the increased action of the blood-vessels when the energy of the nerves of the stomach are excited; and how it is that the stomach, head, heart, and large arteries, shew such powerful sympathy when nerves remotely, though intimately, connected with this family, are greatly excited, as in dysmenorrhagia, or by blows or slight pressure only on certain parts of the spinal column, when inflammatory action is going on within its canal.

Taking into consideration the various facts and analogies I have adduced, they lead me to be of opinion that the immediate, proximate, essential cause of fever, depends on a derangement, or a disturbance of the functions of the nerves distributed to the stomach, sometimes produced immediately by offensive matters contained in the stomach, and at others mediately, that is, through the nervous influence induced by mental emotion, as excessive grief or fear, or by noxious air taken into the lungs, as has repeatedly happened from the inhalation of effluvia from old stinking gutters, and other similar dépôts of decayed animal and vegetable matters; or from all or any of the three causes united. If any one thinks these causes inadequate or incongruous, I say again, let him examine into the astonishing nervous apparatus which has been referred to, and its intimate connexion with the great arteries going to the head and to the heart; and as evidence of the phenomena they are able to produce, and how surely they do occasionally affect the head, stomach, heart, and large arteries, even in a most frightful manner, I beg to instance the case of dysmenorrhagia. In this we first have tortures referrible to actions of the uterus; this is transferred, and, whilst the patient writhes in agony from spasm of the stomach, suddenly it is changed to spasm of the heart. So overwhelming is the sensation of swelling, she dreads her heart will burst; her manner is of one hurried and terrified: a frightful relief, however, comes; suddenly she starts with a wild look of intense alarm, moaning inarticulately, "I die—I am dying:" she sinks motionless, the heart has collapsed, and she experiences a wretchedly melancholy feeling of blood rushing from the

heart, and rapid annihilation: she seems sinking under these accumulated horrors, when another attack is suddenly and unexpectedly made, the collapse ceases, and violent spasm of the subclavian arteries succeeds, causing the most painful distention of the blood-vessels down the whole arm and hand. But though she expresses her fear that the veins will burst, yet she experiences relief and comparative satisfaction; when again a frightful shriek of acute agony, too severe to admit further utterance, the seat of pain only indicated by one hand clasped on the head, the other grasping some support with convulsive effort, the veins of the neck frightfully swelling, the eyes suffused, and the features distorted and twitching, all indicate the agonizing pain that darts across the brain, throwing the whole frame into convulsive contortion. All this I have seen in the brief space of a few minutes, and not once, but repeatedly, arise from nervous irritation in dysmenorrhagia, shewing the rapid transition of irritable nervous impulse from one extremity to the other of the ganglionic nervous system; and it is a fact, that sufferers of this class are greatly afflicted with *gastric irritations*.

[To be continued.]

NEW MODE OF APPLYING HEAT IN CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

I AM anxious, through the medium of your pages, to give publicity to a simple mode of conveying a patient from the spot where he may be assailed with cholera, either to his own residence or to some asylum appropriated to the reception of such cases. I propose having a common portable seaman's cot, fitted up with a folding counterpane, or quilt, which has tubes of India-rubber introduced, capable of containing hot water, and so adapted to the cot that the patient may lie on the one-half of the quilt whilst the other is folded over him. Such an apparatus might be kept at each police station-house, and be always ready on application to meet the emergency. By its employment, the patient might derive the twofold advantage of commencing a process of

cure during the period of conveyance to his destination. The patient being wrapped up in a blanket, would ensure the heat being applied with safety to the body; and one placed externally, would, as a non-conductor, tend to retain the heat more effectually.

I have the honour to be, sir,
Your obedient humble servant,
A. R. SUTHERLAND, M.D.

1, Parliament-Street,
Feb. 16, 1832.

CHOLERA IN SOUTHWARK.

To the Editor of the London Medical Gazette.

SIR,

THE following case of cholera having just occurred, and presenting a few features worthy of notice, you will perhaps think it sufficiently interesting for insertion in this week's number of your journal.

I am, sir,
Your obedient servant,
CHARLES GASELEE.

211, High-Street, Southwark,
Feb. 15, 1832.

*Case of Malignant Cholera in Southwark—
Patient recovering.*

Frank Burns, æt. 13, residing in Winchester-Street, near the Borough Market, is an intelligent lad, health hitherto good, though he has not been to his school (Cripplegate) since last Wednesday, on account of feeling unwell. He has had slight diarrhoea for the last two days, but has played with his brothers and sisters as usual, and yesterday eat his accustomed Sunday dinner of pork and cabbage, and went to bed well; he awoke this morning (Monday, Feb. 15), and feeling ill and faint on rising, he again lay down for an hour, about which time he was seized with copious diarrhoea, followed by "green" vomiting. This part of his history is not very clear, as his mother was out at nurse, and a younger sister was to be trusted to for the account. About noon his mother, being informed of his illness, came to him, and in a state of much alarm carried him to St. Thomas's Hospital, where she could not get him admitted, but an anodyne draught was given him. I first saw him a little before three p.m. The appearance of his face was striking; its colour ghastly; features collapsed, and expressive of suffering; eyes very much sunk and turned up, and surrounded with a livid areola; pupils contracted. His trunk was the warmest part, but the extremities and face were cold; his breath was cool,

and his tongue felt cold to the finger, looking flabby. There were occasional short spasms of fingers and toes, and less frequently of the thighs, the ends of the fingers being corrugated and semiflexed. He was excessively depressed, no pulsation being felt except at the heart, which was labouring feebly at 90 beats in the minute. He has just had one small evacuation, serous, and containing a few small white flocculi, and exhibiting alkaline properties; he has not passed any urine for several hours. Owing to his destitute condition, some time elapsed before a fire and other domestic means could be rendered available. He took now—

Sp. Ammon. Aromat. ℥ss.
Myristicæ
Aquæ, aa. ℥ss.
Vin. Opii, ℥xx.

And having instantly rejected it, it was repeated in about ten minutes, and retained. He was now seen by several medical gentlemen, all of whom were satisfied of the reality of the disease. His bed was placed before the fire, and the pulse at the wrist could be just felt beating at 130 in a minute; he was restless, never remaining long in the same posture, and sighing frequently. At the suggestion of a gentleman from the Central Board of Health, a mustard emetic was given ($4\frac{1}{2}$), but was immediately rejected with considerable force, and he was afterwards more depressed.

Eight P.M.—Since last report he has not varied much; he lies for the most part quiet, but now and then rouses, and evinces considerable muscular exertion. No cramps since four o'clock; skin feels relaxed and cold on the chest, legs, and arms, but the abdomen retains its warmth; if he is exposed to the fire and rubbed so as to feel warm, and then covered with a blanket, he becomes immediately cold again. The following enema has been administered:—

Brandy, ℥ij.
Water, ℥j
Vin. Opii, ℥xx.

but was only retained for two or three minutes. He has likewise taken at intervals—

Sp. Ammon. Arom. ℥ss.
Myristicæ, ℥ss.
Aquæ, ℥ss.

but brandy is instantly rejected.

About half past eight, an improvement having taken place, a vein in the arm was opened, but only a few drops of blood flowed; and Dr. Cobb, who came in at the time, thought it inadvisable to persist in it, and preferred pursuing the stimulating plan only; and with this view recommended the following enema:—

Brandy, ℥ij.
Aquæ, ℥ij.
Vin. Opii, ℥j.

Hot gin and water were given him likewise at short intervals.

Ten P.M.—Lies senseless, unless roused; features more collapsed; sclerotic tunic of eye much suffused; pulse 112, very weak indeed.

Midnight.—No improvement; heart's action oppressed; pulse 120, feeble and fluttering; tongue flabby; general expression more sunk; areola of more leaden hue round lips and nose. His fate was now considered certain; several gentlemen within the last hour having left him with the impression that he was moribund.

As every thing looked so unpromising, abstraction of blood was determined on; both his jugular veins were opened, a vein in the foot and arm likewise, and about five ounces of dark highly-carbonized blood were drained and squeezed away from him. During this the pulse rose to 144, and was very weak and small; but it soon fell to 108, and the face became less pinched and warmer; he struggled much during the bleeding, but soon relapsed into a state of insensibility, the breathing being oppressed, and varying from 12 to 16 in a minute. He took frequent small doses of sp. ammon. arom. and sp. myristicæ; and at one time, after taking some gin and water, he vomited, and raised himself up in the bed during the effort. The coma again came on, and he was perfectly insensible, most probably from the opium. An enema of warm salt and water (about a quart in quantity) was now given; and during the injection of it (it was done by a stomach syringe) the pulse rose perceptibly in power, and was from 112 to 120; the skin became warmer, respiration 20-22. The power of deglutition was now wholly suspended, and he was in a profound lethargy, the tongue and breath being warm. Pupil much contracted; the sclerotic tunic of eye greatly suffused. Having left him at two A.M. on Tuesday morning, desiring to be called if he awoke, I was sent for at half-past five, he having been then awake for an hour.

Seven A.M. I took the following report:—Countenance is regaining its fulness; legs and body very warm and dry, as are also the arms; tongue dry, brown, and furred; urgent thirst. At five he expelled the enema, and passed a small, brown, fluid motion, mixed with mucus, and of a slightly feculent smell. His pulse was 120, and weak; pupil much contracted; eye less suffused. Has just jumped out of bed, and tried to seize a jug of milk and water to allay his thirst; fauces and throat parched; very slight headache; little uneasiness in each hypochondrium.

Cal. gr. ij. omni horâ ad 4tam vicem.

To have milk and water as a beverage.

One P.M.—Has taken four doses of calomel; has had another motion of same character, but mixed with several bloody specks; and he has slight hœmelyache, chiefly about

epigastrium. No sickness nor headache; pulse the same; tongue not so dry; skin not so harsh.

Pulv. Jalap. gr. x.
Pot. Supertart. ℥j. statim.

He rejected this immediately, and with this a little green bile; and he always is sick if he takes jalap. An hour after this he took—

Ol. Ricini, ℥ss.
Liq. Opii Sed. ℥ijj.

and retained it; and repeated it at five P.M. He has taken some beef tea this evening.

Eight P.M.—No evacuation; has passed about a table spoonful of natural looking urine; skin hot, but soft; pulse 120, rather jerking; much thirst; tongue rough but moist. Is sleepy from fatigue; the sclerotic rather suffused; pupil more dilated; face rather flushed; belly soft; thighs ache; general expression of face of natural fullness, and tranquil; breathes hard, but is hoarse. About nine he passed a good deal of offensive flatus, and a very small, brown, mucous stool, of an offensively feculent smell. Is to take gruel occasionally.

Eleven P.M.—Is sleepy, as if fatigued; skin not so hot as in the morning; pulse 116, with good action, and rather full; breathes easily; countenance more natural than it has been since I first saw him; no more evacuation; is easy, and inclined to sleep soundly.

To take when he awakes, Ol. Ricini, ℥ss. and to repeat it in the morning if necessary.

Wednesday, 15th.—Slept a good deal through the night; about four o'clock he passed a fluid brownish stool, feculent in smell, but of no consistence; no headache; skin warm, but by no means dry; tongue moist, but rather rough; thirsty; lies inclined to sleep; has passed more urine with his last evacuation than he has done at any previous time; pulse 120, of good character.

Two P.M.—He is now proceeding favourably, and there is every prospect of a successful termination.

REMARKS.—The plan of treatment first intended in the foregoing case, was the exhibition of stimuli and the abstraction of blood, conjointly; not anticipating any necessity for attending principally to the discharges. We were dissuaded from pursuing the abstraction of blood (which had been commenced) by the advice of a gentleman, as is related; and, in deference to his opinion, stimuli and opium were administered. The anodyne, it will be agreed, fully influenced the system, but the remarkable feature of the disease—"the depression,"—was on the increase, and at night the original plan was resumed.

I pretend not to determine what was beneficial; though, if I might risk an opinion, it would be, that reaction followed as a consequence of the relief given to the circulation by the abstraction of even the small quantity of blood.

Next morning, the blood spilled on the bed more resembled pitch than blood. He took, during the first eight hours, about three-fourths of a pint of spirits.

I cannot conclude this case without including in it the name of my friend, Mr. Tweedie, as a tribute due to him for the perseverance with which he assisted me during its whole course.

Continuation of the Case up to Noon, Feb. 17.

Feb. 15, eight P.M.—A nitric acid julep, which was prescribed this morning, has proved grateful. At 4 o'clock he had a pretty copious feculent dejection, highly offensive, and containing a large quantity of bile. He is still sleepy. Pulse 108, soft; tongue moist, and somewhat white; skin warm and perspirable; expression happier; less thirst.

Has taken beef-tea. Tea and gruel.

16th, eleven A.M.—Passed a good night, and has had three bilious feculent stools; is much less thirsty; tongue moist, and much more natural; skin a little above the natural temperature; pulse 88, soft, drowsiness not yet worn off; no uneasiness about the head; urine free.

Is directed to take a light bread pudding, and mutton broth.

Eight P.M.—Instead of the pudding which was ordered, some "batter" pudding has been given him, but it instantly made him sick. Continues sleepy; there is no alteration, except that his tongue is more furred, and disposed to be rough and coated; two evacuations of the same character; urine pale and free.

17th, twelve o'clock.—Passed a good night; skin cool; pulse 74, free and soft; tongue moist, and though a little furred, is much less so than it was last night; thirst very much abated; urine free; bowels open. Complains of tenesmus; voice, which has been hoarse, is now more natural; he complains very little to-day of the pharynx, which has hitherto been in pain during deglutition.

R. Cretæ Præp.
Pulv. Gum. Acac.
Conf. Aromat. aa. gr. v.
Aque, ℥ss.

3tiâ q q. horâ sumend. si tenesm. urgent.

P.S.—The application of a mustard poultice to the epigastrium on Monday night

has not been mentioned in the report; it produced no effect. A rumour being in circulation that the boy had eaten largely of carrots before his attack, it may be right to add that such a report is incorrect.

The origin of this lad's disorder is involved in an obscurity which it perhaps is impossible to unravel. He has been in the habit of playing about the market, and with other boys in the neighbourhood, and has also, during his holydays of the past week, been amusing himself with the workmen of the old London bridge, being likewise with them in their boats, &c. &c.

Whether he has been exposed to any infected cause is undecided. His father has been working in the West India Docks up to the latter part of the past week, and has come home in the evening through Limehouse, Ratcliffe, &c. He likewise had a diarrhoea at the time of his son's seizure, which, however, subsided after a cordial. Another son has had a slight relaxation of the bowels; and his sister, having been similarly affected, took last night a eretaceous anodyne draught, as her diarrhoea was increasing, and she was complaining of headache and abdominal uneasiness. She is to-day quite well.

C. G.

Friday, Feb. 17th, 1832.

CHOLERA AT MUSSELBURGH.

To the Editor of the London Medical Gazette.

SIR,

I AM happy to inform you the mortality at Musselburgh has been daily diminishing, though there are still a great many on the sick list. It is only in a few cases that the disease is so clearly marked, as I have described it in my last letter. In many there have been precursory symptoms, as head-ache, dyspepsia, and gentle looseness; these were, in some instances, succeeded by the irritable stage in which, if the bowels had been relaxed, vomiting followed, accompanied by symptoms of prostration. In the worst cases the patient was usually attacked without any previous warning, with all the severest symptoms of the second stage, considering himself in a state of health, till severe diarrhoea, succeeded by vomiting, came on, or till spasms commenced, and became gradu-

ally more intense: in these extreme cases death generally closed the scene in less than twelve hours. The second stage assumed two different forms, the paralytic and the spasmodic; the spasmodic usually attacking the young and middle-aged, and the paralytic those advanced in life and under ten years.

As I was at Musselburgh on Saturday, February 11, during the entire day, and had an opportunity, through the polite attention of Mr. Rutherford, of seeing a great many cases, I shall take the liberty of sending you an abridged statement of some of them, which I took down at the bed-side of the patient, in company with Mr. R.

CASE I.—A man, past sixty, was admitted into the hospital this morning—has been complaining for a week of general debility, and has, during this time, been relaxed in his bowels. On admission his surface was cold, the hands and feet shrivelled, and rather livid, especially the last joint of the fingers; his eyes were sunk, erected, and surrounded by a diffuse lividity; his lips were rather blue, and his expression was anxious and ghastly; the pulse of radial arteries was weak, about 60—of carotid and femoral at the groin fuller, but imperceptible in anterior and posterior tibial; the heart's impulse is weak, the respiration laborious, and, during inspiration, painful. He complains of spasmodic pain at epigastrium and umbilicus, and of cramps in the muscles of inferior extremities. He was very restless; he appeared to be gradually sinking when I left the hospital; the expression was more anxious, and the pulse more feeble. Treatment: friction with coarse hair-cloth and warm oil of turpentine, heated bottles to lower extremities, and warm brandy and water.

CASE II.—A woman, aged 40, was received into the hospital at five in the evening: she felt unwell in the forenoon, but had no previous purging or vomiting. The symptoms on admission were, face very cold, cephalalgia, pulse very feeble, no purging or vomiting, cramps severe of lower extremities. Treatment, a pint of water at temp. 32 Reaum., containing 30 drops of tincture of opium, and 15 drops of tincture of camph., was transfused, in a very able manner, by M. Delpech. No blood was previously abstracted, but one or two drachms trickled away from the open orifice of the vein. The woman

seemed to revive for two or three hours after the operation, but she died *comatose* at 2 A. M. the following morning. I procured the history of this case from the medical attendant and nurse. Transfusion was tried in another case—a *stout man, but failed*.

CASE III.—Walter Brown, æt. 23, was seized on Monday night with purging, succeeded by a vomit of a fluid similar to soap washings, was restless during the night, and very cold. On Tuesday morning the symptoms were, lips blue, eyes sunk, expression anxious, surface very cold, not blue, cramps of legs, fingers, and thighs severe, pulse of radial artery feeble, purging and vomiting continue. Bled on Tuesday morning to $\bar{\text{xxx}}$, blood drawn nearly natural, spasms greatly relieved by the bleeding. Was ordered an ounce of brandy in hot water, and a pill, containing opii gr. j. , to be taken if the purging continued severe; and external heat was applied to feet and trunk. Wednesday.—Great irritability of stomach, thirst extreme; heat of surface rather higher than natural; has taken a small quantity of cold water, which he immediately vomited; pulse natural; took two grains of opium in the course of eight hours; no purging since Tuesday night. Ordered *horâ decubitus cal. grs. ij.* Thursday.—Irritability of stomach continues; thirst extreme; rinses his mouth with cold water, but does not swallow any fluid; one offensive stool; pulse natural. Friday.—Stomach less irritable; has taken a little tea, but no solid food; bowels confined. Saturday.—Countenance cheerful; heat of skin natural; pulse full, not accelerated; stomach less irritable, but has taken no solid food since the attack; bowels constipated. Ordered *cal. grs. v. horâ decubitus*. I think this man nearly convalescent.

CASE IV.—Eliz. Scott, about 40, was seized at six o'clock in the morning with coldness, agitation, and restlessness. At 11 A. M. diarrhœa commenced, and she fell down from apparent debility, but has not vomited. From this time she became gradually worse; and at 4 A. M. she was in a state of extreme collapse, her voice was inaudible, and her aspect cadaverous. Her eyes were everted and sunk, and the circumscribed blueness was very remarkable: her lips, fingers, and nails, were also very livid; and the pulse was

gone in most of the arteries. Treatment: brandy and opium, external heat, and friction. Died at nine in the evening.

CASE V.—George Scott, æt. 38, husband of the above. Symptoms: diarrhœa, to the extent of several loose evacuations on Sunday evening, which continued till Monday morning; at which time the spasms became severe in legs and feet, accompanied with great general coldness, and severe twisting pain at epigastrium and umbilicus; pulse of the radial artery weaker than natural; lips blue, but colour of hands and feet natural; countenance expressive of anxiety. Treatment: bled to $\bar{\text{xxx}}$, blood extracted darker than natural. The depletion was followed by great relief of spasms. The heat gradually returned, and the purging subsided. He vomited, for the first time, three hours after the bleeding. Has taken two pills, one on Monday, the other the day following, each containing *cal. grs. ij.*, *opii gr. ss.* Thirst during his illness extreme; and the urine was passed with difficulty, and in very small quantity. Was sitting up on the Wednesday following his attack, and well enough to extract by mastication the juice of a beef-steak, but he did not swallow the fibrous portion.

CASE VI.—George Tait, aged 40, weaver, of spare habit, had five loose stools prior to his illness, but no vomiting or other complaint. The following morning he was seized unawares with severe cramps in right leg and arm, accompanied with a spasmodic pain at umbilicus and epigastrium, which shoots to the heart and down the spine. Has ejected from the stomach a fluid like water, and passed several liquid light-coloured offensive stools. The eyes are surrounded by a livid zone, and depressed in their orbits; lips blue; countenance vacant and anxious; surface of the body bedewed with a cold perspiration; pulse of radial artery very indistinct and small. Treatment: bled to $\bar{\text{xxvj.}}$; blood drawn darker than natural, and followed by immediate relief of pain at heart and spasms. To have $\bar{\text{ij.}}$ brandy in hot water, and to be repeated at intervals every two hours till the heat is restored to the surface. The purging continued, for which he took *pulv. Opii gr. j.* and repeated it the following day, with $\bar{\text{ij.}}$ of *ol. ricini*. There was extreme thirst

for four days subsequent to the attack, and he took no solid food till the fifth day. Convalescent.

CASE VII.—George Scott, son of the above, four years old, was taken ill at five in the morning with severe vomiting and purging: his face and lips became as white as paper; his eyes were sunk in their sockets, everted, and half open; there were no spasms; the pulse was distinguishable; and he remained motionless till death, which took place at 10 A.M. five hours after the attack. Treatment: warm bath, mustard blister to the spine, and one pill was given continually, Cal. gr. ij. Opii. gr. ss.

CASE VIII.—An infant, 22 months old, was, at 11 o'clock at night, disturbed with a bowel complaint, which continued during the remainder of the night, and was succeeded by vomiting of a white watery fluid: the alvine evacuations resembled gruel. In the morning the surface was extremely cold, the eyes depressed and everted, the face livid, and no pulsation could be felt in the radial arteries; there were slight spasms, accompanied with muscular contraction about the umbilicus. The child, to use the mother's expression, squeaked at intervals. The little sufferer died in the evening, and was quite well the preceding day. The vomiting subsided before death, which was placid. Treatment: warm bath, external heat, and a pill of Cal. grs. ii., Opii, gr. ss.

As it is evident from the preceding descriptions the disease assumes a character very dissimilar in intensity, duration, and symptoms, so ought the treatment to be regulated accordingly; though, judging from a comparison of cases, and the general opinion of practical physicians who have treated cholera, I verily believe bleeding judiciously employed, is the very best single remedy in the second stage, with which we are acquainted, although I am willing to acknowledge that the vital powers have in some extreme cases received so severe and sudden an injury as to be insusceptible of the agency of any sensitive power which the genius of man, or science of medicine, has hitherto discovered.

At an autopsy which took place at Queensbury house, Feb. 9th, the subject being an old drunken woman, who died of cholera, there were no particular morbid appearances; the mucous mem-

brane of the stomach and bowels was rather softer than natural, but there was no vascularity at pyloric orifice of stomach or elsewhere, and in the brain there was neither softening, hardening, effusion, fibrinous deposit, or any marks of inflammatory action. The veins were rather engorged. The spine was not opened. I cannot conclude this letter without expressing my earnest hope that the inhabitants of the metropolis will imitate the conduct of the Edinburgh Board of Health, to whom, as a body, for their wise and active exertions, every individual of this city ought to feel extremely indebted. The Musselburgh Board have followed their example, and they have an excellent hospital, capable of holding from 20 to 30 invalids.

I remain, your obedient servant,

J. KNAPP.

P.S.—In the worst cases of cholera, where the disease comes on so suddenly as almost instantly to threaten annihilation, it has occurred to me that the Spiritus Ammoniacæ Succinatus might be the best stimulant, reasoning from its well-known efficacy in rousing the dormant vital powers after the injury sustained by them from the bite of the rattlesnake.

Two cases of cholera have taken place at Porto-Bello; one of the persons attacked is dead. The report at Edinburgh this day, Tuesday 14th, is, total cases, 12; deaths, 7.

Latest Musselburgh Report.—In the course of to-day or to-morrow no less than 40 or 50 patients will be struck off the sick list: out of 400 patients, in whom the cholera has proceeded to the state of collapse, more than half have recovered. From the commencement it is supposed that nearly 1000 cases have been under medical treatment; but great care has been taken that none have been admitted on the report unless labouring under vomiting and diarrhœa, with spasms, or reduced to a state of collapse.

A post-mortem examination by Dr. Llanste, took place at Musselburgh yesterday, Monday, 13th Feb. Nothing was discovered to throw any light on the nature of the disease.

Total cases, 416; deaths, 182; cures, 169.

On Sunday there were seven cases at

Croighall, near Musselburgh; two fatal.

Preston Pans—total cases, 98; total deaths, 18.

Tuesday evening, Feb. 14, 1832.

PRACTICAL HINTS ON CHOLERA.

THE following extracts are taken from a letter addressed by Dr. White (Physician to the Gateshead Cholera Hospital,) to the Rev. J. Collinson, Rector of Gateshead*. We have prefixed headings, to make them more pointed.

Diarrhœa followed by Constipation—Premonitory Symptoms.

In speaking of premonitory symptoms, I may here define what I mean by that term. The first and most general is diarrhœa; the evacuations at the commencement of a dark-brown or blackish hue, gradually becoming less and less feculent, until they assume the general appearance of dirty water. Slight cramps, most frequently of the toes, twitches in the abdomen, with giddiness and sickness, occasionally accompany it. Within six or seven days after the cessation of such purging, when it abates without the interference of art, I should consider the requisite remedies as imperiously demanded, having witnessed several cases of cholera occurring during that period. At a more subsequent period of the irruption, I would call particular attention to a state directly the reverse of this, where no diarrhœa, but *obstinate costiveness prevails*. Under these circumstances, purgatives, sometimes of a drastic nature, are resorted to. A favourite medicine of this kind is the pill cochineæ, a compound of scammony, jalap, calomel, and aloes. When the bowels are acted upon by these remedies, the dejections (during this time black and horribly offensive) retain their unhealthy qualities. *After a longer or a shorter duration of this torpidity of the intestines, the disease suddenly appears.*

Premonitory Symptoms give due warning.

The change from perfect to diseased action of the bowels is never so speedy but its stages may be noticed. By due attention to these indications, the necessity having been duly enforced, the whole body of the attendants at the Gateshead hospital escaped; though many of them experienced such symptoms as obviously threatened the disease.

Advantage of Ablution and Frictions.

To come, then, to the treatment of those

premonitory symptoms. I beg most particularly to enforce the benefit derivable from washing the whole body with warm water, and afterwards producing a genial glow upon the surface by friction with coarse cloths. I was induced to adopt this plan for two reasons:—First, from the known sympathy between the stomach and bowels and the skin; and, secondly, because I am of opinion that a strong infecting medium exists in the filth allowed to accumulate upon the persons of the poor, their clothes absorbing the noxious exhalations that float around in a contaminated atmosphere, thus presenting to the cutaneous pores, if I may so express myself, a perfect plaster of infection.

Light supposed to have a Disinfecting Property.

I have seen the disease in the cleanliest houses; in the best ventilated situations; in places from which day's richest light is never intercepted. I have spoken here of light, because I am inclined to think that it possesses a strong disinfecting power. I cannot now enter into an explanation of the reasons that have influenced this conclusion; but shall merely state, that I have observed the deadliest type in the darkest dwellings.

Efficacy of Enemata of Warm Water.

Injections of three or four pints of warm water, with or without laudanum, according to the indications, should be as frequently thrown into the rectum with a forcing syringe, and restrained there, if necessary, by mechanical means.

I cannot speak too highly of the large injections of warm water. They should be frequently repeated. I have known the general uneasiness, pain, and occasionally the sickness, vanish upon its use, ere the instrument has been withdrawn.

Cold Water not Injurious.

I have witnessed great reluctance to the administration of cold water, when the thirst has been excruciating. I have always allowed it, in general slightly coloured with brandy, both in this stage and the succeeding one of collapse, and never saw ill effects result from the indulgence. I sincerely trust this prejudice will yield; for I have never seen gratification more intense than when this desire has been complied with. Nature's instinctive voice demands it, and science every day discovers how seldom she demands in vain.

Caution as to Diagnosis.

Prior to death, when it takes place during this stage, the disease sometimes assumes a form well calculated to deceive those not acquainted with its character. The pulse rises, the skin becomes warm and covered with perspiration, the patient expresses his relief, and, perhaps, his full anticipation of recovery: in an hour or two, all is over.

* Published by Mackenzie, Newcastle; sold by Highley, London.

Blueness not a General Characteristic.

The blueness, so peculiar to this disease in other countries, has not been, by any means, a general characteristic in this. In not one instance in ten, has it assumed that form. The skin of the hands and face often become of a brownish hue.

Choleric Voice.

In the most deadly form of cholera there is a tone of voice, a wail, which once heard, can never be mistaken; to him, upon whose ear it has fallen in the accents of anguish, it can never be forgotten. I have always found it the certain prognostic of death.

Appearance of Tongue in Consecutive Fever.

The tongue throughout is most frequently moist, and sometimes coated. In general, however, as was observed by Mr. Glenton, it presents a red appearance, similar to what is seen in eruptive fevers. Sometimes the redness is confined to the tip and edges. This is the time at which, in my opinion, the lancet may be most advantageously used.

Dr. Gibson's Report on the Treatment of Cholera in the North of England.

DR. GIBSON, who has lately returned from Newcastle-upon Tyne, has laid a report before the Central Board of Health, on the results of the various therapeutic measures which he had recourse to in the treatment of cholera in the districts lately under his superintendence.

Dr. Gibson divides the disease into three stages—namely, diarrhoea, collapse, and consecutive or re-active fever; and he details the mode of treatment applicable to each of these individual periods. In the first, he informs the Board, that, in young and comparatively robust persons, an early bleeding, in quantity proportional to the age and constitutional strength of the patient, was attended with good effects, especially in the case in which pain was experienced on pressure of the abdomen. Dr. Gibson further states, that calomel, in doses of two grains, with a sixth of a grain of opium, when prescribed every two hours until the sixth time, and followed up by a small dose of castor oil, or Gregory's mixture, was found of eminent service in allaying the urgent tendency to purging, and in restoring the secretions of the intestinal canal. When the purging was obstinate, Dr. G. reports that chalk mixture and laudanum were prescribed with the best effects. At the same time that these measures were practised, attention was paid to the state of diet,

clothing, &c. of the patients, and they were directed to abstain altogether from spirituous or fermented liquors. This mode of treatment, Dr. Gibson continues, was generally successful in a few days, and he expresses his conviction that it frequently prevented the malady from running on into the period of collapse.

This stage of the disease Dr. Gibson would define, or rather describe, as being composed of a group of three or any of the subsequent symptoms, "vomiting, purging, and prostration of strength, occurring simultaneously, or in rapid succession, with sinking of the vital powers." Such were the phenomena, varying of course in intensity, exhibited by this stage in all the cases which came under hospital treatment. The practice adopted in Sunderland under these circumstances, consisted in venesection, if the pulse could be felt; so suddenly, however, was the action of the heart and arteries overpowered in many cases, that detraction of blood could not be satisfactorily accomplished. Where the pulse is strong and the spasms severe, Dr. Gibson thinks that venesection should be practised as a general rule, but he acknowledges that he has seen the disease run a rapid and fatal course in several cases suitable to this proceeding, and where it had been fully and fairly tried.

Dr. Gibson also dwells at considerable length on the employment of mustard emetics in the collapse stage; he states, that he found this remedy to produce full vomiting very effectually when administered in the dose of two drachms, or a dessert spoonful in six or eight ounces of water, and he adds that it has been given in many instances with decided benefit; in short, together with a timely bleeding, it constitutes a very important part of the treatment.

In this stage of the disease, the patient, according to Dr. Gibson, complains most of the spasms or irregular actions of the muscles of the extremities. Provided the stomach retains its vitality, he considers the mustard emetic of much value at this time; and he further strongly recommends the injection of one or two pounds of warm water, containing one drachm of laudanum, thrown in with such force as to fill the whole colon and rectum. This acts, he conceives, as a warm internal fomentation, and has a very beneficial effect. Dr. Gibson dwells particularly on the necessity of supporting or increasing the heat of the body by artificial means, and he recommends a novel apparatus for this purpose—viz. a series of tin boxes made to fit the limbs and surface of the body. One of these vessels is contrived to cover the whole of the abdomen; two wedge-shaped boxes fit the back and meet at the spine.

Dr. Gibson has seen no benefit to the patients from friction, and he laments to add,

that many of the persons engaged in its performance fell victims to the disease. Lastly, Dr. G. recommends that any gastric irritation remaining from the cold stage should be met by a dose of fifty or sixty drops of laudanum, with a little brandy, or other spirit.

On the treatment of the fever stage, Dr. Gibson does not offer very lengthened remarks. He advises it to be conducted on general professional principles. He states, however, that bladders of cold water applied to the head were especially useful, and that calomel, combined with James's powder, was also of benefit. Dr. Gibson met with no case of recovery from collapse without the super-vention of fever.

MEDICAL GAZETTE.

Saturday, February 18, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Ar-tis Medicæ* tueri; potestas modo veniendi in pub-licum sit, dicendi periculum non recuso."—CICERO.

CHOLERA IN LONDON—GOVERN- MENT BOARDS OF HEALTH.

THE formal announcement of Cholera in London has destroyed the dreams of those who were impressed with the flattering belief that the disease was exclu-sively bent on the discovery of the "north-west passage." The event also establishes the truth of our prediction, that notwithstanding all the experience of the past, the malady would find the metropolis unprepared for its visitation. Panic is the result, and hurry and confusion are the *order* of the day. This, in fact, has generally been the case under similar circumstances: people are alarmed at the first decided step which the disease makes towards them—then forget their fears if the malady does not instantly appear—and their ter-rors are only awakened again by the declaration of the enemy being already among them. The extent to which ex-aggerated apprehension, and its atten-dant evils, may be prevented, and the rapidity with which composure, and all its benefits, can be restored, are in pro-

portion to the confidence of the public in the authorities appointed to watch over their safety, and the judgment, good faith, and discretion, with which those functionaries discharge their mo-mentous trust. To say that in London our Government Boards of Health have carried with them that confiding defe-rence so essential to the fulfilment of their measures, would be to make an assertion notoriously at variance with the facts. The first Board sounded a precipitate alarm, calculated to fright the isle from its propriety, holding out the prospect of sanitary cordons and relentless quarantine, exe-cuted with all the rigour of military dis-cipline, and literally enforced at the point of the bayonet. The result proved that such measures, even if other-wise advisable, were incompatible with the state of society in this country, and experience has fully proved with regard to others—not, indeed, that the disease might not in particular instances be ar-rested, or rather, perhaps, excluded—but that the prohibition of all intercourse has invariably failed, on the great scale, when applied to nations. But if, even in those countries where the govern-ment is armed with despotic power, and where the presence of the pestilence gave ocular proof of the danger, every attempt at coercive exclusion ultimately failed, it cannot be matter of astonish-ment that the contemplation of a similar system at home, though but as a possi-ble contingency, should have roused the jealousy of his Majesty's lieges, who at all times would prefer perilous freedom to safety attended with constraint.

Whether, from a conviction on the part of ministers that the urgency of the case did not warrant the rigour of the measures proposed to them, or from an apprehension that the very attempt in a mercantile country would prove an evil more to be dreaded than the one it was intended to avert, we know not; but a

new Board was appointed, whose determination it obviously was, from the very outset, to avoid the rock which had proved fatal to their predecessors. Professing the same general opinions as to the infectious nature of the disease, they yet shortened the duration of the quarantine which had been proposed, and at once declared themselves against all coercive attempts at isolating the disease. It is remarkable, however, that notwithstanding these conciliatory measures, the Central Board were unable to obtain the confidence of the public, and the concessions just mentioned were regarded by many as not quite consistent with the tenor of the reports which the medical commissioners had transmitted from abroad. However this may be, they do not seem to have possessed either the moral influence, or the legal authority, necessary to give weight to their recommendations; hence their suggestions were frequently disregarded, and their strongest injunctions evaded. The result of all this has been, that the announcement of the disease among us, found the metropolis lamentably unprovided for such an event; an announcement, indeed, which took the public the more completely by surprise, inasmuch as it had been officially stated, less than forty-eight hours previously, that the precautions adopted had been effectual in preventing the introduction of the disease.

The proclamation of a pestilence, unaccompanied by the assurance of preparations adequate to the emergency having been made, must always be attended with serious apprehension; and that such arrangements had not been effected, and that sufficient attention had not been directed to the subject, is demonstrated by the extraordinary precipitation with which a Bill* has been passed, though the calamity which is now said so imperatively to require it has already

been of six months duration: we allude, of course, to the declared existence of the disease in the North of England. The evil which may result from this absence of preconceived measures must be very much dependent upon the rapidity, or otherwise, with which the cholera may spread: if it does so rapidly, the consequences must be disastrous, inasmuch as all the measures emanating from authority proceed upon the supposition that the malady is contagious, (the opinion in which we participate) and has thus naturally led to resolutions at most of the general hospitals, prohibiting the admission of its victims within their walls, ere other receptacles had yet been provided for their reception. We confess, however, that though the announcement of cholera in London would have been less alarming had it been accompanied by a notification that every necessary preparation had already been made, still we do not anticipate any such ungovernable speed in its extension as to outstrip the arrangements now at length in progress. The orators of our vestries have discovered that something more is required than to make speeches, and have set in earnest about the necessary provisions. In most of the parishes, houses are being rapidly fitted up as hospitals, and medical men appointed to the charge of the different districts. In a few instances these arrangements are already completed.

But among the preventive means, there is one which in London at least has not received the attention it unquestionably merits—we mean the expediency of removing the predisposition to the malady which the privations of the poor, especially at this season, never fail to engender. No one can pass through a single street without having the more than usual prevalence of want forced upon his attention; and it is to

* See an abstract of this Bill, p. 773.

be remembered, that the houseless and starving wretch, whose misery might fail to insure relief from any kindlier motive, ought yet to be succoured on mere principles of prudence. This, however, can only be efficaciously accomplished by the parish authorities, whose consideration of the subject we earnestly solicit; for, it is well ascertained, that the disease has in numberless instances been propagated by mendicants, whose poverty compels them to go about in quest of subsistence, though already in the first stage of the disease, and who thus carry with them an itinerant atmosphere of infection.

In respect to the relief afforded to the destitute, as, indeed, in all the arrangements they have made, we must again hold up to imitation the Magistrates and Board of Health in Edinburgh. Without either poor-rates or a "Bill," they have fed the hungry and clothed the naked; they have isolated the sick, and placed suspected persons in quarantine: theoretically their plans have been judicious, and practically they have been successful,—still keeping the enemy at bay. So much, indeed, has this been the case, that *we know* it has been attempted, by certain persons in authority here, to explain the small number of cases in Edinburgh by supposing that concealment had been practised. This suspicion we regarded as utterly unfounded; first, from a consideration of the high characters of the gentlemen whose names appear on the list of the Board; secondly, from regarding it as impossible for them to conceal cases to any extent, even if willing; and thirdly, because we see, in the fitness of the means adopted, a rational explanation of the end accomplished. But the truth is, that the Board in Edinburgh is very differently situated from the one in London: it embraces the names of many

distinguished men cordially united in one common cause; their proceedings are the result of mutual agreement after due discussion—not of dictation, for they hold themselves absolutely independent of their London brethren. If the regulations and plans of the Central Board were those of the most perfect wisdom, they would not be acted upon in the same unhesitating and zealous manner; they would be canvassed and cavilled at by the local Boards, who are and will be jealous of authority in which they do not share, and of the surveillance of "inspectors," the very nature of whose instructions, as described in the official announcement of their appointment, implies a distrust of the civil practitioners, with whom they are intended to co-operate. The medical members of the Central Board are neither sufficient in number, nor so eminent in reputation, as to command the deference of the profession in the metropolis. No men could be more fit to constitute a part of the directing body; but we doubt whether it be reasonable to expect that the profession generally will co-operate so cordially as could be desired, on an emergency like the present, with a Supreme Board, at which no practitioner in London has a seat. The general existence of such feeling is too notorious to be concealed, and is strikingly illustrated by the fact, that the only remedy which the Central Board recommended with any degree of confidence in the cure of cholera has never in any instance been adopted;—we mean the application of the actual cautery to the spine. Indeed, so decided has been the rejection by British practitioners of the official injunction on this subject, that the Board have quietly suffered it to fall into abeyance!

We are neither among the alarmists who would encourage unreasonable apprehension, nor among those who, regardless of plain facts, look only to the

effect which the publication of these is calculated to produce upon the mercantile interests. We throw aside the views and the motives of both alike, and we declare our belief, that there exists in the metropolis a disease of more than usual malignity, apparently a fever—that which has received the name of Cholera; but it is also our conviction, that its appearance is not altogether recent, inasmuch as we hold the case which occurred some weeks ago in the first regiment of Guards, as well as others which have been designated only as “suspicious,” to have been in reality the same disease, by what name soever it may be called. The official announcement, however, has spread great and general alarm; has given a shock of the most formidable description to commerce, and has led to the arming of the Privy Council with extraordinary powers. It is essential to the vital interests of the community that these powers be exercised alike with caution and with vigour—the rarest combination of qualities in the human mind; and, without intending any disrespect to the medical members of the Central Board, we must be permitted to say, that to throw on them the exclusive responsibility of advising the Council, and to hold them up to the public as the authority on which the common law of the land is to be superseded, is to place them in a false position,—one which they cannot maintain with grace as respects themselves, nor perhaps with safety to the weighty responsibility which must press upon them. To us, then, it appears that the time is come when a Board, more on the plan of that in Edinburgh, ought immediately to be established, comprising, among others, such a body of medical men from different departments as shall at once command the confidence of all reasonable persons; placing the fidelity of their announcements above all ques-

tion, their motives above suspicion, and giving all the security that human means can afford against precipitate or erroneous judgment, in matters involving the comfort, the interest, and the safety of the nation.

REPORT OF CHOLERA IN LONDON,
UP TO THE AFTERNOON OF FRIDAY,
FEBRUARY 17TH.

ONLY two new cases have occurred since the report of yesterday, and these were in Southwark; there have been two deaths also in Southwark; so that the total for all the places in which the disease has hitherto appeared—Limehouse, Lambeth, Southwark, Rotherhithe, and afloat on the river—stands thus:—Remaining since yesterday, 13; new cases, 2; deaths, 2; recovered, 0; remaining, 13. Total cases in the metropolis since the commencement, 30; deaths, 14. The details of several of the preceding cases have been published by the Board of Health; and as they are probably in the hands of most of our readers, having been inserted in various papers, we have preferred presenting them with some more recent examples of the disease in Southwark, and at Musselburgh, furnished by correspondents of our own. These will be found at pages 762 and 765.

THE CHOLERA PREVENTION BILL.

By this measure, which confers the most ample discretionary power on the Privy Council—1. A quorum of the members of it are empowered to issue such orders as may seem most expedient for preventing the spread of the spasmodic cholera, for providing for the relief of those suffering under or exposed to it, and for the safe and speedy interment of those who shall have died of the disease. 2. Those orders are to be published in the *London Gazette*. 3. Disobedience to those orders is to be deemed a misdemeanor, punishable

by fine. 4. This fine is recoverable when sued for before any two justices of the peace. 5. These justices are empowered to enforce the attendance of witnesses, under pain of imprisonment. 6. In default of payment of the fine, the goods of the party amerced may be distrained, or, if he have no goods, he shall be sent to prison. 7. The penalties shall be applied to the relief of the poor in those places where the offence is committed. 8. The expenses incurred by carrying into effect the orders of the Privy Council, shall be procured, on the warrant of two justices of the peace, from the churchwardens, overseers, or guardians of the poor, out of the poor-rates, and the amount charged to the county; the treasurer of which, at the following sessions, shall reimburse the parish authorities. 9. This clause provides for the rapid and free transmission of the Council's orders by post. 10. The orders, as soon as possible after they have been issued, shall be laid before both Houses of Parliament. 11. The act to be in force only for a limited period.

HUNTERIAN ORATION.

ON Tuesday one of the largest and most respectable audiences we have seen for some time within the walls of the College of Surgeons, assembled to hear the anniversary panegyric on the "great light" of British surgery. The theatre was thronged, and, we are told, many had to go away who could not find even standing room. On the front benches, along with the President and Council, we noticed several very distinguished visitors. And what a gratifying contrast was presented by the whole aspect of things to the state of the same theatre just a twelvemonth before, when a few nameless mal-contents, incited by a notorious disturber, contrived to throw every thing into a state of riotous confusion! But the doings of these desperadoes have only recoiled

upon themselves—while they unintentionally obtained conditions of peace, with ample power of chastisement for their opponents—thus falling into the pit themselves which they had digged for others. How happily the place was rid of them on the present occasion, was testified by the respectful, and marked attention, with which the "order of the day"—the oration—was listened to, and by the hearty round of applause which followed its close. With regard to the manner in which the orator—Mr. S. Cooper—acquitted himself, we are inclined to speak very favourably. Without aiming at flights particularly lofty*, or attempting to surprise his auditors with abstruse paradoxes, by way of scientific illustration, he confined himself as much as possible to the matter in hand, and shewed it to be his chief ambition to be appropriate. Some few of his details might undoubtedly have been spared, nor should we have liked his performance the less had it been more liberally laudatory of departed merit. The tribute to Abernethy, told. There was not, we are sure, an individual present whose feelings did not warmly go with the orator in his brief but graphic eulogium of that eminent surgeon: *we* could have wished Mr. Cooper, who, we believe, from some expressions that fell from him, was brought up at the feet of that Gamaliel, had dwelt somewhat longer on a theme so acceptable. The praises of Paré and Vesalius might very well have given place to the eulogy of the illustrious dead nearer home.

ANATOMY BILL.

To the Editor of the London Medical Gazette.

SIR,

THE misprint of a word in the last paragraph of my communication of the

* What little in this strain Mr. Cooper ventured upon rather shewed that Pindarics were not exactly his forte. "John Hunter, Mr. President, and Gentlemen, is removed from us; but until the infernal waters of Lethe have become the common beverage of the human race, his memory will remain!"

8th instant, rather perverted the meaning of the passage: the word "*students*" should be substituted for "*schools*." It must be obvious that my assertion would hardly hold good, if stated with reference to the existing schools in London.

The perusal of Mr. Travers's letter, published in your last number, afforded me very great pleasure; it is, therefore, not without some regret that I am forced to differ with him, mainly as regards the "licensing of schools," and "the prohibition of the practice of anatomy elsewhere." As to the former, I have already stated that it is fully effected by the recognition of teachers. It is, moreover, an objectionable measure, on account of the power which would be thus vested in the hands of any individual, or individuals, on whom the duty of granting licenses would devolve—a power which would be extremely liable to abuse. The prohibition of the practice of anatomy elsewhere than in licensed schools, must tend materially to injure operative surgery, and to restrict its practice to those who may be in the vicinity of, or connected with, a school. Every operating surgeon must acknowledge the increased confidence which he possesses during an operation, if, immediately previously, he have had an opportunity of operating on the dead subject, and have dissected and examined the relative anatomy of the parts. There can be no doubt that most of our capital operations would be, not only better and more holdly executed, but also more successful in their results, had the surgeons greater opportunities for private dissection. So convinced am I of this, that, were I about to be the subject of an operation myself, I would make it a *sine quâ non* with my attendant, that he should operate upon the dead subject immediately before. The very sensible letter of your correspondent, W. R., is quite in confirmation of these views; and I am happy to have it in my power to obtain further support for them from the words of a justly eminent surgeon and operator. They will be found in a note appended to Mr. Crampton's account of a case of aneurism of the external iliac artery, for which he tied the common iliac*. Having stated, that, immediately previous

to the operation on the living, he performed it seven times on the dead subject, Mr. C. proceeds:—"Two of the operations were performed on the morning of the 18th (the day on which operation was performed); and on each trial, *even to the last*, I found that I performed the operation in a manner much more satisfactory to myself than any of the preceding ones. I mention this, not only as a matter worthy of the consideration of those who would limit the opportunity of the student of surgery to the dissection of two, or at the utmost of four subjects, but as affording a proof (if, indeed, proof be wanting) that operative surgery cannot be usefully cultivated without almost *unlimited* means of practising dissection, and that, as it has been well expressed, 'we must choose between mangling the living or the dead.'"

Facilities for anatomical study would have the effect of supplying the country with a greater number of operating surgeons—a class of practitioners now almost wholly confined to great towns—and thus many patients would be saved the expenses of a long journey and the probable risk consequent on such exertion.

It would be highly desirable that some plan should be adopted by which the schools might obtain a supply of subjects, *proportional to their classes*, from one source; to which practitioners might also have recourse, in case they should require to maintain or refresh their anatomical knowledge. Such a plan would obviously define, as well as simplify, the duties of inspectors, in conformity to Mr. Travers's wishes; it would prevent that competition between schools as to relative supplies of subjects, which must, in fact, keep up the traffic in dead bodies, even though it were removed from the hands of the resurrectionists.

The above observations I have thought it right to make, with the view of explaining more fully the grounds on which I differ with Mr. Travers. I would again express my regret at being compelled to differ from so able an advocate for a question of so much importance to the public and to science.

I remain, sir,
Your obedient servant,
R. B. TODD.

37, Great Russell-Street, Bloomsbury,
February 15, 1832.

* Vide Med.-Chir. Transactions, vol. xvi.

ROYAL INSTITUTION.

Friday, Feb. 3, 1832.

Capt. J. J. CHAPMAN, R.A. VICE-PRESIDENT,
IN THE CHAIR.*Mr. Griffiths 'On Chemical Signs.'*

BEING an old pupil, and formerly an assistant in the Laboratory of the Royal Institution, Mr. Griffiths introduced the Essay which he read this evening, by some very appropriate acknowledgments for the benefit and the encouragement he had received within its walls; and he expressed his feelings in terms which did credit to himself, and must have been highly gratifying to his patrons and the members present.

Offered as the first fruits of his researches, even had it been far less interesting, we should have looked upon this lecture with indulgence; but it required no partial eye to warrant its approval, and we only regret, that, from our inability to transfer his splendid diagrams to our pages, without which the explications would not be intelligible, we are precluded from giving an abstract of his ingenious speculations, by which he satisfied his audience, that many of the alchemical signs so often looked upon as symbols rather of whimsicality than of wisdom, have, when properly considered, a very expressive signification; they only require the emblematic etymologist to unravel their corruptions, and to trace them to their roots.

At the close of the lecture Mr. Faraday demonstrated the principles of a machine lately constructed by Mr. Bate, by which engravings may be taken from medals with a tenth of the labour, and less than a tenth part of the expense, of the usual process; the operation may be performed by any one, not requiring an engraver to do it, and the effect produced by the impressions is quite beautiful. It would be vain, without a diagram, to attempt to describe the machinery minutely; let it suffice to say, that the principle consists in having a beam, something like that which, in the German profile machines, passes by one end over the face, and by the other inscribes an outline on the tablet; here one end passes in a line over the medal, while the other, having a graver attached to it, makes the corresponding line on the copper-plate, and, as the inequalities of the medal will make the lines graved on the plate differ in their distances from each other, so are the effects of light and shade produced by their greater or less proximity.

We noticed on the library table, among several other things worthy of attention, a most splendid specimen of muriate of morphia in crystals, upwards of a pound! This was prepared by that very ingenious practical chemist, Mr. Morson.

DELIRIUM TREMENS.

Of 43 patients labouring under this complaint, M. Pauli has lost but 1, and in 12 cases has been enabled to stifle it in its beginning. His grand remedy is fresh *ox gall*, in a dose of from 3 to 6 drachms (gros) in from 4 to 6 ounces of peppermint water, infusion of valerian or calamus aromaticus. Same time, the patient takes half a glass of brandy in the morning, and at night one or two grains of watery extract of opium.—*Rust's Magazin.*

ELECTRIC EMBRYOS.

For their important researches regarding the formation of embryos by the action of electric currents, Professor Delpech and Dr. Coste have been recently presented with the large gold medal by the Société des Sciences Physicales.

HUFELAND'S EMETIC FOR INFANTS.

R Pulv. Ipecac. ℥j.
Oxym. Scillæ ℥ss.
Syrupi Framb. ℥ss.
Aqua comm. ℥ss. M.

A spoonful is to be given in coffee, at short intervals, until vomiting is induced, and it may be repeated, if the action is to be kept up. For children more than a year old, a quarter of a grain of tartar emetic may be added to the above mixture, unless diarrhœa be present.—*Gazette des Hôpitaux.*

METEOROLOGICAL JOURNAL,

Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.

Feb. 1832.	THERMOMETER.	BAROMETER.
9	from 29 to 50	from 30.25 to 30.33
10	24 43	30.43 30.46
11	29 43	30.40 30.28
12	28 43	30.26 30.10
13	30 41	30.02 30.00
14	32 39	30.02 29.96
15	20 39	30.00 29.94

Wind variable, N.E. prevailing.

Except the 10th and 15th, generally cloudy. Rain on the 9th and evening of the 12th. On the evening of the 11th, a few small flakes of snow fell.

Rain fallen, '1 of an inch.

CHARLES HENRY ADAMS.

W. WILSON, Printer, 57, Skinner-Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF
Medicine and the Collateral Sciences.

SATURDAY, FEBRUARY 25, 1832.

LECTURES
ON
THE THEORY AND PRACTICE OF
MEDICINE;

Delivered at the London University,

By DR. ELLIOTSON.

PART I.—LECTURE XXI.
UNIVERSAL DISEASES—*continued.*
Scurvy.

I PROCEED NOW, gentlemen, to the consideration of scurvy.

Etymology.—The word scurvy is said to be derived from some German words, *scharf-pocke*, meaning sharp or violent pock, which were corrupted to *scharbock*; or from *shorf-pocke*, meaning scab or scurf-pock. However this may be, it is from the word *scharbock*, latinized and corrupted, that *scorbutus* is derived, and a very barbarous word it is. From this we are said to have our English term *scurvy*; but I should rather think it came directly from the Danish word *scurv*; and this name *scurvy* is used by the vulgar in a very indefinite sense, being applied by them to any ill-looking chronic cutaneous disease, but in our profession it is restricted to a particular affection.

Symptoms.—This disease is characterized by a bloated surface, and petechiæ, vibices, and ecchymoses. By petechiæ are meant minute dark red or livid points, little larger than the point of a pin; spots still larger than these are called vibices; and when instead of spots we have patches, the word *ecchymosis* is employed. They all relate to the same appearance, but denote a difference in extent. With respect to colour, these points, specks, or patches, are of a dark red or purple hue, but may contain all the shades which we see in bruises. The surface, therefore, in this disease is bloated, and upon it are seen points, specks, and patches, of a red or purple colour, and of all the shades which we see in common bruises. A very remarkable circumstance, however, also attends the dis-

ease, and that is, the hardness of many parts, but particularly of the thighs. If you examine the thigh of an individual labouring under scurvy, though it be only in the slightest degree, I believe you will find it generally, but more especially under the hams, hard, and in severe cases I have seen it as hard as a board. I have not seen many cases of the disease, but in all of them I have noticed this circumstance. The gums are particularly affected; they are spongy and bleed, and either they or the breath, or both, send forth a very offensive smell. Such is the disease of the gums that the teeth very frequently fall out, and in addition to their being spongy and bleeding, they become enlarged and livid.

This is a disease of great debility, and the spirits are always very much depressed. So great is the debility, that people very frequently faint from time to time, and the pulse is found to be weak, and the surface of the body cold. Very often, ulcers form upon the surface, and discharge a thin and fetid bloody fluid, and at last a coagulum of blood is formed. The gums are in precisely the same predicament. The blood which is discharged and coagulates upon the ulcer is with great difficulty separated from it; it adheres to the ulcer and the flesh which is beneath, and, when you remove such a coagulum, the flesh is found to be, like the gums, soft and spongy. If you remove the coagulum, it is instantly renewed; a fresh oozing of blood takes place, a second coagulum supplies the place of the first, and at length a fungus will sprout forth—a soft, flaccid, dark looking fungus, which sprouts as fast as you take it away, and which is called by sailors *bullock's liver*; it may attain an enormous size. If this fungus be repressed, a gangrenous tendency is frequently observed; the leg will swell, and become more spotted and painful. You of course know that when a fungus sprouts forth from the dura mater after a fracture of the skull, it is very dangerous to repress it; if the part be compressed, very frequently dangerous symptoms ensue. So

it is found injurious in scurvy to repress this *bullock's liver*, because the pressure induces a gangrenous tendency. The very slightest bruise inflicted upon a patient labouring under scurvy to any degree, will generally produce an ulcer of this description.

There are, however, some very remarkable circumstances respecting this disease. Old wounds, and even fractures, have a tendency to recur under it. Wherever an ulcer has existed, wherever a solution of continuity in soft parts has taken place previously, although the parts have been well cicatrized, yet under this disease the wound often opens again. Nor is this occurrence confined to soft parts, but even bones themselves, as I just now stated, which were formerly fractured and repaired, become again disunited, shewing that the callus of bones is not so strong as the original parts of the body, and that it suffers when the rest of the bones do not.

There is also another very singular circumstance connected with this disease, and that is the occurrence of nyctalopia, or night blindness. Patients labouring under scurvy frequently become blind, either altogether or in part, when night comes on.

Causes.—Now the great causes of this disease appear to be the want of fresh animal and fresh vegetable food. It is on this account that formerly the disease was very common at sea, for at one period sailors were supplied with nothing but salt provision. So badly were ships formerly provided for, and so bad was the general management, that in the year 1726, when Admiral Hosier sailed to the West Indies with seven ships, he buried his ship's company twice, and then died himself of a broken heart. Deaths to the amount of eight or ten a-day took place formerly in a moderate ship's company. The bodies were sown up in hammocks, and washed about the deck for want of sufficient strength on the part of survivors to throw them overboard. I may mention that Lord Anson, in the year 1741, lost one half of his crew by scurvy in six months: 961 men sailed with him, only 335 of whom were alive at the end of the year; and at the end of the second year, 71 only were fit for the least duty—not to say duty, but for the *least* duty. Sir Gilbert Blane says that the disease appears in about six or seven weeks from the beginning of sea-voyallings.

You cannot have a better description of the dreadful mismanagement formerly, in regard to the navy, than you will find in Roderick Random. Smollett, both in that and in his History of England, gives an account of the armament which, about the same time that Lord Anson's expedition took place, was sent out against Carthagea. The description is from his own observation. He says the provision consisted of *putrid salt beef*, to which the sailors gave the name of

Irish horse, (I suppose the contractors lived in Ireland, and it looked like horse-flesh), *salt pork* and *musty bread*. The salt pork came from New England, and was neither fish nor flesh, but savoured of both. The bread came from the same country, and the biscuit, like a piece of clock-work, moved by its own internal impulse, occasioned by myriads of insects that dwelt within it. The butter was served out by the gill, and was exactly like train-oil thickened with salt; and though there was water enough to allow each man half-a-gallon daily for six months, yet each had only a purser's quart a-day in the torrid zone, where a gallon would have been hardly enough to repair the waste of perspiration. You cannot wonder, therefore, that scurvy formerly prevailed to this dreadful amount.

Former prevalence in London.—However, the disease prevailed likewise on shore, and scurvy at one period was one of the most fatal diseases in London, so that even in the 17th century—as late as that—there were from 50 to 90 deaths from it annually, and in the year of the plague there were not fewer than 105 deaths. These frightful occurrences took place regularly, and not during a particular year. The same reason, however, existed for the prevalence of scurvy in London which produced it at sea; for the food of the Londoners was then salt beef and pork, with a little real. The lower orders had very little else in the time of Henry VIII. The fact was, that pasture land only was then common, and very little was cultivated. Animals could feed, of course, only during the summer and autumn; hay being a later improvement, it was impossible to feed them longer than that period, and they were therefore killed as the winter came on, and salted, and thus a store of provision was laid up until the next spring. Garden stuff, too, was extremely scarce in those days, so that Catherine of Arragon, one of the numerous wives of Henry VIII., was actually obliged, in the beginning of the sixteenth century, to send to the Netherlands for a gardener to raise her a salad, so ignorant were the gardeners of this country of what is now considered within the reach of every body. Cabbages and other garden stuff were not cultivated in England before the reign of Henry VIII. Government too, at that period, seemed to encourage the consumption of this meat; for the price of meat was fixed by law at one-twentieth of what it now is, whereas wheat was fixed at only one-tenth of its present price. Care was thus taken to have a good supply of animal food, but vegetable food was comparatively neglected. I may mention, that in 1700 a cabbage cost threepence, which in 1760 cost only one halfpenny; such was the advance of art and the increase of knowledge, that so great a difference occurred in the price of a cabbage at those two periods. Other

greens too, at first, were proportionately dear; and garden stuff was only used at that time on Sundays, and as a great dainty, when people had company.

The use of salt or putrid meat appeared to be the cause of scurvy. But it was not the salt, for salt though taken in the greatest excess will not occasion scurvy, and scurvy will take place where no salt is used—nay, persons will have scurvy who eat no meat at all: and therefore it is not this, but the want of other food—the want of fresh animal and fresh vegetable food—which induces the disease. I have seen several, not a large number, but several cases of scurvy, in individuals who had eaten no meat at all; they had been deprived of meat of every description, and it arose in them from the want of food. You will find in the *Transactions of the College of Physicians*, vol. ii. two cases mentioned by Sir Francis Milman, of women that had the scurvy in the country, but who had eaten no meat whatever, having lived on tea and bread after being accustomed to better food.

Sea and land scurvy, I believe, are exactly the same; and I may state, that Dr. Musgrave, who published a work on the Gout in 1703, mentions that this disease was common in Somersetshire; so that you observe it prevailed at sea, in large towns, and in the country.

There can be no doubt that many circumstances conspire to the occurrence of this disease. Cold and the want of exercise unquestionably encourage it; for sailors are observed to suffer in cold latitudes, when they are placed under precisely the same circumstances, with the exception of latitude, in which they escape it in warm climates: this fact strikingly illustrates the effect of cold. Then, as to the want of exercise, Captain Cook says that the people in Kamtschatska, who are habituated to hard labour, never have the scurvy, while the Russian and Cossack in garrison, who live in the greatest indolence, are subject to it. Sir Gilbert Blane says that the prime seamen only were attacked with scurvy, who were exempted from pumping. He instances the case of a particular ship's crew, and says, that once the prime seamen suffered the disease, whereas those who were obliged to work hard at the pump from time to time, the ship having proved leaky, escaped. Moisture also is said to have a considerable effect—I presume, especially when united with cold. La Perouse attributes the prevention of scurvy in his crew very much to the vessel being kept dry by fumigation, and braziers of hot coals. Captain Parry ascribes the first case of scurvy, in one of his expeditions, to moisture. It was observed, when scurvy prevailed in London a few years ago, at the Penitentiary at Milbank, that those persons employed in the kitchen always escaped; perhaps, however, they got

better food than the rest, or more of it, but at any rate they had a warmer place. Captain King told Dr. Macmichael, as he stated in a paper read last year at the College of Physicians, that, in a voyage round the south coast of America, no cause of scurvy was apparent, the crew having had plenty of lemon juice, except that there was a remarkably cold and moist state of the atmosphere. I do not believe that moisture will occasion it alone, but moisture certainly aggravates the effects of cold in this disease, as it does in all others.

The difference between ships' crews now and formerly is very striking. While the crew of Lord Anson suffered so much in a voyage round the world, that of Captain Cook, in a voyage subsequently performed, suffered nothing. The difference arose from this circumstance: Captain Cook had a good supply of portable soup, sour crout, and fresh meat, and he kept his men in regular exercise, at the same time taking care that extreme cleanliness and ventilation should be observed. In addition to this they were only out about three weeks on their longest cruise, although absent so long.

Such measures as these will generally prevent scurvy, if there be no fresh provision on board, provided there is a supply of lemon juice; and sometimes, in spite of the neglect of all these particulars, lemon juice alone will prevent it.

Treatment.—The great remedy, however, for scurvy is fresh food, animal and vegetable; farinaceous vegetable substances are insufficient; and when that cannot be procured, then I believe lemon juice will be found the most efficacious medicine. The effects of lemon juice on the disease are speedy and wonderful; so wonderful are they, that the compiler of Lord Anson's voyage says, after describing the disease, and the horrors which took place from its ravages, that the cure of such a complaint seems impossible by any remedy, or any management, that can be employed. Scurvy was formerly set down, without hesitation, as an incurable disease—not only as a disease incurable *then*, but as being so formidable in its nature that it *never* would be cured; and yet in almost every case we can now cure it with the utmost facility. It is not only lemon juice that will cure it, but all the hesperidæi—the lime, Seville and unripe China oranges. Malt and sour crout are thought to have a similar property. The custom, I believe, is to give three table spoonfuls every morning to each man, for the purpose of keeping the disease away. Lemon juice is preserved by mixing one-tenth of spirit with it. One ounce of lemon juice, with one ounce and a half of sugar, is the present navy allowance; and it is said that scurvy rarely occurs now in the longest voyage. Citric acid is thought to be inferior to lemon juice. During the nine years previous to this supply, the average number of sick sent to the

hospitals was one man in three and nine-tenths of the whole navy; and in the succeeding nine years it was only one in eight and four-tenths. The juice is also said to improve the general health. I may mention, as a good illustration of the power of lemon juice, that the Suffolk left England in April 1794, and had no communication with land for twenty weeks and a day, and yet all the time she had only fifteen sick, and those slightly, and soon cured by an augmentation of the first allowance of two-thirds of an ounce; and at her arrival not one had the scurvy. In 1800, the channel fleet, consisting of 24 ships of the line, besides smaller vessels, had no fresh provisions for 16 weeks, but plenty of lemon juice, and not a single instance of scurvy occurred; whereas in 1780, the channel fleet could not keep sea beyond ten weeks, and was worn out with scurvy and fever; and 2500 men were sent into port with the scurvy. We read in Purchas's Pilgrim, that Commodore Lancashire sailed from England with three other ships, for the Cape of Good Hope, on the 2d of April, and arrived at Saldanha Bay on the 1st of August, the Commodore's own ship being in perfect health, from the administration of three table-spoonfuls of lemon juice every morning to each of his men, whereas the other ships were so sickly as to be unmanageable for want of hands, and the Commodore was obliged to send men on board, to take in their sails, and hoist out their boats.

This disease, of course, occurred in ancient times. It was known in the Roman army, in Germany, and also in the impiously denominated "Holy Wars;" but it was first particularly noticed in the crew of Vasco di Gama in 1497. You will find it mentioned by Pliny, as occurring in the Roman army under the command of Germanicus. But with respect to the remedy, its discovery appears to have been left for modern times, but still not recent times, as you will find it mentioned as far back as 200 years ago. There is a curious fact connected with it, and one which is very instructive, as teaching us not to despise any thing without good reason. It is said that when the London College of Physicians was applied to by Government, for a cure for scurvy, they advised vinegar, which has very little power in the affection; and that a Fellow of the College, who wrote on the disease in 1753, never adverts to lemon-juice at all in his Treatise, and yet that, two hundred years ago, it was mentioned in Woodall's "Surgeon Mate, or Military and Domestic Medicine,"—a work published in 1636, "by John Woodall, Master in Surgery"; and he ends his praise of it by saying that he dare not write how good a sauce it was with meat, lest the chief in the cabin should waste it to save vinegar. It is said even to have been mentioned still earlier, in Purchas's Pilgrim, published in 1600. Dr.

Lind, of Haslar Hospital, revived the knowledge of it, more than one hundred years afterwards. He stated its peculiar powers, in the third edition of his work on the Diseases of Seamen, in 1772, but even then it was not brought generally into use, and the navy actually suffered most frightfully from scurvy till 1795. Although the remedy was mentioned two hundred years ago, in one book, and again in a well-known surgical work in 1636, yet the navy suffered from it till 1795, when a good supply of it was ordered by government, when Earl Spencer, the father of the present Chancellor of the Exchequer, was at the head of the Admiralty, on the representations of Dr. Blair and Sir Gilbert Blane, who were commissioners of the board of sick and wounded seamen. In less than eighteen months there was not a case of scurvy in Haslar hospital. In 1780, there had not been fewer than 1457; in 1806, there was but 1; and in 1807, but 1.

So great is the effect of this article, that you will find the following passage in Sir W. Herschel's work, published in Dr. Lardner's Cyclopædia, on the cultivation of the physical sciences. He says, "at present the scurvy is almost completely eradicated in the navy; partly, no doubt, from an increased and increasing attention to general cleanliness, comfort, and diet, but mainly from the constant use of a simple and palatable beverage—the acid of lemon, served out in daily rations. If (he adds) the gratitude of mankind be allowed on all hands to be the just meed of the philosophic physician, to whose discernment in seizing, and perseverance in forcing it on public notice, we owe the great safeguard of infantile life, it ought not to be denied to those whose skill and discrimination have thus strengthened the sinews of our most powerful arm, and obliterated one of the darkest features in one of the most glorious of all professions."

The scurvy is now prevented by great attention to cleanliness—by giving sailors as wholesome food as possible—by attention to exercise and cheerfulness, and by a regular supply of lemon-juice. In spite of all this, however, sporadic cases still occur; but, in general, that is all. I have had myself several cases of this disease in London, and some of them were in persons who had never been at sea, who had eaten no salt meat, but had been deprived of food nearly altogether. Others were sailors, who came on shore labouring under the disease; for in merchantmen there is continually the greatest neglect. I had one patient, a few months ago, who had been sixteen weeks at sea without any medical man on board (that, I suppose, is unavoidable in small ships); who had had nothing but the hardest salt beef, without a particle of any thing else except biscuit, during the whole voyage. He was, as might be expected, labour-

ing under scurvy to a great extent; and he said that several of the crew had died. I am not sure that the lemon-juice, which I gave these patients as a matter of course, did them any good, because they were allowed fresh meat and greens every day, with porter, every article of good diet, and this was quite sufficient, I am sure, to cure the disease. I gave them lemon-juice in addition, because we have such authority for its employment. However, some persons now begin to say that the lemon-juice does no good—that the benefit entirely arises from the other means that are employed, and that the neutral salts, particularly nitre, answer a better purpose. I dare not say, however, that authority, so accumulated and so immense as it is, respecting the powers of lemon-juice, is at all to be disputed. I certainly cannot but think, till we have further facts, that it is our duty in every case to supply lemon juice, or similar things, if it cannot be obtained, in the hope of doing away with the ill effects which a want of fresh food occasions.

I may mention also, that, in regard to local applications, it is found that lemon-juice is one of the best when there is a scorbutic ulcer. I believe a slice of lemon laid upon it is one of the best applications that can be employed, as *Père Lebat* is said to mention, in his *Voyage to the Antilles*.

Now this is a disease which I should say is a chemical disease, if any one be so. In one sense, the constitution is not at all in fault; all the fluids and all the solids appear to be changed, but you have only to give a different chemical state to the body, and the disease is cured. You need give nothing which acts by a specific operation—no drug, I mean, which acts as a medicine—but employ fresh articles of diet, and you remedy the depraved constitution of the whole mass of solids and fluids. I have, therefore, mentioned this disease before I came to any of those which are clearly seated in particular parts. I am not aware that this attacks any one part in particular; it seems to be a cachectic state of the whole frame; and if any disease be an instance of universal disease, I should certainly say that it was scurvy.

There is an affection very similar to scurvy in some respects, which has been arranged and described, by *Willan*, among cutaneous diseases, and which is called *purpura*. Some are of opinion that this is the same as scurvy, but I cannot think so, for reasons which I will state when speaking of diseases of the skin.

I shall, at the next lecture, consider other diseases which, perhaps, have a local residence, but the seat of which is not known: I mean fevers. It is a matter of dispute whether they have a local seat or not, and I shall therefore speak of them the last, before I come to those diseases which are undoubtedly local.

Ossification of the Heart and Spleen.

I have brought with me a curious specimen of diseased spleen and diseased heart, shewing intense ossification. Here is the spleen, with the peritoneal coat exceedingly thickened—so thickened that it has become absolutely white. It is not only thickened, become almost cartilaginous, but within, there is a rare occurrence—the blood-vessels, I presume the arteries, are ossified. On passing your fingers over any of these you will find that the coats of the vessels are bony. The tendency to ossification was so great in this case, that not only the vessels of the spleen but the coronary vessels of the heart have fallen into a state of induration and ossification. There is another curious circumstance—ulceration has taken place in the peritoneal coat of the spleen, which is rather a rare thing; but sometimes you will see ulceration beginning in a serous membrane and going inwards. In general, ulceration proceeds outwards, as is the case in ulceration of the intestines, till at last the peritoneal coat is ulcerated; but here the external coat of the spleen is itself ulcerated, there being no ulceration beneath it. The patient from whom this was taken had no symptoms of angina pectoris, which are said to take place when the coronary arteries are ossified.

CLINICAL OBSERVATIONS

Delivered in the General Dispensary,

By MR. COULSON.

Carcinoma of the Velum Pendulum Palati.

GENTLEMEN,—I will first direct your attention to-day to a singular case which terminated fatally last week; I believe it to be a case of carcinoma of the velum pendulum palati. The history is this:—

Wm. Haslop, *æt.* 25, by trade a baker, and of irregular habits, was admitted under my care Sept. 19th, 1831, for a complaint, as he termed it, in his throat. On looking into his mouth I saw a tumor of the size of a large egg, connected with the velum, and indeed forming part of it. The tumor was separated from the tonsils by a slight fissure on either side; but the tonsils themselves were enlarged, and on the right there was a superficial ulceration of the size of a shilling. It had a very bad appearance, the edges being everted, and the surface covered with dirty-looking irregular granulations. The tumor had been growing, according to the patient's account, since last April; for about that time, or a little before, he felt a slight pain, or soreness, in his throat. This soon left him, and he never complained of pain afterwards, either in his throat, or any other part. His articulation was so indistinct, that I could scarcely make out what he said; he always spoke as if his mouth was full of meat. His breathing at times was labori-

ous; and at night it was so loud that no person could sleep soundly in the room with him. It was described to me (for I did not witness this myself,) that the patient would breathe a short time easily, and without making any noise; that all of a sudden he would start up in his bed, with the feeling as if he were about to be suffocated, and at the same time utter a peculiar sound. He always breathed with his mouth open. This man could swallow solids pretty well, but liquids were frequently ejected through the nostrils, particularly if coughing came on during the act of swallowing. Latterly he had a troublesome cough, with dyspnoea, and copious foetid expectoration. I considered the tumor to be of a malignant character, of which, indeed, there could be little or no doubt. I told him that the complete eradication of the disease was impossible, but that his most distressing symptoms might be relieved, by the removal of as much of the tumor as could be effected. To this he readily consented. I introduced a long, narrow, curved bistoury, with a blunt point, between the fissure and the tumor, and carried the knife from one fissure to the other, as close to the connexion of the soft with the hard palate as I could. In this way I removed a large portion of the morbid growth, which was rather more than an inch in length, and about two in thickness: in the centre of the inferior edge there was a vestige of the uvula (its apex), of a pale colour, but apparently not changed in structure. The hæmorrhage which supervened was rather alarming, and continued more or less for four hours; syncope at last occurred, and the bleeding then stopped. The blood came from a large branch going to the middle of the tumor, most likely a branch of the descending palatine artery, which is, as you know, distributed in part to the velum. The patient was for a time considerably relieved by the operation; he could sleep without rising up in bed as he had been accustomed to do, and his articulation was more distinct. The tumor, however, soon increased in size, his cough became worse, and his health was much impaired. In December he was obliged to keep his bed, and to lay on his back, for the moment that he sat up, or turned on either side, a violent fit of coughing came on. He continued in this state till the beginning of this month, when his cough became less troublesome. On Saturday week last (Jan. 6), he was suddenly seized with spitting of blood, and expired in the night.

I examined the body; and, in order to get a good view of the parts, made a section of the jaw at the symphysis, and removed the tongue, larynx, and a considerable portion of the pharynx and œsophagus. This mass, which I now shew you, commenced, I believe, near to the posterior edge of the palatine bones; I do not think that it was connected with the membrane lining the floor of the ventricles, though I am not positive, as a

perpendicular section of the skull and face was not made. The tumor is of a firm texture—in fact, almost semi-cartilaginous—distinctly lobular; and intersecting it at different parts there are several white bands, composed of condensed cellular tissue. That part of the tumor opposite to the base of the tongue, and in two or three other spots, present a warty appearance, which induced Mr. Langstaff, to whom I sent the mass, to consider it as composed of an admixture of carcinoma and chimney-sweeper's cancer. Beneath the mucons membrane, at the base of the tongue, which is here dissected off, there are several small lobules, of the same structure as the larger mass. The larynx and pharynx were healthy. The tonsils were also involved in the same disease as the velum. The neighbouring glands were a little enlarged. There were strong adhesions between the pleura pulmonalis and costalis, particularly on the right side; and in the upper and back part of the right lung there was a very large cavity, which contained some foetid pus and blood. The bronchiæ and lower part of the larynx were filled with frothy blood. I need not tell you that carcinoma of the velum is of very rare occurrence. Indeed, I do not know where to refer you to a case of the kind. Dr. Mouru, in his *Morbid Anatomy of the Gullet*, says that he had occasion to visit an old lady whose under lip, uvula, and pharynx, had been reduced to a scirrhous state, but he gives no account whatever of the case. Here there was an admixture of two diseases, or rather varieties of the same disease. The mention of chimney-sweeper's cancer naturally recalled to your mind the common situation of this disease, and the persons usually attacked with it. Still, other parts than the scrotum, and other persons than chimney-sweepers, are now and then attacked by this disease. Sir Astley Cooper mentions three cases on the cheek, and Sir Jas. Earle saw the disease on the wrist of a gardener; Mr. Langstaff has met with it in a baker; the man Haslop was also a baker. The chimney-sweeper's cancer formed but a small part of the diseased mass, and we must never be surprised at meeting different kinds of malignant diseases in the same persons, or even in the different stages of the same morbid production. Mr. Langstaff, to whose valuable papers in the eighth and ninth volumes of the *Medico-Chirurgical Transactions* I must refer you, has endeavoured to shew that there exists a close resemblance in the morbid structure of fungus hæmatodes, pulpy or medullary sarcoma, tuberculated sarcoma, and carcinoma, and that these diseases may exist together in the same person, locally and constitutionally, and that fungous tumors may become cancerous, and cancerous become fungous. I may also observe that Mr. L. considers cancer, and the other malignant dis-

eases, to originate in the cellular tissue, and not in the serous or mucous membranes, or muscles or glands, &c. near to which they may have their seat.

In scirrhus, then, the secretion first takes place into the cellular tissue, and the neighbouring parts become involved in the disease. There is a dense, fibrous structure in the middle of the tumor, from which different striae, or bands, sometimes proceed to the circumference. Sir E. Home, in his work on Cancer, calls these ligamentous bands, and considers them characteristic of this disease. I would not advise you, however, to lay too much stress on these bands, for you will sometimes meet with them in tumors not malignant, and not find them in those that are. Describing the section of a carcinomatous breast, which he had removed, Mr. Langstaff says*, "there were a few irregular appearances of condensed cellular substance, such as we notice in any hardened part of the body, occasioned by disease not of the malignant class; but none of these firm whitish ligamentous bands, arranged in the way which authors who have described the genuine carcinomatous affection of the breast have discovered. Nor have I been so fortunate as to meet with a single specimen of this description, although I have examined a great number of carcinomatous breasts, which induces me to suppose that the nature of carcinoma has become so much modified as the genuine chancre described by that very accurate observer, Mr. Hunter." The hardening process generally involves more or less of the surrounding part; some portion of the tumor softens, and the skin at last gives way, when the disease assumes the name of *cancer*. In the case of Haslop there was cancerous ulceration at the commencement on one of the tonsils, and it was the appearance of this that led me at first to decide on the nature of the disease. Those cases where ulceration exists at the commencement of the disease, and in which the morbid growth occurs subsequently to it, are termed by some authors *carcinoma*. The ulceration which occurs at the commencement of cancer is called *cancerous ulceration*, whilst that which occurs in the progress, or towards the termination of the disease, is termed *cancerated ulcer*. These different names, however, to the same disease, only tend to embarrass and perplex the student; it is the result of our imperfect acquaintance with the subject; for there can be no doubt, when our knowledge of this difficult part of pathology, viz. malignant diseases, is improved, the nomenclature will be more simple and rational than it is at present.

Acute darting pains have been described as characterizing the progress of these

tumors, but the patient whose case we are considering complained of no pain whatever after the soreness which attended the first appearance of the complaint subsided. A scirrhus tumor is not necessarily painful in itself: the pain will depend on the nature of the part affected, its situation and structure. Cancer of the face, of the breast, of the womb, of the rectum, are extremely painful, whilst cancers of many other parts, as the lungs, liver, and spleen, are said to be unattended with any pain. The age of the patient should not be overlooked; it was an early age for the development of this disease. The great inconvenience experienced by the patient was in deglutition, respiration, and articulation. Solids were swallowed without much difficulty, though from the size of the foreign body (if I may use the expression) at the entrance of the pharynx, we might have expected obstruction to the passage of solids. But liquids were commonly rejected through the nostrils, and the matter expectorated from his lungs (which I omitted to mention in the history) also passed by the same passage. The velum being so much diseased, and the power of the muscles over it being lost, the posterior opening of the nares could not be closed. I may here observe, that in animals the velum palati appears to serve the purpose of assisting in deglutition, and not to have much influence on the formation or production of sound. It has been observed, that when the velum has been wounded in animals, little or no change takes place in the uttering of sounds, but great difficulty in swallowing. The singular noise which the patient made at night in breathing, is similar to what I have seen in children with pigeon-breast. His respiration was much easier after the removal of a portion of the tumor, which shews that the aperture between the velum and base of the tongue must at times have been completely closed, and produced the sensation of choking which I described. That the patient's articulation should have been indistinct, is not a matter of surprise, for you are well aware that in man, if there be only the smallest opening in the velum, or loss of its substance, a very unpleasant nasal sound is produced. In this instance the man spoke exactly as if his mouth were full, so coarse and indistinct was the articulation. With respect to the disease of the lungs, it has struck me whether it were not probable that by the irritation of the upper part of the larynx, the cough might have been produced which called into action the disease of the lungs. The treatment which I adopted was followed by considerable relief to the patient's symptoms, and I would advise you in cases of this kind to use an instrument similar to the one I employed. The only question is, whether the carcinomatous growth might not have been completely removed if I

* Medico-Chirurgical Transactions, vol. ix. page 383.

had seen the patient early. When I first saw him, the tonsils, in addition to the velum, were diseased. You might, however, have a malignant disease of the tonsil come under your notice, for the cure of which the removal of the part may be necessary. You must therefore bear in mind its relations; that on the external side the tonsil is but a few lines distant from the internal carotid, from which it is separated by the superior constrictor of the pharynx, a plexus of veins, and some nerves. The situation of the artery must guide you also in the direction which you give to the lancet or bistoury in opening an abscess: you should direct the point of the instrument internally, or towards the pharynx, for some fatal cases are on record of the artery being wounded by neglecting this precaution.

Chronic Enlargement of the Tonsils, and relaxation of the Uvula.

These parts frequently become enlarged from repeated attacks of inflammation, and the uvula elongated. Now before you think of resorting to excision, let me suggest to your notice the plan which I adopt in these cases with the most successful results: it is the application of the nitrate of silver, in the form of solution. Take a probang, the sponge of which has been wetted with a saturated solution of the nitrate of silver, and keeping down the tongue with a spoon, or an ivory knife, touch the relaxed parts with this solution. Repeat the application every other or third morning, and after a few applications, the worst cases usually mend.

Disease of the Shoulder-Joint.

John Stannard, æt. 20, by trade a joiner, was admitted December 12th, 1831, with an affection of his right shoulder. Says, that about a fortnight ago, he gave his right arm a jerk, the effects of which he did not feel till two days afterwards, when he experienced a stiffness and soreness in the joint, and an inability to move the arm; to use his own expression, it stuck quite close to his side. The pain extended down the arm, in the course of the biceps to its insertion, and was very severe at night. The joint was increased in size. His general health was much disturbed. I ordered him to lose fourteen ounces of blood by cupping from the shoulder, and afterwards to apply a blister. Two grains of calomel, and a quarter of a grain of opium, every four hours, were given, and continued until the 15th. He was cupped again on the 17th, and afterwards rubbed in the tartar emetic ointment. Strict rest to the limb was enjoined. By these means the severe symptoms subsided, and he has now almost recovered the use of the limb. I entered this as a case of ulceration of the cartilages. The severity of the pain at night, and the

great and rapid disturbance in his general health, led me to form this opinion. You know that in disease of the cancellated structure of the bone, the pain is not so severe at the commencement, nor the general health so much affected, as it was in this case. You may ask me to account for the increase of size in the joint. I should say that the inflammatory action might have extended from the cartilage to the synovial membrane covering it, and thence to the same structure lining the capsular ligament, and an increased secretion of synovia taking place.

ROYAL MATERNITY CHARITY.

To the Editor of the London Medical Gazette.

SIR,

I SEND you my annual statement, embracing the chief particulars of the cases which occurred in the Eastern District of the Royal Maternity Charity during the last year, and am

Your obedient servant,

FRANCIS H. RAMSBOTHAM, M.D.

Physician to the Royal Maternity Charity;
Obstetric Physician to the Eastern and
Tower Hamlets Dispensaries, Lecturer
on Midwifery at the London Hospital, &c.

14, New Broad-Street,
Feb. 3, 1832.

During the year 1831 there were delivered in the Eastern District of the Royal Maternity Charity, under the superintendence of Dr. F. H. Ramsbotham,

2144 WOMEN, of which Cases

- 26 were twins—about 1 in every $82\frac{1}{2}$ cases; of these, in 15 cases both heads presented; in 9, the presentation was head and breech; and in 2, both were breech.
- 1134 children were males.
- 1036 were females.
- 2013 were presentations of some part of the head; of which
 - 7 were face presentations—about 1 in every 310 children; with 2 of these there was also an adherent placenta.
 - 60 were presentations of the breech, or some part of the lower extremities—about 1 in every $36\frac{1}{2}$ children; of these, 13 were twins.
 - 7 were transverse presentations—all of the shoulder or some part of the upper extremities—about 1 in every 310 children. 6 of these patients were delivered by *turning*, the other by *decapitation*. She was about seven months advanced in pregnancy. This was her twelfth child, and the seventh time the shoulder had presented in labour. I had delivered her four times before, my father twice, under the same kind of case.

After her last labour, in June 1830, in which the shoulder presented, a large abscess formed in the thigh, from the effects of which I did not think she would have recovered. When I saw her, in this instance, the membranes had been ruptured an hour; the left shoulder was firmly wedged in the brim of the pelvis, and the uterus strongly contracted round the child's body. The fœtus was excessively putrid. With great difficulty I brought down a foot, but in my efforts to extract, the femur separated from its attachment at the hip-joint. I then introduced a blunt hook over the neck, and with very little trouble separated the head. The body, head, and placenta, were expelled by uterine action, but the uterus afterwards relaxed. The hæmorrhage could not be restrained by any means, and she died in two hours from delivery. She was subject to mental derangement.

In 3 the placenta was implanted partially over the os uteri—about 1 in every 715 cases; two of these were head presentations, one a shoulder. In one of the head cases, the hæmorrhage, although previously excessive, ceased entirely on the membranes being artificially ruptured, and the child was expelled by uterine action three hours after. In the other, six hours elapsed between the artificial rupture of the membranes and delivery, which was also effected without manual or instrumental interference. The hæmorrhage did not quite cease on the evacuation of the *liquor amnii*, but abated so considerably as to induce me to trust to the unaided efforts of nature for the completion of the labour. Under the shoulder presentation, the child was turned.

8 were complicated with dangerous hæmorrhage before delivery, not the consequence of placenta presentation—1 in every 268 cases. In seven of these the head presented, and the labour was terminated without any operation being required beyond the rupturing of the membranes; in one of them a draining had been going on almost constantly for many weeks before delivery; in five, flooding came on soon after the commencement of labour, and ceased entirely, or became greatly diminished, on the membranes being ruptured. In the last, the hæmorrhage did not cease till some time after the expulsion of the placenta. The other case was a shoulder presentation, and the patient was delivered by turning.

In 19 the placenta was retained within the uterus, either by atony or irregular contraction of the uterine fibres, or morbid adhesion between the placental and the uterine surfaces, so as to require the introduction of the hand into the uterus, for the purpose of removal—about 1 in every 113 cases; with all there was more or less hæmorrhage: two were under twins—one of them at about six months; two after the expulsion of the child, under a face presentation; one was after delivery by the long forceps, and the other after craniotomy under convulsions.

6 were complicated with dangerous hæmorrhage after the natural expulsion of the placenta—about 1 in every 357 cases. One of these patients died two hours after delivery, under a shoulder presentation; in the other cases, the hæmorrhage ceased on the application of pressure over the uterus and cold to the pubic region.

5 were delivered by craniotomy—about 1 in every 715 cases. In all of these cases there was a distorted pelvis; one was a second child; the first was born without the use of instruments, after a very laborious labour, under which, I was informed, she had nearly sunk. Having allowed the labour to continue as long as I thought was consistent with the patient's safety, the base of the cranium being still quite above the brim, I applied the long forceps, without much expectation, however, of succeeding by their means; and as the head made no advance during half an hour, although I used as much exertion as I considered myself justified in doing, I perforated and delivered by the crotchet. The extraction occupied more than an hour, and nearly the whole of the brain was evacuated before the head passed. The child was alive at the time of the operation. Another was under convulsions; it was a first child. The patient, after eighteen or twenty hours of strong labour, was suddenly attacked with a convulsive paroxysm. I saw her in about an hour; the spasms were then very violent, and almost incessant; the head was pressing on the perineum. The abstraction of more than forty ounces of blood produced no effect on the symptoms. The operation was most difficult, owing to the narrowness of the pelvis and the convulsive contortions of the body. Instantly on delivery the convulsions ceased; she had only one slight fit after. In about ten hours she be-

came sensible, but a state of partial stupor continued a few hours longer. A large number of leeches were applied to the temples, which very much relieved the head, and she gradually recovered completely. The child was dead at the time of perforation.—The other woman my father delivered, during my attendance on a patient a few miles from London. It was a first child. The case was one of great difficulty, and she died on the sixth day after delivery, never having rallied from the exhaustion consequent on her labour.

3 were delivered by the long forceps—about 1 in every 715 cases; in all, the pelvis was narrow. One was the eleventh child; her labours had always been lingering; the child was much larger than the average size. Another was the thirteenth child; she had been delivered by instruments twice before; the child weighed $10\frac{1}{2}$ pounds avoirdupois. The other patient was delivered by a professional friend during my attendance on a lingering case of labour. All these children were born alive.

4 were complicated with puerperal convulsions—1 in every 536 cases; three of these were first children. In one case, craniotomy was performed, as already noticed; in the second, during a lingering labour, while the head was pressing on the perineum, convulsions suddenly supervened; the child was soon after expelled; three more fits occurred after delivery, before I saw the patient; she was completely relieved by one bleeding, carried to the extent of thirty ounces. In the third, two or three slight fits occurred previously to delivery; they appeared to have ceased after the birth of the child. I did not see her, nor were any means used, until seven or eight hours after delivery, when they recurred, and six or seven fits followed quickly in succession. They were, however, totally cut short by one bleeding, to between twenty and twenty-five ounces. The last case occurred in the mother of a family, one week before the accession of labour; she suffered three paroxysms rapidly in succession, and was completely relieved by one very large bleeding; no untoward circumstance happened during her labour. All these patients recovered perfectly well, and the children of the three last were born alive.

In 3 premature labour was induced—about 1 in every 715 cases; all these women had narrow pelvis. For one, I had performed the same operation in 1830, and twice previously I had

delivered her by the long forceps. This was her tenth child; they had all been born dead except one; this also was expelled dead, 100 hours after the operation. In the second case it was the sixth child. All this woman's children also had been born dead, but one, which was premature, and died in a few hours. I had once delivered her by *turning*, under a shoulder presentation, on which occasion I was obliged to perforate the head before I could extract it. In another labour, craniotomy was performed by another practitioner; in this instance the child was born alive, 115 hours after the operation. The last case was the ninth child, all of which, except one, had presented in labour preternaturally, and all were born dead. I had twice delivered her under a shoulder presentation, and had induced premature labour for her four times before; in this instance, the breech was the presenting part, and the child was expelled dead, 43 hours after the operation.

In 1 case, the upper part of the vagina ruptured under labour, and allowed nearly the whole of the child's body to pass into the abdomen. It was the patient's seventh child; her labours had always been lingering; the pelvis was considerably contracted at the brim. I delivered her by *turning*. She died in about twenty-four hours after. On dissection, a transverse rent was found at the upper part of the vagina, between the uterus and bladder, through which the child had escaped into the abdominal cavity; neither of the latter viscera were in the least degree implicated. From the intimate connexion existing between the necks of the uterus and bladder, I should have thought it almost impossible for so extensive an aperture to be formed between these organs without either of them partaking in the lesion, had I not proved the case to have been as described by *post-mortem* examination.

In 1 case, a second child of twins was delivered by *turning*, twenty-four hours after the birth of the first; it presented with the head, and was born alive. A slight hæmorrhage occurred after the expulsion of the first child.

12 women died within the puerperal month—about 1 in every 179 cases.

2081 children were born alive.

81 were born still—about 1 in every 24 $\frac{1}{2}$ births.

Of the Deaths,

1 was from inflammation of the uterus, twenty-six hours after delivery. I saw the patient under labour; the

uterus was excessively tender, but acting vigorously. The labour was not of more than average duration, and the child was expelled naturally. The disease must have existed for ten or twelve weeks, and the poor woman had never applied for medical assistance until after the commencement of her labour.

1 on the sixth day after delivery by craniotomy. She never rallied from the exhaustion produced by her labour.

1 about twenty-four hours after delivery, by turning, under lacerated vagina.

1 from hæmorrhage, two hours after delivery, under a shoulder presentation, at seven months.

3 were from the effects of adherent placenta; one from flooding, half an hour after removal. I was not at home; a professional friend attended in my stead. The second was on the seventh day, from irritative fever; it was a case of twins, at about 7½ months. One of the placentæ was strongly adherent; the patient had not lost much blood. The last was from the same cause, on the tenth day; she had lost a large quantity of blood; I have little doubt that, with common caution, her life might have been preserved, for, on the third day after her delivery, I found her dressed, and sitting by the fire.

2 were from confirmed *phthisis*—of which disease, one patient had been the subject three, the other five years; one died on the eleventh, the other on the twelfth day after delivery.

2 from inflammation of the lungs; one on the eighth day after delivery—she had been subject many years to what she termed *asthma*; the other on the ninth day—she had suffered from similar attacks after most of her labours.

1 three weeks after labour, from a sudden attack of *hæmoptysis*, of which she was the subject during the latter weeks of her pregnancy. I had no opportunity of ascertaining the state of her lungs. Indeed I never saw her; she had been attended, during her pregnancy, by a gentleman in her own neighbourhood, and continued under his care after her labour. The particulars I learned from her midwife.

Of the Stillborn Children,

29 were premature.

11 were putrid at full time, or nearly so.

13 were breech presentations, at full time, or nearly so.

3 were shoulder presentations.

1 was after the mother had suffered from dangerous hæmorrhage, under a partial placenta presentation.

4 were after accidental hæmorrhage.

3 were delivered by craniotomy.

1 was delivered under a rupture of the vagina.

2 were under very lingering labours, both the children being excessively large.

With 7 the funis prolapsed by the side of the head, and could not be returned.

With 1 the funis prolapsed by the side of the breech.

14 were at full time, or nearly so, head presenting, not putrid, nor delivered by art.

ANATOMY BILL.

To the Editor of the London Medical Gazette.

SIR,

SEEING in your Gazette of February the 4th a letter upon the Anatomy Bill, by that deservedly eminent surgeon, Mr. Travers, and believing that one of his eight desiderata contains a principle subversive of the best interests of medical science and the public good, I solicit a page in your valuable journal for a few remarks.

Most sincerely do I accord with him in those expressions of dissent which he makes to every species of traffic in human flesh. I agree with him also in desiring the repeal of that obnoxious statute which consigns the bodies of murderers to dissection; and further, I approve highly of the mode of obtaining bodies for dissection, as set forth in his fifth desideratum; and I do so, because I perceive in these means the accomplishment of what is really desirable and necessary for the acquisition of sound medical knowledge—for the total abolition of that most repugnant calling, a resurrectionist—and of that most horrible crime, *burking*. But when Mr. Travers descends from the high position of endeavouring to obtain for his brethren a power of legally pursuing their anatomical studies unmolested, and stoops to recommend the adoption of severe restrictive measures, which will affect thousands of his fellow-labourers, we part company immediately; and I shall consider it a duty to oppose (however feebly,) all such contracted, partial, and unjust views.

The second *desideratum* of Mr. Travers has this remarkable expression:—“The licensing of schools of anatomy, and the prohibition to practise anatomy, except in the licensed schools, under a heavy penalty.” And again, at the con-

cluding paragraph of his letter, he gives it as his decided opinion, that it would be "injurious to the interests of the medical profession, as well as to those of the public at large, *that anatomy should be practised except in licensed schools.*"

I trust that Mr. Warburton, and all other members of Parliament whom this gentleman addresses, will perceive the *excluding, fettering, illiberal, and monopolizing* character of this proposition—will anticipate its evil consequences, and never allow such to become incorporated with any anatomical bill which may pass their honourable House.

I will now, in a few words, substantiate the epithets which I bestowed upon this monstrous, narrow-minded proposition, and then point out its mischievous tendencies. By the above measure, not a single physician, surgeon, or general practitioner, is allowed to take scalpel in hand for the purpose of dissecting anatomically any human body (unless at some licensed school), without incurring a "heavy penalty." Now every one with a grain of common sense will perceive this to be *fettering* with severity, and *monopolizing* beyond all precedent; because he knows that those who preside over and teach at the large metropolitan schools are aware that such a measure must compel every student who is learning anatomy, and every practitioner who may be desirous of preserving that anatomical knowledge which he has learnt, to resort again and again either to London, or some other place where there exists a licensed school. At present every medical man, wherever stationed in practice, is at liberty to dissect, and keep up his anatomical and surgical knowledge, but pass Mr. Travers's desideratum into a law, and then who will run the risk of incurring "heavy penalties?" Such a plan might prove, as it doubtless would, a grand *desideratum* to all teachers of licensed schools, but would it be advantageous to the medical public, or beneficial to the community at large? I unhesitatingly say no.

It is an old but wise saying, that whatever is worth learning is worth retaining also; and in no case can this be more strikingly applicable than to that of a medical education, because it not only interests those who are educated, but promotes in particular the welfare of mankind. Its great utility, and universal daily application, render it of paramount importance. This being admit-

ted, what are the means which conduce to its attainment and preservation? Practice and reflection combined.

The human mind is known by past experience to be most fallible, and impressions on it (however strong) are too apt to become effaced, or weakened, by time, unless care be taken to renew them occasionally. Anatomical knowledge, surgical adroitness, and medical acumen, are, in common with other acquirements, influenced by similar causes, and require similar means for their preservation. Who is there about to undergo a surgical operation that would not select (if allowed) the most skilful and experienced operator, and who is known to dedicate a portion of his time to the preservation and increase of his skill?—and where the person, throughout these realms, who is not often liable to require immediate surgical aid? and how can this aid be so *effective* as when facilities are *extensively*, yet *legitimately*, given to dissection? But I will refer all those who are aiming to coop within narrow limits the practice of dissection—to confine it within their own arena, and who would for ever prevent the practitioner from touching it, (save in a licensed school)—to one quotation from the Anatomical Report of a Select Committee of the House of Commons in 1828.

"But it is not only to the student, while learning the rudiments of the science, or to the teacher while endeavouring to improve it, that dissection is necessary, and the operation of the law injurious. It is essential also to the practitioner, that during the whole course of his professional career he should dissect, in order to keep up his stock of knowledge, and to practice frequently on the dead subject, lest by venturing to do so unskilfully on the living, he expose his patients to imminent peril. He is required also, in many cases, civil and criminal, to guide the judgment of judges and jurors, and would be rebuked were he to confess, upon any such occasion, that from having neglected the practice of dissection he was unable to throw light upon a point at issue in that science which he professed. He is liable in a civil action to damages for error in practice, due to professional ignorance, though at the same time he may be visited with penalties as a criminal for endeavouring to take the only means of obtaining professional knowledge."

Arguments like these are unanswerable, and abundantly prove an indispensable necessity for two things: the one that of legalizing dissection, and the other that of diffusing its practice most universally. If men who are in the daily habit of performing operations, and of enjoying every opportunity of dissecting, (as in London) require to operate on the dead body, in order to refresh their memories, and mature their plans, (which is known to be the case) how much more shall the country practitioner require it, whose occasions for operating are comparatively rare, and yet imperative, (as in hernia, &c.) These latter, then, most clearly demand, upon every rational ground, an increased facility for pursuing dissection, and therefore that it would not only be impolitic and injurious, but unjust, to enact a law which shall fetter the practitioner, and prohibit him from dissecting at home, by heavy (doubtless very heavy) penalties, thereby compelling him either to grow old in ignorance, or at the greatest inconvenience and expense—absolute loss, to resort from time to time to some public schools. Such a measure in this age of growing intelligence and liberal feeling would be a blot upon legislation, and the consequent monopoly of great schools would become odious and intolerable.

I know that Mr. Warburton's views upon this question are most liberal, and that he desires to obtain an act which shall benefit the public at large. I trust, therefore, that he will make a stand against all narrow-minded monopolizing views, from whatever quarter they may proceed, and that both houses of Parliament may support and assist him in so good a cause!

Since writing the above, another letter of Mr. Travers has appeared in your columns, wherein he avows the alleged hardship upon practitioners in the country wishing to refresh their knowledge, to be "*a pure delusion*," and that they may, if desirous, resort to the metropolitan, or provincial schools, to brighten up their ideas. This is being rather gratuitous as regards the delusion, very candid in the avowal, and extremely kind in the invitation. I know many who do take advantage of that liberty which they now enjoy of keeping up their anatomical knowledge. Mr. Travers must allow that without it they could not operate with a chance of suc-

cess equal to operators in London. He must know, also, that however "in earnest" a country practitioner might be to refresh his anatomical knowledge, and however well-appointed and comfortable his "private rooms" in the London schools might be, it is in the great majority of instances quite impossible to leave their patients, and that so doing would prove their utter ruin. Hoping that you will excuse the freedom of my request, I am,

Your obliged and obedient servant,

JOHN WAYTE, M.D.

Lynn, Feb. 14th, 1832.

DISSECTION VIEWED WITH REFERENCE TO THE RESURRECTION.

To the Editor of the London Medical Gazette.

"Si trapassamo per sozzo mistura
Dell' ombre e della proggia, à passi lenti,
Toccando un poco la vita futura."
DANTE, *Inf. Cant. 6.*

STR,

A SURVEY of the world in its present condition displays a strange anomaly of tyranny and freedom, of ancient prejudice and novel wisdom. Men have not yet learned to despise the panoply of political governments; and myriads are still ready to delight in the moving miseries, the useless cunning of military tactics. War, with its disastrous consequences, is still the glory of mankind; and a multitude of particular evils is merged and forgotten in the cruel splendour of a day of slaughter. Life may still be sacrificed with impunity at the caprice of a single man; and a king may bear of an army lost, and coldly confer the haughty favour of saying, that his faithful subjects do their duty. But in every nation there are now arising men who profess the ways of peace, and acquire and diffuse, with active benevolence, the simple truths of science. The discovery and application of a solitary fact, conducive to human safety and human happiness, is a crown more lasting than that of diadems; and the miner who is enabled to explore, with personal security, the perilous passage of his dungeon, is enlightened and protected by the efforts of a genius cultivated in the daylight of peace. Astronomy instructs us to contemplate the particles of a universe; botany traces along the heather, glade, and brake, the varied pencil of a present

Deity; and anatomy develops the organs of life, and assists us in alleviating the multifarious afflictions of a world. Nevertheless, the same nation that would readily supply a thousand men with arms for slaying, slowly concedes the utility of anatomical dissection, and superstitiously conceals the remains of those very bodies whom it delights to immolate in the wanton exultation of conquest. The soldier is exalted for his prowess,—the anatomist is favoured for his knowledge; but still the noisome carnage of a field of battle is deemed more admirable than the arteries of a limb beautifully displayed by the knife of the careful dissector.

Death is more interesting than life. In the darkness of the grave we are taught to behold the brightness of futurity; and in the passive structure of the corpse, we are led to discover the active mechanism of life in all its functions. The knowledge of anatomy is the knowledge of nature, as demonstrable in the noblest work of the Creator, and as applicable to the noblest of his attributes—peace and good-will. In exploring the dead body, the anatomist is in the pursuit of truth, and is perpetually endeavouring to ascend from death to life, from the mere organism of matter to the laws which govern matter itself when alive. In pursuing these delightful speculations of intellect and reason, his labour is not vain; he is learning to meet the casualties of life, to adjust the fractures of a limb, and to fortify the blessings of health. From the filthiness of death, he learns the excellence of life; he unfolds the nice inter-dependence of different organs; he traces throughout every part the slender rudiments of minute anatomy.

Such and so fair is the study of death, when we have once been admitted between the veil, and behold within the sanctuary the inmost fashion of the temple. But the temple stands in the obscurity of vulgar superstition.

The full blaze of christianity has not yet dispelled the mists that still linger over the last term of human existence. The resurrection of the dead is the source of error; and the populace, ever more eager to preserve their latest remains against that awful moment, forget that the frailty of earth is not the perfection of heaven. The spoliation of disease and the decrepitude of age, the imbecility of infancy and the precarious

virility of manhood, afford but feeble notions of a glorious and eternal vitality. Happiness could not be complete in a body perpetually liable to dissolution; virtue could never be accomplished in a being whose passions are always opposing his reason. Eternity itself would be scarcely worthy of acceptance on the terms of mortality; and the fleeting particles of matter, which are incessantly conforming and renewing our corporeal fabrics, at once reduce to an absurdity the notion of their eternal subsistence. Impossibilities can never be true. In reasoning upon this subject, human intellect is at fault; and we are compelled to submit implicitly to the words of revelation, while learning the final destination of our bodies. Scripture has not left us doubtful in this “land of darkness and the shadow of death:” it has pierced beyond the grave; it has unfolded the gates of that dread abyss; it has suffered the light of heaven to gleam in grateful splendour on the world. We are assured, that our present bodies of flesh and blood form not a part of eternity; that the body that is sown is not that body that shall be, and that our individual identity shall develop itself in a state of novel independence. A grain of wheat is the appropriate emblem of this subject: it is sown in coldness and moisture, its blade ascends, and its ear ripens into the colour of gold under the opening aspect of the skies. We shall, indeed, possess a substantiality, a visibility, and a sensual perceptivity, but it shall be remote from terrestrial enjoyments, and above the conditions of matter. Nor shall the putrefaction of the corpse prevent this wonderful metamorphosis; for when time shall have an end, the mountain and the sea, the costly mansolemn and the dingy vault, shall alike divulge their morsels of corruption in the clothing of immortality. “And I set me down in the midst of the valley which is full of bones, and they were very dry; and there was a noise and a shaking, and the bones came together, bone to his bone; and the sinews and the flesh came upon them, and the skin covered them above, and the breath came into them, and they lived and stood up an exceeding great army.” The same event shall happen unto all men: to the savage who has been devoured by his fellows, and to the monarch who has been entombed in all the pomp of regal

magnificence. The rite of sepulture will not confer the right of resurrection; nor will the artificial preservation of the corpse (and the mummy even has a limited duration) make its security greater in heaven. The rite of burial is profitable only to social decency; for if it be indispensably necessary to salvation, what is the lot of those who have died unavoidably without it?—of Mungo Park, who perished in the wilds of Africa—of Cook, the navigator, whose bones have whitened on a foreign shore?

What then shall we say for the corpse? Shall not the anatomist dissect the body which must needs of itself melt into putrescence? can he profit the living by neglecting to learn from the dead? does he prevent the resurrection of a world by learning the nature of its dead before the whole is dissolved? Nay, but anatomy is good, because it is humane; and it is as worthy of the thoughts of ministers and the eyes of princes, as all the obscurity of politics and the captivating glitter of arms. The dead rise; and the knife of the anatomist, while unlacing the body for the good of mankind, can *not* nullify the eternal fiat of the Creator.

These observations have been made, because it is vulgarly supposed that dissection is a sacrilege on popular worship. Dissection from mere wantonness is impiety; dissection for knowledge is wisdom. If we are wise, we shall revere our intellects, and not our bodies; we shall use, and not abuse, our limbs and animal functions while yet they are ours in life; and, resting in hope, we shall have no fear concerning our deaths, whether our bodies become the dust of the grave, the food of maggots, or the subject of the anatomists; or whether, by any chance medley in human affairs, they be lost and wasted in the waters of the ocean.

MEDICULUS.

Jan. 21, 1832.

WOUNDED ABDOMEN.

To the Editor of the London Medical Gazette.

Yoxford, Feb. 2, 1832.

SIR,

As you were pleased to publish, in a recent number of your valuable periodical,

(page 585) some particulars of a case of wounded abdomen with which I furnished you, and as the comparatively short time which had elapsed since the infliction of the wound might have induced some of your numerous readers to entertain less sanguine hopes of the result than I did, I am induced to trespass yet again on your time and space, happy in being able to inform you that the case has terminated most satisfactorily, so far as the bodily health of the patient is concerned;—the poor fellow, however, has settled gradually into a state of idiocy.

A part of the early treatment of the case I omitted to mention. On succeeding in replacing the intestine, which had been protruded, by his struggles, between the sutures, I ordered constant gentle pressure to be kept up on the wound with a cushion of linen, by the hand. This was done until the edges of the wound had sufficiently consolidated, so as to remove all risk of further similar accident. While engaged on my knees (for I found him, on entering the house, on the floor) in reducing the displaced viscera, he more than once said, "Cut it a little more, sir; you won't be able to put all that back through the small hole I've made." This was said with the greatest *sang froid*, contemplating the effects of his rashness with the most perfect composure. It would have perhaps appeared strange to the bystanders had I been eventually obliged to adopt the very course which he recommended, and which, I think, would have been correct practice in the event of extreme difficulty. In the after-treatment of such cases, I conceive that nothing but strict quiet, and great attention to the system and diet, can avail. The freedom from any of the usual symptoms of extravasation must be attributed to that general and equal pressure of the parietes of the abdomen which, more than anything that art can contrive or effect, tends to confine and fix the viscera, whether actively engaged in the process of digestion, or in a state of repose. I have assumed, on the evidence of the knife being covered with fæces, and the escape of fætid gas from the wound, that an intestine was wounded; no part, however, which was displaced, was found to be so, being all greatly distended with flatus. It is, I think, reasonable to imagine, that the tenderness and slight

external inflammation which occurred subsequently, and required leeching, was the effect of an internal adhesive process, which has proved his eventual protection from a fatal result.

Should these further remarks be acceptable, you will oblige me by giving them publicity.

I remain, sir,

Your obedient servant,

JOHN JAMES HALLETT.

ON THE BEST METHOD OF PRACTISING PERCUSSION.

To the Editor of the London Medical Gazette.

SIR,

THE following methods of percussing the chest are at present in use:—

1st. By striking the chest with the ends of the fingers. This is objectionable, on account of the pain inflicted by it in pleurisy and other similar affections, in which percussion is most likely to be tried. In the case of females also it cannot be expected that it would be permitted; and although in hospitals, and in the instance of some private individuals, no obstruction may be offered, yet the method not being of universal application, it may be wanting at the crisis when the practitioner is in the utmost need of it.

2d. By percussing with the ends of the fingers of one hand on a finger of the other hand laid on the chest, in order to receive the blow. Undoubtedly by this the patient is saved from being hurt, but, in consequence of the difference of action between the right and left hand, it is hard to percuss a patient placed in a bed, without changing the angle at which the fingers strike, so much as to obscure very considerably the sound of fulness or emptiness, upon which our judgment of the case is to be formed.

3d. By percussing with the fingers on an ivory plate, as recommended by M. Piorry.

4th. By percussing with the fingers on a piece of Indian rubber. Both of those methods are objectionable, on account of the different angles at which the fingers of the operator strike, and the different sound thence produced; but chiefly on account of the superficial sound produced by the stroke on those substances, and which is so loud as to

obscure the sound of fulness or emptiness—the great desideratum to be attained.

The method of percussion adopted by Dr. Osborne, of Sir Patrick Dun's, although very simple, yet has been the result of many trials, made under the conviction that the methods now mentioned are all of them attended, with uncertainty and difficulty to the young practitioner. He uses two pieces of sole-leather of a circular form, and about an inch and a half in diameter, and percusses with the edge of one, perpendicularly, on the surface of the other. The accuracy with which these render the sound of fulness or emptiness is quite remarkable, and appears to be owing to the leather and integuments of the body having nearly the same density, and consequently rendering the same vibrations. The leather must be of a porous texture; for if it has been hammered, the superficial sound takes place which is so objectionable in the ivory and Indian rubber. To strike with the edge of another circular piece is advantageous, both on account of the ease with which the stroke is made at the same angle, and also because the superficial sound is thus reduced to the least possible, inasmuch as the periphery of a circle can touch a right line only in a point. It is necessary, however, to observe, both with respect to this and any other means of *mediate* percussion, that it must never be performed till the substance to be percussed has been placed in the closest possible contact with the patient's skin. If this has not been done, then hollow sounds will be communicated from the intervening space, and the true sound of the interior will not be obtained.

Yours, &c.

J. O.

Dublin, Jan. 1832.

OBSERVATIONS ON THE CHOLERA AS IT WAS EXPERIENCED AT ST. PETERSBURGH*,

In the course of last Year.

By DR. OCKEL,

Counsellor of State, Director of the School of Midwifery, &c. &c.

THE greater number of our physicians believe that the cholera is not conta-

* Translated from the original French MS. in our possession.—E.G.

gious. I am of that number—that is to say, I believe that the disease does not communicate itself by immediate contact of either persons or clothes, but by a principle in the air allied to *malaria* and m^rshy exhalations.

The occurrence of instances of contagion is attended with much doubt; examples of the propagation of the disease through the agency of the atmosphere are much more common; and the latter hypothesis is well supported by the circumstances which are usually found in connexion with the sudden appearance of the malady, and its rapid spread in the localities where it makes its attacks. A number of persons, in fact, will always be found in those places, who, though they may escape the cholera itself, yet are subject to certain indispositions evidently connected therewith, and which they undergo in a greater or less degree, according to the predisposition of their systems to be affected by atmospherical influences.

This, however, is an inquiry of far less importance than that regarding the best mode of treatment. So many methods and remedies had been recommended, that it seemed to be the wisest course to adopt that which accorded best with our views of the nature of the complaint. No one particular remedy had hitherto been, apparently, more successful than another. During the first fortnight most of the patients died. The mortality after that began to diminish; and in the last period of the disease, a greater number were cured than died, though generally, and on the average, more than fifty per cent. perished. We used vapour baths and frictions with some success, as we already had done at Moscow,—the supervening perspirations, by the way, ought not to be pushed too far,—and bleeding in the commencement, or after the crisis in the (so called) period of reaction, we also found to be not unfrequently successful. But I shall not proceed to detail the several observations of this sort which have been made by others in the course of their practice; I will speak of what I myself observed; and I may be allowed to mention, that some of the following facts have been communicated to certain German physicians, and are already published in the 302d number of the Frankfort Journal. I should inform the reader of these remarks, that the time

of my experience at St. Petersburg was during the second period of the disease, while it was still very fatal; but it is my anxious wish that the method which I recommend should be fairly tried in the first period, in which, I am strongly inclined to believe, it will be found equally efficacious, inasmuch as I only employed it in the very worst cases that came under my care, treating those which were not so severe in the ordinary way. The results of my method were the loss of not more than two in fifteen, while of the common cases treated after the common way, far more than the proportion of ten in fifteen were lost,—a fact which I beg particularly to recommend to the notice of the profession.

Being entrusted with the inspection of a part of the city which embraced four districts and two hospitals, I had an opportunity of seeing about 600 patients, and was enabled satisfactorily to make the following observations. My earliest notions of cholera were derived from the works of English authors who had seen and treated the disease in India. I availed myself also of the experience derived from the appearance of the malady in Moscow; and the conclusion at which I arrived was, that the essence of the cholera consisted in palsy of the circulatory system. This was the theory adopted, too, by my colleagues generally, with the exception of a few, who held that the disease was one of the blood itself, coagulated by the direct influence of miasmata. The external treatment which was found to be most efficacious seemed to confirm our views—vapour-baths, frictions, sinapisms, and moxas. With respect to internal treatment, we found ourselves at liberty to choose out of an infinite variety of remedies, variously recommended, and, in truth, our principal embarrassment arose out of the profusion of our stores. I determined, however, to be cautious as to what internal remedies I should employ until I had formed a better judgment of the indications afforded in the progress of the disorder. The high eulogium pronounced on the oxide of bismuth by Dr. Leo, of Warsaw, induced me to begin with that. I also tried, at the same time, other remedies, which were extolled in like manner—such as nitric and nitrous acid, &c.; but not one of them seemed superior to another.

Whatever patients survived the first attack appeared to enjoy a sort of convalescence, in which there was a respite from the severest symptoms—such as the icy coldness of the extremities, the acute pains, the vomiting, purging, &c.; but after the second or third day, the appearance of a new set of symptoms, of a very different character, shewed that what was taken for a crisis by no means deserved that name. A new stage of the disease began; and I was surprised to find that it was of a very different kind in different persons. Most of the patients became delirious, and then lethargic—a state in which the greater part of them perished; and this it was that induced many of the medical attendants to consider cholera as a nervous fever. Other patients, however, had none of those nervous symptoms, but suffered from inflammation—generally of the liver, but sometimes of the parotid glands. Others, too, had fevers of the gastric, bilious, or the purely inflammatory kind, without local affection; and, in fine, there were other examples in which a convalescence steadily succeeded the first attack, which had resembled intermittent or apoplectic fever. The extreme variety which appeared to belong to the second stage of the disorder, led me to infer that it was not really a *stadium reactivus*, as it was called, but a true secondary disorder with which we had to contend, brought on by the efforts of the system, which had not been relieved thoroughly from the first attack with a well-marked crisis. Hence I was encouraged to look out for other remedies of more virtue than had hitherto been employed; and hearing much of the great efficacy of *salt*, which had been extensively used in one of the hospitals, by M. Esenbeck—without any communication with that gentleman, and indeed by no means sanguine about the success of the remedy—I ordered it in doses of a table-spoonful of the solution of muriate of soda, in a pint of warm water, to be repeated every hour, for a patient who was carried in labouring under one of the most severe forms of the disease. A large and strong sinapism was at the same time directed to be applied to his stomach, and his body was rolled up in a hot blanket. The first effect of the salt was to increase the vomiting, but in less than an hour the nature of the ejected fluid had become decidedly bilious, and an

hour and a half had not elapsed when the patient threw up an enormous quantity of bile. There never was such a change for the better: all his formidable symptoms had vanished as if by enchantment; his *facies hippocratica* was completely gone; his pulse was now full, like that of a man coming out of a hot-bath; the extremities, which had been icy cold and black, were now of their natural colour, and bathed in a comfortable perspiration; and, in short, the poor creature, who could not utter a syllable on his arrival, was now able to tell me, in a good loud voice, that he was “much better.” He had several bilious stools next day, and three days after went home quite recovered.

From this time I ordered solution of muriate of soda for all the severe cases that came to the hospital. There were fifteen of them of the worst description; and all, in the course of an hour after taking the salt, threw up a great quantity of bile. Thirteen were saved. Of the two that died, one sunk through weakness, on the third day; the other lethargic, on the fifth day. Those who passed bile from the bowels were speedily well—in three or four days at farthest; but the others, who had not such stools so soon, remained ill for some days longer, with many of the unequivocal choleric symptoms, which yielded, however, eventually to some doses of tincture of rhubarb. Several of my colleagues, to whom I communicated these facts, experienced the same happy results from the use of salt. It seemed that the solution acted rather by virtue of its efficacy in relieving the spasm of the biliary ducts, than as an emetic; and I am persuaded that the true method of treating cholera is by evacuating the bile as soon as possible. Those who direct their attention to allaying the striking symptoms merely—the vomiting and purging, for instance—may succeed, probably, in getting over the first stage; but they will produce no salutary crisis, and the secondary disease will come on, which never is the case when the bile is duly drawn off. It is clear that the sooner this is done the better, as it will obviate much of those dangerous congestions which are apt to take place in the head, and may lead to consequences of a fatal kind.

The morbid appearances observed after death in Russia have been like those noted in India—the gall-bladder full of

bile, the ducts spasmodically closed—very rarely absence of bile. I fear, however, that sufficient attention has not been paid in those necroscopic examinations to the stage of the malady in which the patient sunk.

Many of my colleagues, as well as myself, have observed that the best preservative against cholera, when certain premonitory symptoms are present, is a good emetic. And we had occasion to observe, that when the disease was on the decline, numbers who had been treated on the calming system, were long recovering, and still much troubled with vertigoes, pains, cramps, &c.; while no instance of this sort was ever observed where the bile had not been early discharged.

On the whole, I have been led to alter very materially my opinion regarding the essence of the disease, and to conclude that the miasm, acting principally on the biliary system, causes an abundant secretion of bile; which being arrested by the spasm of the ducts, induces ultimately spasm of the *vena-cava*. Hence the disorder in the circulation, and in the blood itself—nervous sympathy giving rise to many of the other symptoms, as, for example, the serous effusion in the stomach and bowels, the vomiting, purging, and vertigo. I shall not add a word with reference to the treatment of the (supposed) secondary disease—or *stadium reactionis*, as it has been called—it being, in my opinion, purely accidental; and whatever it might seem proper to prescribe in such circumstances—such as bleeding, leeches, antiphlogistics, and so forth—being stated at length, and ably too, in the protocols of the faculty of Riga.

(Signed) OCKEL.

Conseiller d'Etat et Directeur
de l'Ecole des Sages-femmes
à St. Petersburg.

Nov. 9, 1831.

GENERAL RESULTS OF EMETICS IN CHOLERA.

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To the Editor of the London Medical Gazette.

SIR,

A GREAT deal has been said regarding the priority of administering mustard as an emetic in Cholera, and various claimants have appeared in the field to snatch what little honour there is re-

specting its *invaluable* discovery. With regard to emetics in Cholera, it is needless to state that I was the first person who fully tried their effects in England, and so sensible was I of their use, that I constantly carried ipecacuanha about with me. Certainly Dr. Johnson and I both had one object in view—that of restoring the balance of the circulation; and it occurred to me long ago that if I could only produce full vomiting, that end would be obtained. Since then I have had much pleasure in seeing the precise object stated in the work of my talented friend, Dr. Holland, where he fully shows the exact results of experiments with emetics. As to the use of mustard as an emetic, I do not approve of it. In the first place, should the functions of the stomach not be suspended or destroyed, every purpose is answered by the ipecacuanha, and where the vitality of the stomach is affected, all emetics are useless, as I have repeatedly witnessed. Again, the mustard produces inflammation of the mucous membranes, (as I have ascertained in some instances on examination after death,) which is a serious objection to its internal use; and then the abuse of the thing is beyond all imagination. In one case, that of E. Stoddart, in the hospital, (which was also placed under the care of Dr. Lindsey, but which he has, no doubt unintentionally, forgotten to mention in his report)—I say in that case the quantity of mustard given was enormous, without the slightest advantage: therefore it will be readily seen that every benefit to be hoped for from these remedies, will ensue from the use of mild emetics. But I do not attach much importance to the use of emetics alone: my plan is, (time being every thing in Cholera,) if called sufficiently early, to abstract blood, conjoined with the administration of an emetic. In the vast majority of cases, the dreadful collapse, and all the attendant horrors of this disease, may thus be prevented: I have repeatedly tried the practice, and can speak confidently of its almost specific effects. In those cold blue cases, where all the various functions of the body are suspended or destroyed, I entertain a high opinion of galvanism, with dry heat to the surface, but I had no opportunity of trying its power, as the galvanic battery which I ordered did not arrive in Sunderland until after the hospital was closed.

In a work now in the press, by Dr. Haslewood and myself, cases will be given, fully proving the advantage of the practice stated in this letter.

I am, sir,
Your obedient servant,
W. MORDEY, Surgeon,
Late in charge of the Cholera Hospital.

P.S. I beg leave to add a few words on the subject of quarantine, which, as far as this place is concerned, seems to have been strangely misunderstood and misrepresented. The medical gentleman who generally examined the crews is well known, in great practice, and of high character. I was applied to, to take the duty in his absence, and the names of other practitioners on the list were of decided eminence. It so happened, however, that, though frequently sent for, I was always absent when the officers called, so that I never attended professionally, but I received from the principal officer the most special and particular directions as to what was required, and he directed my attention to many details which I should not otherwise have considered important. I know how the duty was performed here, and that the examination was sufficiently minute in every respect. Of the anxiety to do the duty well and efficiently, no better proof can be adduced than the circumstance of a steam-boat being generally at the orders of the officers. The severity of the law has more than once excited complaint, but how far quarantine of any kind can check the disease or prevent it from spreading, is a totally different question.

Sunderland, Feb. 21, 1832.

SHORT EXTRACTS FROM RECENT PUBLICATIONS ON CHOLERA.

(From Ainsworth on Cholera as it appeared at Sunderland.)

Bloodletting.—In the generality of cases bloodletting is efficacious in proportion to the earliness of its employment. In the first stage, while the pulse is yet full and the temperature not reduced, it may be plentiful, as there are chances of its cutting short the disease. The pulse may be the guide here; if oppressed, bleed till it becomes softer and fuller. But when the collapse has come on, the quantity drawn should be small. Eight ounces in an adult will be sufficient to allow the remainder to circulate more freely, and relieve the heart, and at

the same time will not exhaust the patient too much. The absence of the pulse is no prohibition of the use of the lancet. In all cases care should be taken to avoid syncope; and in persons addicted to spirituous liquors, bleeding, though beneficial, often increases the violence of the spasms in the muscles of the extremities and the trunk. Dr. Keir, whose opinions (if I may judge from the able report published in "*Papers relative to Cholera Spasmodica*") are deserving of the greatest attention, mentions that bloodletting, when the vital energies are very much depressed, has proved hurtful. There can be no doubt but that in cases of extreme old age and debility the amount of blood drawn must be very small to do it with safety; and it is probable, that in cases where a sudden and very large evacuation is the first symptom of the disease, immediate bleeding will be pernicious. But with these exceptions, the experience obtained from what I saw of the general practice at Sunderland, combined with the light which pathology has thrown upon the nature of the disease, lead me to think that bloodletting should always be had recourse to in the first and second periods of the disease, while there is any chance of obtaining blood.

Mode of applying Heat.—Measures must at the same time be taken to restore animal heat; and the simplest methods are the most efficacious. Much has been said about hot bags of sand, &c., and frictions with various hot stimulating substances. The bags are thrown aside by the cramps, or found quite ineffectual; the bottles of hot water are every now and then on the assistants' feet; and the preparation of frictions withdraws attention from more important symptoms. Latterly, in the hospital, the whole plan consisted in placing the patient before a large fire, and momentarily renewing very hot blankets. This is the best mode of proceeding, and the patient can then be watched carefully, and without distraction. Hot-air baths, or vapour baths, may be of use at this juncture; they certainly are of none in the period of collapse.

Alkalies in Cholera.—The carbonates of soda and ammonia have been found of the greatest utility in this period of the disease. Mr. Goss says* he was sent for, in India, to an old native woman who was labouring under all the symptoms of the second stage of the disease. He administered about a drachm of the soda with ten or fifteen grains of the carbonate of ammonia: the stomach ejected three or four doses, which were repeated as often as the organ became tranquil enough to admit of its being swallowed; ultimately one dose was retained, and half the quantity was given every hour, until all bad

* Practical Remarks on the Disease called Cholera.

symptoms had disappeared; and the patient recovered without the aid of any other medicine, excepting a dose or two of calomel and jalap. Mr. Goss was taken ill himself; and during his confinement four or five (why so indefinite?) cases were treated under his instructions by a native assistant, all of which terminated favourably.

Without any knowledge of these facts, Mr. Torbock was led to make trial of the carbonate of soda at Sunderland, and met with the most successful results. One case which I attended with him, the patient, a boy of about eight years of age, had scruple doses administered to him every half hour, and at the lapse of that period exhibited the greatest anxiety lest they should be omitted. The action reminded me of the effects of the carbonates of soda and ammonia on the bite of a snake or the sting of a wasp.

GENERAL PLAN OF TREATMENT.

1st Period.—*Oppression.*

Diarrhœa. Calomel and opium; chalk mixture.

Prostration. Bleeding; salt or mustard emetics.

Evacuations. Bolus of calomel; carbonates of soda or ammonia.

Cold surface. Heat; frictions with hot dry blankets; vapour or hot-air bath.

Loss of colour. Warm restoratives.

Febrile action. Mild aperients; diaphoretics.

2d Period.—*Collapse.*

Epigastric pains. Mustard poultices or liquid epispastics.

Evacuations. Warm enemata; turpentine injections.

Cramps. Sinapisms; frictions; sedatives. Absence of secretions. Aromatic spirits; volatile essential oils; calomel.

Cold breath. Oxygen; nitrous oxide.

Cold affusions. Loss of sensation; heat, pulse, and voice, deficient.

3d Period.—*Febrile Action.*

Mild. Tepid baths; gentle aperients.

Severe. Local depletion; blistering; purgatives and diaphoretics.

(From Dr. Lawrie's *Essay on Cholera in India, Sunderland, and Newcastle.*)

Fear does not predispose to Cholera.—Does fear predispose to this disease? I do not believe that fear ever gave a man cholera, or ever will. To the production of cholera, a regular chain of causes and effects is as necessary as to poisoning by prussic acid. The inhabitants of Gateshead fell asleep on the 25th December, in perfect security and devoid of panic, but before the sun rose on the 26th, fifty-five individuals had been seized, thirty-two of whom were destined not to see it set. For several days subse-

quent to the 27th, the panic of the inhabitants was greater than I have ever witnessed under any pestilence, while the new cases decreased, and on the 30th were as low as twenty.

Efficacy of Cholera Hospitals.—A doubt has been cast on the efficacy of hospitals in relieving cholera. My own opinion is, that in no disease are they more useful. A cholera patient requires to be treated with a degree of care and assiduity, and demands external agents and comforts, which the poorer classes cannot command in their own houses. Far be it from me to hint that the poor do not receive every attention from official medical men which the nature of their circumstances permits. I am convinced they do—but I am certain, that if cholera invades the larger towns of Scotland, it will be impossible for medical men to undergo the fatigue of attending the poor in their own houses; and, moreover, that they will experience insuperable difficulties in procuring the necessary remedial agents. In an hospital, on the other hand, medical visits at stated intervals during the day and night, together with careful students acting as house surgeons, will effect more, with one-twentieth amount of labour, than the most active can possibly accomplish in the houses of the poor.

In Sunderland I found one excellent hospital, in Newcastle three, and in Gateshead one was got up during my stay there. They were found very useful, but much less so than they will be in such large towns as Edinburgh and Glasgow, in which the people are in the habit of going readily into hospitals.

State of the Respiration in Cholera.—1st. Of mechanical respiration. I ascertained, when in Newcastle, that this, in almost every instance, is unimpaired, and in very many augmented. The stethoscope proved that at every inspiration the lungs are fully dilated, and that a larger than ordinary quantity of air entered them. In only one instance (Jean Crawford) I found the mechanical respiration interrupted, probably from spasm of the diaphragm; and it is remarkable that the girl died suddenly of spasm an hour afterwards. In very few could I discover effusion into any of the parts contained within the chest. In almost all, the breathing continues perfectly easy until death; in many, there are none of those convulsive sobbings which precede death from other diseases, making it very difficult to say, in cholera, at what time the patient has ceased to live; so calmly and gently are his previously severe sufferings terminated. From all this I infer, that the nerves supplying the external muscles of respiration, the phrenic and the recurrents, are unimpaired in function. In corroboration of this, we know that cramps are much more violent in those muscles supplied with nerves from the

lower portion of the spinal marrow, as the abdominal and muscles of the lower extremities, than those of the chest and arms. The external respiratory nerve, and the phrenic, are given off in the neck.

2d. Functional respiration. If we cut across the eighth pair of nerves below the point where the recurrens are given off, mechanical respiration continues for many hours, and the breathing becomes difficult before death, in consequence of effusion into the bronchi and substance of the lungs. In cholera, the blood being rapidly deprived of a large portion of its fluid ingredients, and little blood being in circulation through the lungs, effusion cannot occur, and the patient dies without dyspnoea. If mechanical respiration can go on while the influence of the brain on the pulmonary plexus is cut off, is not the probability strong, that to these plexus belong the functions of the lungs, properly so called? That the functions of the lungs are suspended, is proved by the following facts:—1st. The deadly coldness of the surface, of the air issuing from the lungs, and of the tongue and palate. I need hardly state my adherence to the physiological doctrine, that animal heat depends on respiration. Those who doubt it, would not be convinced by any arguments I can adduce. I may say, however, that cholera affords an additional argument in support of its independence of the brain. The functions of the brain are less impaired than those of any other organ; and yet, in cholera, animal heat ceases to be formed. 2d. Less carbonic acid is thrown off in expiration, and the blood is not decarbonized. These are the unquestioned functions of the pulmonary ganglia.

Bleeding in Cholera.—I am happy to have it in my power to state, that, from all I have seen of cholera in Musselburgh, Tranent, and Haddington, and from what I have learned from the medical gentlemen resident in these towns, bleeding is pre-eminently useful in this disease. From the practice pursued in these places, one point I consider settled—viz. the utility of bleeding in the premonitory stage, more especially on its first appearance. How far, or if at all, it ought to be put in practice in the other stages of the malady, I am at a loss to state.

Proportion of those attacked to Population.—1st. The numbers attacked, in proportion to the population of the places infected, are more than five times greater in Sunderland and Gateshead than in the Russian dominions; and the deaths to the population more than three times. The seizures are nearly one-half less than among our troops in India.

2d. The proportion of deaths to seizures is $1\frac{1}{2}$ higher than among our Indian troops,

and considerably lower than among the Russian peasantry.

3d. When we consider that this disease is limited to the inhabitants of certain localities, its effects on families must be fearful. It is no uncommon occurrence to meet with two, six, or even eight of the same family stricken with it, within a few hours. The picture is aggravated when we recollect that the disease selects its victims from that class of the community who are least able to sustain any addition to their misery.

4th. The statement that cholera does not increase the bills of mortality, is not correct. The average number of deaths in Gateshead was nearly eighteen monthly. In that parish the deaths from cholera alone, in nineteen days, were $7\frac{1}{2}$ times that amount.

MEDICAL GAZETTE.

Saturday, February 25, 1832.

“*Licet omnibus, licet etiam mihi, dignitatem Art. Medice tueri; potestas inodo veniendi in publicum sit, dicendi periculum non recuso.*”—CICERO.

CHOLERA OR NO CHOLERA ?

WHEN we last addressed our readers, the metropolis was in a state of panic; but, like most other instances where exaggerated apprehensions get possession of the mind, the excitement has been followed by a quiescent confidence which bears as little relation to a sound and healthy state, as the commotion to which it has succeeded. We, who stand detached and personally free—pledged to nothing but the promulgation of the truth, can easily perceive two parties busily at work—the cholera and anti-cholera; nor are whig and tory, terms designating more opposite opinions, or interests maintained with more uncompromising rancour than the medical warfare regarding the present sanitary state of the metropolis.

Now the first impression which has been forced upon our minds, is, that there exists a great want of candour on both sides. The object with the conductors of the daily press especially, is

not in any degree to ascertain whether the disease called cholera be actually present in London, but simply and unequivocally to make out that it is not; and, in their zeal to accomplish this, they not only forget all that they themselves so recently said, with regard to the attempts at concealment practised in Sunderland, but at once embrace opinions respecting the nature of the disease directly the reverse of those which stand recorded in their own pages. The blind anxiety to make out a particular case without reference to the facts, leads them to allude only to those circumstances and those doctrines which tend to give plausibility to their own side of the question. This is the true explanation of our seeing individuals, without either experience or reputation, adduced as high authorities; and thus the *imposing* epithets of "eminent physician" and "distinguished practitioner," are prefixed to names of the most subordinate grade in the profession, and applied to persons on whom such announcements, in the eyes of their brethren, constitute the bitterest satire. So we have "striking" articles on cholera copied into their pages by journalists wholly incapable of perceiving that the only *striking* points in the quotations are their utter lack of common honesty and common sense. These observations naturally lead us to speak of another set, or rather another mode of acquiring notoriety adopted by the same set of candidates for newspaper reputation: we mean the band of spokesmen who on Saturday enlighten the world at the Westminster Medical Society—due care being taken that their very important and authoritative opinions shall appear in the Herald or the Post of Monday; on the evening of which day they fail not to transport themselves to Bolt-Court, in the speech-stirring hope that, by a little variety in the diction, the

same orations may again find a place in the columns of the Times or Chronicle of Tuesday. There is, in fact, a knot of them, whose names are as familiar to the ears of admiring gossips as the cholera itself—or any other sort of plague. Now these are just the kind of persons who suit the newspapers, for they are always ready when a reference is wanted, constantly perking themselves up as oracles on all occasions, in season or out of season; being, as Grattan happily said, "little men, but out in all weathers."

If our non-medical cotemporaries had simply confined themselves to a temperate expression of their incredulity, and their want of satisfaction with the proofs alleged for the existence among us of the same malignant disease which has committed such ravages abroad, we should have admitted the reasonableness of their doubts, and acquiesced in the propriety of instituting a rigid inquiry, conducted by men who had no interest in the result, and who, familiar with the diseases which infest the poor of this metropolis, were able to say what was and what was not of foreign origin—or at least of unwonted aspect. But when we see the leading journals of the day, however violent their political animosities, joining here as in a common cause, and, in the most intemperate manner, raising the cry of "no cholera!" we naturally call to mind the conduct of the Hungarian cerfs and boors of Russia, and almost expect to see their example followed up, and "down with the doctors" added to the general shout. How unjust, how unreasonable, how impolitic is all this! What a spectacle do we present to the rest of the world, and more particularly to the intelligent portion of our continental neighbours! When the disease reached Sunderland, and afterwards Newcastle, was it not the universal exclamation of those very

parties who now are loudest in their denial of any cholera existing in Britain, that they who opposed themselves to the conviction of the unwelcome visitor having arrived, were acting under a selfish, mistaken, and dangerous policy? and were not the opponents of the truth in every instance ultimately compelled to acquiesce in the conclusion which reason pointed out? Is it possible, then, that after all this, with nearly four months of experience allowed us,—to say nothing of what was to be learnt from contemplating the history of the disease as it pursued its slow, but steady progress from the Sunderbunds to the shores of England;—after all this, we ask, is it possible that the same scene of useless controversy is to be renewed, as to the identity of a malady which hundreds of intelligent witnesses have declared to be as much the same as the phenomena of disease ever are when displayed in different races of men and in different regions of the world? and all this for no other reasons than that the complaint is not so unlike what has been seen before among us as some had led themselves to expect, or, forsooth, because it has not already committed greater havoc in the metropolis. If the denial of its existence had any tendency to bring about the accomplishment of the wished-for end, we should be the last to raise our voices against the delusion; but the very reverse is the case. Whether cholera be infectious or not, experience has shown that it is to be most efficiently checked by removing the sick from situations in which they contracted the disease, and also, if possible, removing from their abodes of filthiness and misery the inhabitants of those localities in which the malady first displays itself; or, when this cannot be done (as must too often be the case) in lessening their liability to become affected, by improving their condition. To accomplish such extensive objects, considerable sacrifices must be made by

the wealthier part of the community. But the view taken by the public press has been of the most pernicious consequence in this respect, retarding or wholly arresting the preparations reluctantly begun, and willingly abandoned; so that if the “no cholera” cry should prove fallacious, the public will have to thank those who raised it, for infinitely more serious mischief than has resulted from the unskilful proceedings of the Board of Health.

A very short time must decide to the conviction of all, whether the cholera be or be not in London, and till then let us act as if the question were already beyond all doubt: the poor will be the gainers whichever party proves to be correct, and the evil that trade has sustained by the announcement of its presence, can scarcely be aggravated by so short a delay as will be requisite. This is undoubtedly a wiser and honester proceeding than to deny the existence of that to-day which to-morrow may be too apparent to be concealed:—at Orenburg, where it ultimately prevailed so extensively, only two cases occurred during the first fortnight!

Meantime, let us, as faithful chroniclers of passing events, consider how the facts at present stand. If there be any truth in history or in medicine, cholera has proceeded, step by step, from the banks of the Ganges to the shores of the Baltic; and the manner of its progression has been such as in our minds to prove, that it is carried by human intercourse. At one of the ports of England opposite to and trading with those places, where the cholera then prevailed, a malignant disease, some months ago, made its appearance. Many competent persons who had seen the cholera in India and in Russia, have also seen the cases in the north of England, and have pronounced them to be the same. Several persons—to the amount of ten or twelve—have also seen and examined the patients said to be

affected with cholera in London, and they have deliberately, but positively, stated the disease still to be the same. We have seen and conversed with some of these, and if they be wrong, then is there no value in human testimony. It is a great mistake to say, as the daily press has done, that these gentlemen receive extravagant remuneration, and thus have a direct interest in keeping up the deception; at least in the instances to which we allude, no such temptation existed; and even with regard to the "Inspectors," who act under the Central Board, we may remark that they are taken from military surgeons and assistant surgeons on half pay, some of whom are dragged unwillingly from the repose of private life to laborious and often invidious duties, for which they receive only the difference between their retired and full allowance, which in none of the ranks exceeds a guinea, and in the latter amounts only to a few shillings a-day. Several, we know, would give double the sum to be freed from the "appointment," and we doubt whether in any, the *auri sacra fames* be so strong as to make them see their patients *blue*, when other people would discover in them only their natural complexion. With regard, however, to the Central Board, it must be acknowledged that the injudicious violence of the press has stirred up probably one of the strongest feelings of the human mind in favour of the existence of the disease; for it now touches the honour, not less than the interest, of the members of that Board to search out and bring to light every single case that can possibly bear the name of cholera. Their interest and their duty are most clearly set at variance; and we can only say, that if they now really wish the malady to cease, they must be the most upright and disinterested of men.

Supposing the disease in all the countries where it has appeared to be essentially the same, we have ex-

pressed our belief that it has been conveyed by transmission from man to man—but this idea by no means carries with it the assumption that the mode or facility of its transmission is analogous or equal to that evinced by small-pox and certain other diseases. Indeed, to us there appears to be something peculiar, and hitherto unexplained, in the propagation of cholera, and hence the plausibility which may be given to either side, contagion or non-contagion, by those who take a limited or partial view of the subject. Accordingly, the number of well-authenticated cases in which individuals have been exposed with impunity to whatever influence cholera in others is capable of producing, is so great, as scarcely to leave any parallelism between this and the avowedly contagious diseases; so that while the history of its progress leads us to believe that it is capable of being communicated by human intercourse, yet the proportion constantly exposed without suffering induces us to regard the risk incurred by individuals not *residing in the locality where the disease has originated*, as extremely limited;—such as ought not to produce any apprehension on the mind of the medical practitioner, and such as to render perfectly illusory and absurd the panic existing among many in the better ranks of life, who, we believe, run little more hazard from the existence of cholera in London than from that of the plague in Constantinople.

We believe cholera to be a fever, in the cold stage of which, the patient often dies—that it is the same which has been described by several of the older writers, and that this particular visitation has reached us over-land from Bengal. But the most important points after all are not, whether cholera be a new malady, or an old one—whether it be of Asiatic origin or of English growth: but this is the vital question—does there, or does there not, exist in London a disease (by what-

ever name it may be called) which proves fatal to a large proportion of those affected by it in less than twenty-four hours, and has it, or has is it not, been customary to meet with such cases in this metropolis?

If there really be no more sickness, and no greater mortality among the lower orders than usual, then, indeed, is the alarm which has been sounded unjustifiable, and then may the ordinary provisions for the poor be sufficient; but if (as we believe,) a malignant fever, called cholera—the same as has proved so fatal in other great cities of Europe and Asia, be really among us, then it is imperative that extraordinary exertions should be made to obviate the evils of an extraordinary emergency. It is not for us to dive into the future, and we can at best only reckon upon probabilities, but this we will say, that if the disease do not spread in London, so as to put to shame the intemperate cry of “no cholera,” then will this town constitute a solitary exception to the many hundred places which it has visited; and if it do so spread, what language will be strong enough to denounce the selfish, the base, the stultified opposition of certain members of the press to the preparatory measures already too long delayed, and which, if the Central Board had been less fearful of incurring unpopularity, and more energetic in representing to the government what was requisite, would have been long ago completed without precipitation, without clamour, and without panic. As it is, we have said before, and we repeat, that the public is not satisfied, will not be satisfied, and ought not to be satisfied with a Board of Health for the direction of the empire at which only *three* professional men have seats, while not one of those three is a medical practitioner, or can by possibility be familiar with the diseases of the metropolis*.

CASES OF CHOLERA.

WE are anxious to impress upon our correspondents that little interest can now attach either to the mere opinions of persons who have not actually seen the cholera, or even to individual cases, unless they illustrate some important point. On this account we have declined to insert several which have been sent to us. General results, and inferences as to the effects of remedies, we shall be thankful to receive—such, for instance, as are contained in the valuable paper from St. Petersburg, and in the shorter communication from Sunderland, to be found at pp. 792 and 795. We shall also continue to extract the most important facts from each work as it appears, prefixing appropriate headings, as in the last and present numbers.

Frank Burns, whose case was given last week, is now quite well.

WHAT DO THE LAWYERS THINK OF CHOLERA ?

It is not impossible but that the question may be mooted ere long, in some of our Courts of Law, whether an action for imputing the Cholera to a man would lie. This, as Mr. Amos recently observed, in a lecture on Medical Jurisprudence, might lead to a result perhaps in some measure desirable; for, notwithstanding the apparently widely-differing opinions of the faculty with regard to the contagious or non-contagious nature of the disease, it is probable that our legal functionaries would not lose much time in coming to a *judicial* determination on the subject. We believe that, in general, it is actionable to impute a *contagious* disease; and on this ground several curious decisions have been obtained by the gentlemen of the bar. “Thou hast the falling sickness” was found to be *not* actionable in the 4th year of the reign of James the First—whence we may infer that epilepsy was not, even at that pe-

* For latest return of cases of cholera in London, see page 808.

riod, deemed contagious; though, at a subsequent date, the expression was accounted actionable, when applied to an *attorney*, because it tended to slander and disable him in his calling. Leprosy, not so much, perhaps, on account of its supposed contagiousness, as of its odious nature—rendering it expedient that the infected individual should be removed from the public eye—has frequently been the subject of legal inquiry. “Thou art a leper,” is highly actionable, for it charges a man with thrusting himself into society where he ought not to be, and intimates that the ancient and severe writ *De leproso amovendo* ought to be executed upon him. The question concerning plague, small-pox, and other notoriously contagious disorders, may be looked upon as settled long since.

EXTRACTS FROM JOURNALS, *Foreign and Domestic.*

EXTERNAL APPLICATION OF CROTON OIL.

M. ANDRAL has recently published an account of some experiments which he has been making with the croton oil*. In more than thirty cases he had it rubbed upon the skin of the abdomen, thighs, or arm-pits, in quantities varying from four to twenty drops; sometimes pure, sometimes mixed with almond oil. These frictions were made under different circumstances, and in some instances certainly under conditions favourable to absorption; yet, to his astonishment, no appreciable result referrible to such effect was met with, except in one instance. On the other hand, in all the patients an eruption appeared on the parts where the friction had been employed, differing in degree according to the quantity of oil, and the part rubbed, and the sensibility of the individuals. The following were the general phenomena:—A few hours after the friction (supposing this to have been employed for some minutes) the patients

experienced a smart tingling in the part. In some this sensation was transient, in others prolonged, but never to such an extent as to produce troublesome nervous disturbance; at the same time there appeared a great number of little red points, at the apices of which there soon were perceptible little pustules, which within from thirty to fifty hours had become fully developed. Many of these became united, and then formed large patches, filled with white opaque fluid. The eruption increased for three or four days, after which it remained stationary, and subsequently died away like small-pox. In some cases where the oil was applied to the face, the eruption was more copious and intense than elsewhere.

Thinking that this form of counter-irritation might prove serviceable in cases which had resisted other means, M. Andral had recourse to it: in many of these it also failed, but in others the improvement was so rapid, as to shew the friction to be a therapeutic agent of considerable value. Among the successful cases are enumerated old rheumatic pains, obstinate sciatica, numbness of one side of the face, and chronic inflammation of the alimentary canal.

BILIARY FISTULA.

At a late meeting of the Academy of Sciences in Paris, M. Civiale read an account of a case of biliary fistula, exhibited by a patient now in the Hôpital Necker. The subject is an officer, who, in the campaign of 1812, received a thrust with a lance in the abdomen, and was left for dead on the field of battle. The wound had penetrated the cavity on the right side, about two inches from the umbilicus. There resulted from this a ventral hernia, which was restrained by means of a circular bandage round the body. Since that time the patient has lived in an alternate state of health and suffering till the autumn of 1831, when a urinary calculus having made its appearance, he came to Paris, to be treated at the hospital above mentioned. The digestive organs were found to be much impaired; and the liver was so much enlarged as to extend to the umbilicus. It soon became apparent that this viscus contained purulent matter; and the abscess was subsequently opened with all due care. The fluid evacuated did not present the

* Gazette Medicale.

usual characters of pus coming from the liver. When the suppuration had nearly disappeared, a greenish fluid escaped from the opening; and at length one day, after a violent fit of colic, a considerable quantity was evacuated, with regard to which there could be no mistake in taking it for bile. From this time a similar discharge has occurred at each dressing. Loss of appetite, and sleep, and great emaciation, were the immediate consequences of this evacuation of bile. Three days after the stools became white, but there was neither constipation nor looseness. The digestion, however, became gradually restored, but the skin has not yet regained its natural appearance. About a wine glass full of bile is evacuated in the course of twenty-four hours; the quantity, however, during the two days preceding the date of the narrative had become diminished. The operation of lithotripsy was performed notwithstanding the above complication, and apparently with complete success.—*Gazette Medicale*.

GEOGRAPHICAL RELATIONS OF YELLOW FEVER.

YELLOW fever shews a most decided predilection for the northern hemisphere. The equator seems to present it with an insurmountable bar. Yet at Rio Janeiro, in Brazil, at places in Chili and Peru, and in certain localities on the coasts of Africa and Asia, all the conditions of atmosphere and soil most favourable to the disease would seem to exist in a state of concentration.

It cannot be on account of the greater heat of North America that the complaint is so peculiarly attached to that quarter, for Brazil and Peru are of a temperature infinitely higher than is to be found in the United States, nor are they destitute of changes or of extreme humidity. Guayaquil and Panama, which are situated but a few degrees north of the line on the west coast of America, are ravaged with yellow fever, while Lima, fourteen degrees south, as Martinique is fourteen degrees north, is very healthy, though far from being very cleanly.

What have the winds to do with the question? Their direction and intensity seem to have much concernment in it, yet it is difficult to state any thing with precision on this point. We can

only say, that the prevalent winds on the west coast of South America, or the south-westerly, come from the ocean, as at the Antilles, when the trade-winds similarly come from the main. Has the elevation of the country any thing to do with it? It would seem not, for Avica, in Peru, is not only situated on the flat low sea-coast, but is exposed to a breeze which blows perpetually off an island covered thickly with the dung of sea-birds, diffusing a stench which infects the air around the whole country to a great extent. Avica, however, is entirely free from the desolating malady. Quilia, another town of Peru, is also on the coast, and near a river: it is an extremely hot situation, yet the yellow fever never shews itself there. At Lima, the unseemly nastiness of the streets is remarkable: dead dogs, asses, and mules, are left there to putrify, without removal, even after the birds of prey have devoured every thing but the bones. Nor is the disposal of the human body better cared for in that region; the graves in the immediate vicinity of the city are quite exposed, and uncovered with earth. The humidity of the atmosphere, too, is extreme and invariable: the sky is perpetually charged with clouds, and the sun-beams can never reach the soil: in short, neither are there here any occasional brisk gales to dissipate the malaria which is constantly accumulating. Yet, with all this, yellow fever never makes its appearance at Lima; nor do we find the inhabitants particularly disposed even to gastric affections, bilious complaints, or any of the disturbances which seem to have relationship to the disorder.

This preference of yellow fever for the northern hemisphere is still further countenanced by what occurs in the old world—we allude to the frequent epidemics of Leghorn, Barcelona, the coast of Africa, and the almost annual affliction of Cadiz.—*From the Notes of M. Sper, R.N. of France*.

SNUFF CHEWING.

WE have seen wretched creatures victims to this habit, who, in their haggard countenances and blood-shot eyes, are little better, on the scale of suffering, than the opium eaters described by Mr. Madden. All that we have said of the noxious effects of the use of tobacco, will apply to snuff chewing. When reduced to powder, this poison is more

readily dissolved and mixed with the saliva, and more intimately applied to the surface of the mouth and tongue. Of course it is more readily absorbed in this state, and is more injurious to the sense of taste and the nervous system in general. Tobacco, applied to a limb or any part of an animal denuded of its skin, at first irritates and then paralyzes it. Next to this, as a means of making it operate on the living body, will be to put it on a moist surface, such as that of the tongue, mouth, throat, and nostrils. By this process, as in chewing, smoking, and snuffing, the animal economy is subjected to the morbid influence of this poison. No person escapes it who uses tobacco in any form, mask the thing as he will. The poison, he may perhaps allege, is a slow one; but poison it is, and he will suffer from it sooner or later. His complexion will suffer, so will his digestion, and his breathing, and strength of body. His senses will be less acute—his mind more fickle and less able to sustain itself for any length of time under energetic efforts. He may boast of the aid which he has derived from segars or snuff, while sitting up at night in his study or counting room,—but what will he say of his feelings in the morning? Has not the excitement of the evening been dearly purchased by the languor of the following day?—*Philadelphia Journal of Health.*

ROYAL INSTITUTION.

Friday, Feb. 10, 1832.

Voltaic Agencies; Laws of the Simple Battery applied to the Compound one.—Auzoux's Anatomical Model.

DR. RITCHIE gave an ample account of his views of Voltaic action. He paid high tribute to the researches of Sir Humphry Davy and Mr. Faraday on the subject; and dwelt on the immense importance of Volta's discoveries in revealing the mysteries of nature. Before he proceeded to investigate the laws of the compound battery, he took occasion at some length to explain to his audience the principles upon which his torsion galvanometer is constructed—an instrument by which, through the elasticity of fine glass threads, some of the most delicate motions in nature can be appreciated. It was frequently employed in the course of the even-

ing to illustrate certain positions laid down by the professor. The ordinary explanation of the appearances presented by the action of the simple Voltaic battery is founded on the assumed *decomposition* which takes place, in consequence of the greater affinity which one of the metals manifests for oxygen than the other; but Dr. Ritchie maintains, that the phenomena are wholly owing to a *change of place*, which is effected among the molecules of the fluid employed. This he endeavoured to render conceivable to all present, by an ingenious though homely contrivance of a plane representing the plate of metal used in the battery, and a row of white and black spherules on either side of it, standing for the elementary molecules of the fluid in which the metal was supposed to be immersed. Volta's theory of the contact of dissimilar metals he also undertook to disprove as erroneous, and to shew that the phenomena were consequent upon the separation of the metals, not their contact: nor need the metals be dissimilar, as Dr. Ritchie shewed, for he produced considerable action by the employment of two copper spirals, one fitting into the other; and also by using two disks of the same metal, one hot and the other cold, with the addition of a little weak acid. The cold disk, we may observe, exhibited indications of being (so to speak) positive, the hot of being negative; and Dr. Ritchie took occasion to point out the advantage of using hot water in place of cold, in preparing the battery; it increases its energy in the ratio of no less than two to five. He denied the propriety of applying the terms positive and negative to the indications of Voltaic agency; and he went farther; he disputed the inference from Dr. Wollaston's celebrated experiment, and shewed that it was defective as a proof of the identity of voltaism and common electricity. In conclusion, the learned professor proved, by the composition of ratios, various laws of the compound battery; in particular, he demonstrated that the powers of those batteries varied directly as the square roots of their number of plates. This, as well as his other conclusions, he verified by experiment, the results being registered by his torsion galvanometer. Of two batteries which were used, one of 120, and the other of 50 plates, it was shewn that the energies were not in a higher ratio than as two to one.

After Dr. Ritchie had finished his valuable lecture, M. Auzoux's anatomical model was introduced into the theatre, and shewn up by Mr. Costello. It excited considerable interest among the spectators; but they were tired with a superfluous and lengthy exordium from the demonstrator, and greatly puzzled by a figure of speech—something about *Frankenstein*—with which he concluded.

In the library the great attraction of the evening was Mr. Cheverton's specimens of miniature busts in ivory, copied from casts and marble, by a mechanical process, of which the inventor has not yet given any description to the public. We also saw an excellent specimen of English beet-root sugar.

WESTMINSTER MEDICAL SOCIETY.

Saturday, February 11, 1832.

G. JEWEL, ESQ., IN THE CHAIR.

No paper was read. Mr. Hunt alluded to the combination of quina with alkalies, as medicinally advantageous, though it might appear to be chemically incongruous. He recommended it on the authority of Dr. Prout, Dr. James Johnson, and he had frequently used it.

Mr. Burnett made some remarks on the different species of cinchona.

Dr. Gregory related a case of spontaneous gangrene of the left foot in a young woman: the patient died. Disease was found in the mitral valves, but the vessels of the limb were not examined. The account of the dissection was regarded by several speakers who followed as incomplete. Some very unprofitable conversation followed, for which we cannot afford space.

February 18th.

—CHINNOCK, ESQ. IN THE CHAIR.

A desultory and ill-conducted discussion took place on the subject of cholera, which, as it has already appeared in the newspapers, we deem it unnecessary to insert. We can safely assure our readers that they sustain no very important loss by the omission. The next meeting is to be held in Great Windmill-Street, where there is to be another cholera—(quere, choleric?)—discussion.

REPORTS OF CASES OCCURRING AT PUBLIC INSTITUTIONS.

LONDON HOSPITAL.

Fracture of the Spine.

BENJAMIN KING, aged 32, a strong muscular man, was brought into the hospital December 5th, about three o'clock in the afternoon. He had fallen, while descending a ladder, a

distance of about fifteen feet, upon his back, across a puncheon of brandy. He complained of great pain in the situation of the dorsal vertebrae, near their middle, and the spinous process of the seventh vertebra was found projecting considerably beyond the sixth. The abdominal muscles and lower extremities were completely paralysed; respiration was more laboured than natural; and there was priapism. In order to replace the bones of the spine, by Mr. Luke's direction he was placed upon a bed, with a pillow under his loins; his feet were fastened by bandages to the end of the bed, and a jack-towel was carried over the chest, and then under each axilla to his back, by means of which extension was made by four assistants, while the loins were raised by another towel placed under them, and pressure was made upon the sternum. In between three or four minutes a crepitus was felt, and immediately the deformity disappeared, the ridge of the spinous processes becoming continuous. The man immediately expressed himself relieved from pain; sensation returned in some degree in the abdomen, and the priapism became less. The pillow was still continued under his loins.

Vespere, V. S. ad 3xxx; Cal. gr. ij. c.
Op. gr. ʒ, nocte maneque.

Dec. 6th.—Complains of pain in the back and abdomen; the lower extremities remain paralysed; there is still partial priapism; and it is necessary to draw off his urine by the catheter. Pulse quick and full.

Iirud. xl. to the seat of the fracture.

7th.—Pain relieved by the leeches; the abdomen tumid; his bowels have not been relieved.

Rep. Iirud. xx.; Ol. Ricini, ʒj.; Contin. Pil.

8th.—Pulse not so quick, and weaker; faeces pass involuntarily.

Rep. Iirud. xx.; beef-tea.

10th.—Pain in the back increased. The urine, which was at first secreted in small quantities, has become clouded with mucus, has a strong ammoniacal smell, and requires to be drawn off frequently during the day.

11th.—He is feverish, and erysipelas has attacked the back and left side; the paralysis has not diminished, and the patient is much weaker.

Ordered poultice to the part; Omit. Cal.; Contin. Op.

12th.—Erysipelas increased; great uneasiness in the region of the bladder; urine more turbid. From this time the man continued to sink, and died on the 18th, the thirteenth day after the accident.

Post-mortem Examination.—Upon opening the thorax there was found extensive inflammation of the pleura, particularly on the left side, with effusion of serum; and lymph of a flocculent nature was observed upon the pleura at various parts. An oblique fracture was observed traversing the sixth and seventh dorsal vertebrae, without displacement; some extravasated blood was found in its vicinity. On removing the spinous processes, a portion of coagulated blood, too small to occasion pressure on the medulla spinalis, was seen adhering to the dura mater. The medulla spinalis was softened near the seat of the fracture. On making a longitudinal section of it, a drop or two of pus escaped from its centre; and at this part its whole texture seemed disorganized. The bladder was considerably thickened, and its mucous surface was coated with lymph.

Amputation at the Shoulder-Joint.

John Martin, aged 53, a stone-sawyer, was brought into the hospital September 16th, at half-past seven in the evening. His arm had been crushed by the shaft of a cart being accidentally forced through it. There was a compound fracture about the centre of the humerus, with great contusion of the soft parts, and a wound three inches in extent on the outside of the arm. Upon introducing the finger into it, the soft structures were found to be completely detached for some extent around the fractured edges of the bone. The biceps muscle was nearly torn through, and the sheath of the brachial artery was exposed. There was also a wound on the back part of the arm, and a considerable laceration of the triceps muscle. There was a slight bleeding from the wound, and he suffered no pain. The arm was recommended to be removed at the joint, but no persuasion could induce him to submit to an operation. The arm was put into splints, and he had forty drops of laudanum, which were repeated in the night.

17th.—He had dozed a little during the night; the arm was in the same state as the preceding evening, being without pain or tension. He was again recommended to submit to an amputation, and after some difficulty he consented at half-past three in the afternoon, when he was carried into the operating theatre.

Mr. Scott performed the operation with a very large scalpel. Pressure being made upon the subclavian artery with the thumb, he commenced the incision at the front of the joint, making a flap of the deltoid muscle; and after dividing the capsular ligament, the head of the bone was dislocated; the brachial vessels were next divided, and then the soft parts on the inside. Twelve vessels were secured, and about eight ounces of blood were lost, in the operation. The parts were kept in approximation by three sutures

and several layers of adhesive plaster. After the operation he had a good pulse.

Ordered Tinct. Opii, 3j. statim; Liq. Calcei c. Spirit. to be kept applied over the part.

18th.—Had slept a little during the night; tongue brown and furred; pulse rather quick and full; skin hot. No tension or pain about the shoulder.

Mist. Salin. Cathart. tertiis horis donis alvus responderit.

21st.—The stump was dressed, and looked healthy; it was adherent at the upper part. There was a small slough on the back part, from pressure in lying.

Decoct. Cinchon. ter die; porter, one pint daily.

Oct. 3d.—All the ligatures had separated. Porter, two pints.

From this time the stump continued healing, except at the back part of the scapula, where a sinus formed, from which there was a constant discharge; a counter opening was made, but it did not close, and the rest having healed, in the expectation that he would be better out of the hospital, he was discharged December 8th.

Aortal Aneurism.

Nov. 24th, 1831.—A. B. had been admitted into the hospital about six weeks ago, having fallen down a ship's hold, by which his right thigh was fractured, and he sustained considerable injury to his chest. His thigh had become pretty firmly consolidated, and in other respects he appeared to be doing perfectly well, when suddenly, last evening, he complained of pain in the stomach, and was in the act of taking a pinch of snuff, when he dropped down and died instantly.

Secutio Cadaveris.—On dissecting back the integuments of the chest, a fracture was observed of three of the ribs on the left side, near their sternal extremities; two of the cartilages also were broken, and both they and the ribs were firmly united. The cavity of the chest being opened, the right pleura was found to contain about a large wash-hand basin of blood, partly fluid and partly coagulated; this had evidently escaped through a lacerated opening of the pleura, where it passes by the side of the posterior mediastinum, the cellular substance of which appeared full of dark blood. There was also a quantity of blood between the pleura and the diaphragm. On tracing down the thoracic aorta nothing unusual could be detected; but just where it enters between the pillars of the diaphragm, and becomes abdominal, a distinct aneurismal sac was clearly seen. This had given way at its upper part, and the blood had passed up between

the pillars of the diaphragm, and had thus entered the posterior mediastinum, which it had distended to such a degree as to ultimately lacerate the pleura, and in that manner the effusion had taken place into the right pleura.

NOTE FROM MR. S. COOPER.

[WE readily give insertion to the following note; at the same time we cannot help expressing our astonishment that Mr. Cooper should not perceive that the ridiculous is quite as strongly marked in his version as in ours: giving a lengthened extract does not alter the bathos of the "expression," that John Hunter's memory should endure "*till the infernal waters of Lethe became the common beverage of the universe.*" This was the only point our report aimed at giving; and this Mr. Cooper, by his *reclamation*, makes more conspicuous than it was before. As to the rest, we said before, and say again, the oration was a very good oration.—*Ed. Gaz.*]

To the Editor of the London Medical Gazette.

SIR,

In your notice of the Hunterian Oration, you ascribe to me the following passage:—

"John Hunter, Mr. President and Gentlemen, is removed from us; but, until the infernal waters of Lethe have become the common beverage of the human race, his memory will remain!"

What was truly delivered runs thus:—"John Hunter is no longer a living member of our profession; he has quitted the stage on which he acted a most distinguished part; but his deeds live after him, and his name must continue to be revered, as long as the power of discriminating splendid merit shall bid defiance to all revolutions. To use a figurative expression, I may say, that, until the infernal waters of Lethe become the common beverage of the universe, his works shall never be forgotten, but that, as the record of them descends from generation to generation, they shall continue to receive new honours at every transmission."

Such are these two sentences, as they appear in the MS. copy of the oration; and I would rather be judged of, in this instance, by my own composition than by that of your reporter.

I am, sir,
Your obedient servant,
S. COOPER.

Great Russell-Street,
Feb. 20, 1832.

REPORT OF CHOLERA IN LONDON, UP TO THE AFTERNOON OF FRIDAY, FEBRUARY 24TH.

New cases since our last report, 19
Deaths 17

	Cases from Commencement.	Deaths from Commencement.
Limehouse	7	4
Afloat in the River	6	3
Southwark	26	14
Lambeth	4	4
St. Marylebone ...	1	1
Rotherhithe, Ratcliffe, & White-chapel..... }	5	5
Total.....	49	31

METEOROLOGICAL JOURNAL,

Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.

Feb. 1832.	THERMOMETER.	BAROMETER.
16	from 19 to 38	from 29.85 to 29.60
17	30 45	29.62 29.79
18	33 46	30.02 30.22
19	30 42	30.22 30.20
20	21 42	30.22 30.25
21	29 48	30.19 30.23
22	24 37	30.25 30.26

Prevailing winds N. and N.E.

Except the 20th and 21st, generally cloudy.

Rain at times on the 17th and 18th.

Rain fallen, .025 of an inch.

CHARLES HENRY ADAMS.

NOTICES.

The letter, dated "Brighton East Cliff," is without signature. The contents, however, are at all events unfit for publication.

Dr. P.—k's letter does not appear to us to contain any thing of sufficient novelty to induce us to insert it.

No Pupil: the papers on the same subject, which have appeared in the last two numbers, appear to us to embrace all that he has said.

Y. Z.: we cannot insert the absurd tirade about Dr. Ryan.

ERRATA.

In our last number, page 763, for "about 8½, an improvement," read "no improvement;" and in the first case from Musselburgh, for "eyes erected," read "eyes everted."

W. WILSON, Printer, 57, Skinner-Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MARCH 3, 1832.

LECTURES

ON

THE THEORY AND PRACTICE OF
MEDICINE;

Delivered at the London University,

By DR. ELLIOTSON.

PART I.—LECTURE XXII.

Fever.

FEVERS, gentlemen, are divided in general into three kinds—intermittent, remittent, and continued. Some divide them, as you may recollect I mentioned in my introductory lectures, into intermittent and continued only, and subdivide continued into those which continue unremittingly, and which are therefore called *continent*, and into those which have remissions, and are called *remittent*: you may divide them in either of these ways. I shall first speak of

Intermittent Fever.

All fevers, whether intermittent or continued, are characterized by those symptoms which I before spoke of, when treating of inflammation, under the name of *pyrexia*—that particular set of symptoms which are denominated *pyrexia*. In speaking of inflammation, I mentioned that there are only two stages well marked in the *pyrexia* of that complaint—cold and heat, but in an attack of real fever there are often three—a cold, a hot, and a sweating stage. Now in intermittent fever these are very distinctly marked. In continued fever you may have a shivering at the beginning, but then it will terminate in the course of the disease, and you will have long continued heat to treat; it is only towards the last that there are any sweats, and frequently there are none of a remarkable character; frequently there is merely moisture of the skin as the disease declines. But in intermittent fever there are usually three distinct stages—a cold, a hot, and a sweating stage, the first of which is the

Cold Stage.—When this stage is about to commence, the patient feels himself very weak and listless; he begins to yawn, gape, and stretch; he finds his mind less active, and his external senses are more or less dull; sometimes there is even real stupor, but in all cases the patient's mind is very dull; he is unable to go on with what he is about, and even his external senses are impaired; there is also at the same time a great depression of spirits. Very soon a sensation of coldness is felt, first of all in the back, and the patient will complain of chilliness before others who touch him can perceive it. As soon as the cold has begun, the surface becomes pale and dry, and the mouth and fauces also become dry. Then the patient begins slightly to tremble; he becomes really cold, the temperature falls perceptibly to others, he trembles more and more, and becomes colder, till he is in a state of downright shivering, and then his jaws chatter. The constriction of the skin at this time is such that it becomes rough; it is so exceedingly constricted that it becomes roughened, and that state is called *horripilatio*, in which the hairs stand an end. In common language this is called *goose's skin*; but in Latin, *cutis anserina*: such is the shrinking, that rings will fall off that fitted very well. The urine which is made at this time is pale and scanty. Probably the same constriction of the secreting vessels of the kidney takes place which occasions the dryness of the surface of the body. It is the same constriction, I presume, of the secreting vessels which gives rise to the dryness of the mouth and fauces, and also the thirst. This is altogether a state of debility, and consequently the pulse is weak, and sometimes slow; and I presume, from the accumulation of blood in the internal parts, the breath is generally short. Sometimes the stomach is affected with vomiting. In a very intense cold stage, the face, hands, and feet, become blue, and the fingers become shrivelled and the eyes sunk.

Hot Stage.—After this state of things, in

which you see that the blood has receded from the surface, and probably from all the small vessels, so that it has accumulated in a large quantity in the large ones of the interior—after it has existed a certain period, of very various duration, the skin relaxes, it regains its warmth, colour, and sensibility, and the pulse becomes quick and fuller. The heat, the colour, and the sensibility of the skin, go on increasing, till at length they exceed their natural standard, and the pulse grows full and very rapid. Such is the excitement now, that not unfrequently the patient complains of headache, and even delirium occurs. The thirst, the dryness of the skin, and the deficiency of the urine continue, probably from the extremities of the secreting vessels being still in a state of constriction, though the small vessels have become filled with blood. The urine, however, changes its character; from being pale and watery, it is high-coloured, but it is still clear. The breath becomes fuller, most probably from the blood getting into the small vessels, and the patient can take a deep inspiration; there is not that slow breathing that there was at first, but still there is more or less oppression; for the heart is in violent action, and when that is the case there is always a degree of dyspnoea. That state now takes place which is vulgarly called *fever*. If the disease intermit altogether, it is called *ague*, from the French word *ague*, sharp, acute; but the common people limit the word “fever” to the hot, or hot and sweating stages, and denominate the cold stage only “ague,” so that it is common to hear one of the lower orders tell you that he has got the ague and fever; but ague, properly speaking, includes the whole of the three stages. You may recollect the following passage in *Macbeth*:—

— “The obscure bird
Clamour'd the livelong night: some say the earth
Was *feverous*, and did *shake*.”

the word fever being applied to the cold stage as well as to the hot in those days. There can be no doubt that both the expression *pyrexia* and *fever*, though made by medical writers to include the whole stages, imply properly the hot only, as the former is derived from $\pi\upsilon\rho$, fire, and the latter from *ferveo*, to be hot.

Sweating Stage.—After the hot stage, the skin at length becomes still softer; it grows also moist, the moisture augmenting, till at last the person is in a profuse sweat. The same relaxation of the secreting vessels takes place within; the thirst, therefore, declines, the urine becomes copious, and the vessels let through so much substance that it contains matters which form a lateritious sediment. After the sweating has continued an indefinite time, the pulse gradually grows slower, the sweating and every other symptom diminish. The appetite, which is

generally absent in all the three stages, now returns, and the patient is as well as though nothing had happened. If the disease last very long, the patient becomes shattered by it; but in general, if the disease be not very severe, if there be no local affection, if the patient have not long laboured under it, when the paroxysm is over, he is much about as he was before.

Varieties in the Symptoms.—Now there is a great variety in the intensity of every one of the symptoms which I have mentioned; there is a great variety in the relative intensity of the different stages, and a great variety as to the intensity of the whole disease together. This, however, is not all; there are occasionally incidental symptoms, such as tetanus, convulsions, fainting, violent delirium, and the appearance of petechiæ on the skin. Some persons in the paroxysm have been known to have their muscles rigid—absolutely in a tetanic state; others have been violently convulsed; fainting has occurred; and delirium, in the hot stage, is very common. Sir John Pringle mentions violent delirium as the character of an epidemic intermittent which he saw prevail. He also mentions, that at Copenhagen, in 1652, petechiæ appeared in the hot stage of an intermittent which prevailed there. Bartholini gives an account of an epidemic in which petechiæ appeared in the hot stage, always disappearing afterwards. I have not seen the whole of these things; delirium in the hot stage is common enough, and in two cases, I saw in the cold stage violent clenching of the hands, so that the fingers and thumbs were drawn together in the most violent manner, and continued so until the cold stage was over.

Varieties as to Type.—Now these stages may all take place in the course of one day, and never return, and then the disease has been called *ephemera*—a disease of a day's duration; but for the most part these stages return periodically. They return, in general, not only regularly, but *periodically*, so that you do not have ephemeral ague, but intermittent fever. The intermission between two paroxysms is usually at least part of one day; (by day in medicine, we of course mean day and night—the 24 hours) usually the intermission is part of a day, or it may also be a whole day, or two days; beyond this the intermission is rarely regular. If the intermission be only part of a day, the fever is called *quotidian*—that is to say, if the attack returns every day, there being an intermission of only part of a day, it is called *quotidian*. If there be an intermission of a whole day, so that the patient has an attack every other day, it is called *tertian*. It assumes this name because the first day is counted as well as the third; the day on which a person is attacked is the first, the day of intermission the second, and the day

of the second attack the third; it is, therefore, called *tertian*. If the intermission extend to two days, so that that the patient shall have a fit on Monday, none on Tuesday, none on Wednesday, but have one again on Thursday, then it is called *quartan*. The day of the paroxysm constitutes the first, then follow the two days of intermission, making three days, and then on the fourth you have another paroxysm. These are the most common forms of the types of intermittent fever, and in London the quotidian and tertian are far more frequently seen than the quartan.

But the intermission may be still longer, so that you may have not a quartan, but a quintan, a sextan, a septiman, or even a deciman, so that there is an interval of nine days, and the first day being counted, it makes up the ten. I heard a gentleman say, that he saw a septiman take place for three weeks—that is to say, the person had a paroxysm after an interval of six days, occurring on the seventh day, regularly for three weeks. I myself, in 1827, had a patient in St. Thomas's Hospital with a septiman; he regularly had his disease after an interval of six days. During the same year I had another patient, who had an interval always of four days, so that his disease was a quintan. I once treated a double octavan,—a man had a paroxysm every Sunday and Thursday, the Sunday paroxysm being at one hour, the Thursday paroxysm always at another. When they extend to these periods—when they are more than a quartan—when the intermission is longer than two days, the disease is called *erratica*; it wanders out of the usual regular course. The epithet "*erratica*" is also given to an ague which observes no regular period—which at one attack is quartan, at the next quintan, &c. The term "*erratic*" you will find employed by authors in both these senses.

Now these are for the most part all the varieties which it is necessary to recollect; but as a matter of curiosity, I may mention that the disease has sometimes other types than these. Sometimes you have more than one paroxysm in a day—more than a quotidian; and I heard a gentleman say, that he saw a case in which there were four paroxysms daily. I never did. But it is very common, indeed, to see what is called a *double tertian*, where you have a paroxysm every day; but you see paroxysms two days in succession which do not belong to each other. For example, suppose a patient has an attack every day in the week, but the paroxysm on Monday is at eight o'clock in the morning, and the paroxysm on Tuesday not at eight o'clock in the morning, but at four in the afternoon; and the paroxysm on Wednesday at eight o'clock in the morning, like the paroxysm on Monday, so that the paroxysms of Monday and Wednesday agree; whereas the paroxysm on Thurs-

day is at four o'clock in the afternoon; the same hour as the Tuesday paroxysm. Now this appears a quotidian, because it occurs every day, but it is, as it were, a double tertian—it is as if the patient had a tertian which came Monday and Wednesday, and another which came Tuesday and Thursday; and inasmuch as the paroxysms occurring every other day agree, it is called not a quotidian, but a double tertian; and in Latin, *tertiana duplex*. But it will sometimes happen that there shall be two fits on the day of attack, and none on the following day. Supposing the disease comes only every other day, as for instance Monday and Wednesday, but you have on each of those days two attacks, it is then called a *double tertian*, *tertiana duplicata*; that is the difference between *tertiana duplex* and *tertiana duplicata*. I do not know whether you will be at all questioned on this matter, but it certainly is a singular fact in the history of the disease, that such occurrences take place.

Respecting the double tertian, the one which comes on Monday and Wednesday, for instance, and Tuesday and Thursday, but at different hours, you will sometimes find scarcely any intermission between the Monday's and the Tuesday's paroxysm, the intermission between the Tuesday and Wednesday paroxysm being distinct enough, or, *vice versâ*, that the one is hardly over when the other begins; and it is then called *semi-tertian*, *half-tertian*, or, if you prefer Greek, *hemitritæa*. The paroxysms are brought so near, that it is almost remittent fever, and it is scarcely worth while to call it any thing else than remittent; it has, however, as I have just stated, received the name of "*semi-tertian*;" and though the interval between the attacks on the first and second day is so short, yet the intermission between the second and third is much greater. You may, however, not only have a double tertian, one on Monday and one on Wednesday at a certain hour, and again on Tuesday and Thursday at a given hour, but you may have on Monday two fits, and on Wednesday two fits, and then this is called a *triple tertian*.

You may also have a *quartana duplex* (there being an additional paroxysm on the first day of the intermission, at its own hour), and a *quartana duplicata* (two paroxysms on the day of attack), and also a tripled quartan (three paroxysms on the day of attack); but these things are so minute that you may not be able to follow them in a lecture, and therefore it is better to refer to them in books; and perhaps immediately after you have learned them you will forget them again. The observations I have made are sufficient for all practical purposes, and I only wish you to be aware that these extraordinary deviations do certainly take place.

Duration of the Paroxysm.—Now a paroxysm of intermittent fever, including the

three stages, is generally finished in eighteen hours; it is a rare occurrence for it to last longer than this period. Dr. Good, however, quotes an author, who stated that he saw a paroxysm which lasted exactly *one minute*. How the stages were divided, I do not know. What was the severity of the cold stage, it is impossible to conceive; but I should think, that when each of these stages was over, the man might have doubted whether he had been ill or not: but authors may be quoted for any thing.

Periods of Attack.—The paroxysm of ague is said, like almost all fevers indeed, to begin more frequently in the day than in the night. As a general rule it may be stated, that the paroxysms commence between eight o'clock in the morning and the same hour in the evening. Of course there are ten thousand exceptions, but in the majority of cases the paroxysm begins in the day.

You will find it mentioned in authors, that a quotidian usually has its paroxysms in the morning, a tertian at noon, and a quartan in the afternoon. I believe there is some truth in the observation, that a quartan generally begins late in the day; but you will everlastingly see tertians come on in the afternoon, quartans come on at noon, and quotidiens in the evening.

Relative Duration of the Paroxysms.—It is said likewise, that a quotidian has the shortest cold stage, but altogether the longest paroxysm; that a tertian has the longest cold stage, but altogether the shortest paroxysm; whereas a quartan has a still longer cold stage, but nevertheless a shorter paroxysm than either of the others: but in this there is no universal rule.

Partial Ague.—You will find in authors some very curious cases of paroxysms affecting only a part of the body. There are a number of cases on record by authors entitled to credit. There is one case mentioned in which a vertical half of the body suffered an attack of ague, and the other half became convulsed in the cold fit. What is still more singular, the same half was not always similarly affected, but the symptoms changed sides. You will likewise find a case mentioned where the paroxysms of ague affected one-half of the body horizontally. In some cases it has only affected half the head; and Dr. Macculloch says that a paroxysm may affect one limb only. Now these facts are not at all insulated; they are perfectly analogous to many others which are occasionally observed in the human body. Epilepsy will sometimes affect only one-half of the body; sometimes it will affect only one limb. Catalepsy will do the same; and paralysis so continually does this, that one of the established forms of palsy is hemiplegia, and another is paraplegia. Dr. Abercrombie, in his work on the Pathology of the Brain, says that a friend of his, when in exercise, only sweats in a vertical half of the

body—that there is a line upon the forehead, perfectly distinguishing the sweating from the dry side; but that if he take very violent exercise indeed, then the dry side is at last forced to sweat out a little like the other. You will find a case mentioned by Dr. Falconer, in which a child became pale and emaciated in one-half of the body only. You will find in the work of Andral, another modern writer, like Dr. Abercrombie, instances in which sweating took place in one-half of the body only. In some of these cases it was a vertical, and in others a horizontal half.

Change of Type.—Intermittent fevers, during their progress, frequently change their type. From tertian they will frequently become quotidian; from quotidian they will become tertian; and from either of these, they will become quartan; and from quartan they will become either of these. Sometimes they will only change their type once; sometimes they will change it more than once; and sometimes they will continue changing, so that they observe no rule at all; and then, as I have already said, they are called *erratic*. Another variety is, they will not change their types—they will still remain quotidian, tertian, or quartan—but they will change the hour of their attack; instead of beginning at the same hour, they will begin later and later, or the reverse; and sometimes they will begin with such irregularity that a patient never knows when to expect them. Sometimes they will be very irregular at first, and then at last they will grow regular; seem only after a time to make up their mind how they shall go on. At other times they will grow milder only. It is a general rule that the paroxysm grows later and later as they grow milder. As the paroxysms become more severe, more intense, and the disease gets worse, they generally come on earlier and earlier; but when the paroxysm grows later and later, and at the same time milder and milder, it at last degenerates down into nothing more than a little chilliness, a little heat, or a little sweating, or a little of all. It is very common, at last, for the paroxysm to shew itself only a little at a certain time of the day, or for there to be only a little sweating, or only a little chilliness.

These diseases occur chiefly in spring and autumn; you will therefore read in authors continually of vernal and autumnal ague. But the quartans usually take place in autumn—that is to say, there are more quartans among autumnal than among vernal agues.

Imperfect Paroxysms.—Although agues have generally these three stages, you will sometimes see the paroxysms imperfect even before the disease has much declined. I have often seen and cured an ague in which the cold stage took place, but was never succeeded either by the hot or sweating stage. So sometimes the hot stage will come on

alone, without being preceded by the cold stage. Generally a paroxysm will be terminated by the sweating stage, but sometimes there will be no sweating at all. You may have either of the three stages alone. This is worthy of remark: it is an undoubted fact, for I have witnessed it all over and over again. Cullen says that the cold stage is necessary to the others; he considers the cold stage to excite the *vis medicatrix nature*; that, to get rid of the cold stage, nature bestirs herself, and brings on the hot, and then the sweating stage. But this is a fallacy, for I know you may have the hot stage without any cold preceding it, and that you may have the cold stage without any subsequent hot stage.

Dumb Ague.—Sometimes there is no regular paroxysm at all, but patients with ague will have a frequent and general chilliness at various periods, and great depression of spirits—so that they will cry, yawn, stretch, and even be frequently a little silly, and have a number of odd feelings. Now this state of things is well known by the lower orders, and called by them, very significantly, the *dumb or dead ague*—an ague which is not at all lively, or does not speak or shew itself in an open and manly manner. It is, however, to be treated exactly like all other forms of the complaint.

I have mentioned, respecting the variety in the paroxysms, that sometimes there will be one stage alone without the others; but one author tells us that he saw all the stages reversed—that the disease began with sweating, then the hot stage came on, and the paroxysm was closed by the cold stage.

Premonitory Symptoms.—Before this disease is fully formed, the attack is sometimes preceded by headache, by pains in the nerves, neuralgia, by vomiting, by general indisposition; and all this may last for a longer or shorter time. These symptoms may cease on the appearance of the disease, or they may continue more or less when the affection is established. Even continued fever has preceded ague.

Duration of the Disease.—Ague may last, from one paroxysm, from being ephemeral, to many years; at least it did do so before we could cure it as we can now. One case is said to have lasted eight-and-forty years. Lommius, on the continent, who wrote in beautiful Latin, mentions a case which lasted twenty years. One unfortunate fellow, Valescus of Tarenta, is said to have had it all his life. Dr. Gregory said that he saw one case which had lasted four years. On the other hand, Good quotes Senac for a case in which the disease destroyed life the moment it began. When it proves fatal, Sydenham says that it is in the cold stage, because, when the hot stage comes on, nature is getting the better of the disease; that a patient cannot die from it if nature

be recovering, as is the case in the hot stage. But the truth is, that it will kill in the hot stage. In hotter climates than our own, there is great congestion of the internal parts of the head, and persons will fall into a state of coma, as they do in the first stage. I have known some people have it every spring for many years. Although we have never seen people that have it regularly all the year round, yet most of us have seen individuals who have the disease regularly once a year. Respecting its duration, however, quartans are said to last the longest; tertians and quotidians are more like an acute disease, but the quartan coming on after an intermission of two days, partakes more of the nature of a chronic affection, and being chronic in its character, so really it is chronic in its duration. Certainly it is the quartan that lasts longer than the others in general, and it is generally more difficult to cure. It is said, again, on the same principle, that a tertian, left to itself, lasts longer than a quotidian.

Liability of all ages to Ague.—Ague affects all ages: it is seen in young children even at the breast. I have at this moment an old man, about eighty, under my care for this affection. However, it is said to affect children earlier than when at the breast; to affect them before they are born. You will find cases mentioned, not only in which the children had a paroxysm of ague the very moment that they came into the world,—just as it is said that some children have had hooping-cough, so that the first thing they did was to hoop instead of cry,—but you will find some cases on record where a child has had the disease before it was born. In Dr. Russell's History of Aleppo there is an account of a woman who had a tertian ague. This woman was with child, and she shook every other day, but the child within her she felt regularly shaking on the day when she was disposed to be quiet. She shook, for example, on the Monday and Wednesday, but the little one shook on the Tuesday and Thursday, so that she had one tertian and it another. If it had not been for this, she could not have ascertained it, nor could the doctors. What farther proves that the child had a different ague from the mother, peruvian bark was given to the latter, and it cured both her and the child; but as the child was young, younger than its mother, I suppose the bark had more effect upon it, for it was cured one paroxysm before the mother. A gentleman informed me that he saw such a case at a dispensary in London. The woman came shaking with ague, and the child within her shook like herself, only at a different time. I suppose there is no doubt of the truth of the case related by Dr. Russell. There is no question about children having had the small-pox in the womb. Mr. Abernethy used to mention the case of a child

that had the disease, though not the symptom of hooping-cough, before it was born. Of course it was affected by the contagion: though for want of breath it could not hoop, yet it embraced the very first opportunity it had for doing so.

Liability to recurrence.—When this disease has once occurred, it is very liable to return from common causes. Cold, wet, and the east wind particularly in this country, have a tendency to bring it back. Whether it will recur spontaneously, I do not know; whether without cold and moisture, and without an east wind, or some wind charged with malaria, it will return, I cannot say, but certainly the least cause will frequently bring it back, when a person has once laboured under it. Dr. Gregory used to mention that he had seen two persons in whom ague returned from some common cause after having been absent forty-seven years. Dr. Gregory also stated that it had been known to recur after a lapse of years, at the day and hour on which it originally began.

Influence on other affections.—The presence of this disease gives an intermittent or remittent tendency to many other affections that may be present. A person has a disease, and he is then attacked by ague; the first disease will have a tendency to blend with the ague, so that he will have remissions, if not intermissions of it. When ague prevails epidemically, persons who are exempt from it still shew a tendency to an intermittent or remittent character of any other disease which they may have at that period. This, however, is chiefly seen in fever. When ague prevails, or persons are exposed to the cause of ague, continued fever has a tendency to put on the remittent form, and remittent fever appears to be a combination of continued and intermittent fever. Ague is very frequently followed by rheumatism. I am not acquainted with a more frequent occurrence than that of ague being followed by severe rheumatic pains in the extremities, or in the head. Occasionally it will alternate with rheumatism, so that the rheumatism shall cease and the ague return, and *vice versa*. Dr. Gregory used to mention that he saw ague alternate with epistaxis, and in another case he saw it alternate with hæmaturia—bleeding from the urinary passages, which bleeding, however, at last ceased, and then the ague returned, and was followed by paralysis. He also used to mention that he saw two cases of ague continue four years, at the end of which they were cured by bark, but periodical delirium came on in its stead.

Ague attended by local inflammation.—Ague is very often attended by local inflammation; sometimes by inflammatory pains of the head; sometimes by inflammatory affections of the chest, and likewise of the abdomen, particularly gastritis and hepatitis. The ab-

dominal viscera suffer the most severely in autumn and in hot climates; and ague is likewise very frequently attended, when not by inflammation of these parts, with bilious vomiting and bilious purging, and even by jaundice and by dysentery. In some places, for instance in Zealand, Sir John Pringle mentions that ague is called *gall fever*. On account of the great irritation of the liver that takes place, and the abundant secretion of bile, aguish diseases are then called *gall fever*.

Morbid appearances.—After death during intermittent fever we often find congestion and effusion in the head, chest, or abdomen. The mucous surface of the alimentary canal is likewise in a state of great congestion, and the liver contains a large quantity of bile. When this disease proves fatal, it frequently does so by great internal congestion, whence you find fulness of all the blood-vessels of the head and chest, the stomach and intestines, and the large quantity of bile in the liver. When the disease has continued any time, you have various organic affections, such as dropsies and other things, which I shall hereafter mention. But when a patient dies in a paroxysm, or dies after the disease has existed only for a short time, you find decided marks of internal congestion.

I will consider the chronic effects of the disease at the next lecture.

ANALYSES & NOTICES OF BOOKS.

“L'Auteur se tue à allonger ce que le lecteur se tue à abréger.”—D'ALEMBERT.

A Treatise on the Diseases of the Heart and Great Vessels; comprising a new view of the Physiology of the Heart's Action, according to which the Physical Signs are explained. By J. HOPE, M.D. Senior Physician to the St. Marylebone Infirmary. W. Kidd, 228, Regent-Street, 1832.

WE promised, in a preceding number, to take an early occasion of entering upon the consideration of the volume that now lies before us. Hitherto we have been prevented by matter of immediate urgency from doing that deliberate justice to its contents which the important nature of the subject, and the able manner in which it is treated, naturally demanded from us. We rejoice that the time is at length arrived when we feel ourselves enabled to redeem our pledge, by laying before our readers an analysis of the facts and opinions that are

contained in this valuable and original production.

"Notwithstanding (says our author) the strong light diffused over the diseases of the heart by the researches of Corvisart, Kreysig, Burns, Laennec, and Bertin; notwithstanding the brilliant sunshine emanating from the discovery of Laennec—a discovery which, according to M. Bertin, has in a few years more completely illumined the diagnosis of the diseases in question than all the other modes of exploration had done for two centuries—the great body of the profession still deny that the piercing ray has reached its destination; still doubt the utility of auscultation in reference to the primary organ of circulation; still find the ordinary symptoms beset with their accustomed difficulties; still complain, in short, that the obscurity which involves the diseases of which we speak, is scarcely less profound than ever; and while conflicting opinions are embarrassing the judgment, and undermining the confidence of the patient investigator of truth, there is a general outcry for an additional mass of well attested evidence which may bring the subject to some kind of conclusion." (p. 11).

To furnish this "mass of well-attested evidence," Dr. Hope has ventured to explore the vast region that lay almost untrodden before him. To supply the vacancies left by preceding writers—to remedy the defects, and confirm the accuracies of their observations—to detect and explain the fallacious portions of their principles—to apply facts thus established to the practical end of improving the treatment of these diseases—and to erect the whole into a fabric of solid foundation, are the objects to which our author has devoted the energies of his mind with no common degree of labour and research. It will be for the reader to determine in the sequel in what manner this task has been executed, and whether the author has succeeded or not in bringing "the subject to some kind of conclusion."

Having, in a copious introduction, well explained the general design of the work, and pointed out its more prominent features, Dr. Hope enters at once upon his subject, which is arranged with great perspicuity in six parts, viz. 1. The anatomy and physiology of the heart. 2. Inflammatory affections of the heart and great vessels. 3. Organic

affections. 4. Nervous affections. 5. Miscellaneous affections. 6. Cases. These we shall proceed to examine seriatim.

In the first chapter, or that on the anatomy and physiology of the heart, our author has very judiciously pretermitted, as being foreign to his purpose, any minute anatomical description of the central organ of circulation. As, however, an acquaintance with the exact position of its several compartments in relation to each other, and to the surrounding structures, is of capital importance with a view to auscultation, he has supplied us with the necessary particulars in a description of much clearness and accuracy. The reader will do well to study it carefully in the original, as we regret that our limits do not permit us to lay it before him in this place.

For the purpose of percussion, when great delicacy is required, Dr. Hope uses the *plessimeter*, lined with wash-leather, as originally recommended by Piorri, in preference to the ordinary mode of percussion on the back of the fingers. By means of this instrument he tells us, "the circumference of the heart may be measured with considerable nicety."

The theory of the heart's action, as proposed by Laennec, not appearing to him to be sufficient to explain the various phenomena that are observable, he next institutes a course of inquiry, in order to satisfy himself on this head, and by means of a series of experiments, performed with the assistance, and corroborated by the testimony of many distinguished members of the profession, he arrives at the conclusions which follow:—

"Of the Motions of the Heart."

1. The auricles contract so immediately before the ventricles, that the one motion is propagated into the other almost as if by continuity of action, yet the motion is not so quick that it cannot readily be traced with the eye.
2. The extent of the auricular contraction is very inconsiderable, probably not amounting to one-third of its volume. Hence the quantity of blood expelled by it into the ventricle is much less than its capacity would indicate.
3. *The ventricular contraction is the*

cause of the impulse against the side; first, because the auricular contraction is too inconsiderable to be capable of producing it; second, because the impulse occurs AFTER the auricular contraction, and simultaneously with the ventricular, as ascertained by the sight and touch; third, because the impulse coincides with the pulse so accurately as not to admit of being ascribed to any but the same cause.

4. It is the apex of the heart which strikes the ribs.
5. The ventricular contraction commences suddenly, but it is prolonged until an instant before the second sound, which instant is occupied by the ventricular diastole.
6. The ventricles do not appear ever to empty themselves completely.
7. The systole is followed by a diastole, which is an instantaneous motion, accompanied with an influx of blood from the auricles, by which the ventricles re-expand, but the apex collapses, and retires from the side.
8. After the diastole the ventricles remain quiescent, and in a state of apparently natural fulness, until again stimulated by the succeeding auricular contraction.

Of the Sounds.

9. The first sound is caused by the systole of the ventricles.
10. The second sound is occasioned by the diastole of the ventricles.

Of the Rhythm.

Order of succession—

1. The auricular systole.
2. The ventricular systole, the impulse, and the pulse.
3. The ventricular diastole.
4. The interval of ventricular repose, towards the termination of which the auricular systole takes place.

Duration.

This is the same as indicated by Laennec, viz.—

The ventricular systole occupies half the time, or thereabout, of a whole beat.

The ventricular diastole occupies one-fourth, or at most one-third.

The interval of repose occupies one-fourth, or rather less.

The auricular systole occupies a portion of the interval of repose." (pp. 28, 29, 30).

We have carefully examined the experiments which gave birth to the above deductions, and we must confess that to us they appear free from fallacy; they were planned with judgment, and performed with precision. Though at the time of their first publication in this journal, in the year 1830, the opinions and deductions of our author were by some called in question, yet a repetition of the same experiments, recently made in presence of a number of the most distinguished physiologists of this country, has only confirmed their accuracy, and has left no room to doubt of their complete truth. They demand careful consideration, and we must refer the reader to our sixth and seventh volumes, as our space will not permit us to give even a brief sketch of them here.

Having established the conclusions above detailed, our author proceeds to offer a few remarks in explanation of the muscular mechanism of the heart's action, and of the cause of its sounds, both in a state of health and of disease. On this last head he combats the opinions of Laennec and others, who refer the *morbid* sounds of the heart (the natural ones being left by them totally unexplained,) to "the movement of the muscular fibres of the organ when they contract *spasmodically*." He contends that such cause is totally inadequate to account for them; and he shews, by a reference to the laws of physics, that they "are occasioned by the motions of the contained fluid." Thus,

"When the ventricles contract, an impulse is given to the particles of fluid in contact with them; and this being propagated by collision from particle to particle, generates sound." (p. 48.) The irregularity of the inner surface of the ventricles, the numerous conflicting currents of blood thereby occasioned, all converging towards the aortic and pulmonary orifices, tend to favour the production of sound: hence it happens, that in proportion to the quick or gradual nature of the contraction, will the degree of sound be greater or less. This constitutes the reason why hypertrophous hearts (which contract slowly) are not attended with an intensity of sound at all commensurate with the violence of the impulse against the side; and why

ventricles simply dilated, however thin in their muscular substance, if they act suddenly, occasion "sounds proportionably short, loud, and clear."

"The second sound, or that of the ventricular diastole, is generated by a more simple mechanism, and is consequently more uniform in its character. When the diastole takes place, the blood, put in motion by a number of concurrent circumstances already particularized, shoots with instantaneous velocity from the auricles into the ventricles; and the reaction of the ventricular walls on its particles, when their course is abruptly arrested by the completion of the diastole, is, I conceive, the cause of the loud, brief, and clear sound." "The auricles do not contribute to the production of either of the sounds." (p. 49.)

Dr. Hope illustrates these views in various ways; by a reference to the laws of physics, as well as by a survey of the modifications of sound observable in cases of hypertrophy, dilatation, and other diseases of the heart—modifications that are easily understood on the above principles, but which, as Laennec himself found, were not explicable according to his own doctrines.

Laennec supposed that the first sound of the heart was occasioned by the systole of the ventricles, the second by the systole of the auricles. This proposition, when directed to explain the murmurs occasioned by affections of the heart, was met by so many anomalies and exceptions, that it has been found impossible to apply it with any degree of certainty in determining the precise nature and seat of the disease. Dr. Hope shews by direct experiment, supported by the authority of Turner, and of the older physiologists, Haller, Harvey, Lancisi, &c. that this view of the heart's action is manifestly inaccurate, inasmuch as the systole of the auricle takes place *before* that of the ventricle. He arrives, as we have seen from his experiments, at the conclusion, that the first sound attends the ventricular systole, the second the *ventricular diastole*; both caused by the motions of the contained fluid. This forms the basis upon which he is enabled to found an explanation of the various phenomena that auscultation detects in the action of the heart and arteries during a state of disease, whether organic or func-

tional. The remainder of the first part of the work is occupied in illustrating this point.

On the subject of murmurs occasioned by valvular disease, some facts are mentioned by the author, which "assimilate with, and substantiate the view which he has taken of the heart's action."

"When the aortic or the pulmonic valves are contracted, a morbid murmur accompanies the sound of the ventricular systole; and when the valves, not closing accurately, admit of regurgitation, a murmur accompanies the sound of the ventricular diastole also; but in the latter case it is extremely slight and brief, because, as I imagine, the swift influx of blood from the auricle during the diastole, almost instantly puts an end to any regurgitation capable of producing sound. When the *mitral* or the *tricuspid* valve is contracted, a murmur accompanies, and sometimes entirely supersedes, the second sound, being occasioned by the obstructed passage of the blood from the auricle into the ventricle, during the diastole of the latter. When the valve, not closing accurately, admits of regurgitation, a murmur accompanies the first sound. This fact was one of the very few overlooked by that wonderfully accurate observer Laennec. It was noticed by the writer in 1825, and the number of cases with which he is enabled to substantiate it, leads him to assume it as certain." (pp. 54, 55.)

"Valvular murmurs," he tells us next, "are occasioned by collision of the particles of the blood, when this fluid is by any cause thrown into preternatural commotion during its passage through the orifice of a cavity;" and "are not, as is often supposed, louder, *ceteris paribus*, in proportion as the valvular contraction is greater. On the contrary, the loudest murmurs are produced by a moderate contraction, and they become weak when it is extreme." Any disproportion between the cavities and orifices of the heart, consequent either on enlargement of the former, or contraction and irregularity of the latter, causes morbid sound. In cases of great hypertrophy, with dilatation, bellows murmur frequently attends the heart's first sound, and our author tells us, that this is chiefly attributable to the changed form of the ventricle; for

as in hypertrophy with dilatation, the cavity is more spherical than natural, and its artery consequently rises more abruptly with respect to its internal surface, the currents of blood reflected from its sides meet in the orifice at more obtuse angles, and thus by their collision not only give rise to the murmur, but impede each other's passage into the vessel. *For the latter reason the pulse is sometimes small and weak, when the impulse of the heart is violent*—a paradox with which authors have been much perplexed." (pp. 63, 64.)

Murmurs, independent of organic disease, are frequently noticed both in the heart and arteries. They were referred by Laennec "to a real spasmodic contraction, whether of the heart or of the arteries;" whilst in another place he (Laennec) tells us, they "accompany the diastole of the heart, and that of the arteries." Now, as it is obvious that a *spasmodic contraction* of the heart or arteries cannot, by any possibility, exist during their *diastole*, this theory of Laennec's is manifestly inconsistent with itself, and easily disproved. We accordingly find that Dr. Hope takes a different view of the subject; he tells us, "I have found it (the murmur) accompany the systole of the ventricles, and not the diastole, unless it at the same time, and in a predominant degree, attended the systole. In the arteries it coincides with their diastole. The purring tremor occurs at the same moment, and is a result of the same cause. The arterial thrill is nothing more than a less degree of the purring tremor." (p. 69.)

He establishes these positions by shewing—

1. That, in physics, liquids passing through inert tubes, without admixture of air, produce sounds, which are louder in proportion to bends, projections, or irregularities of the tube, and depend on the friction and collision of the particles of fluid.

2. That in the heart and arteries, such modifications in the motion of the fluids arise either from the diminished quantity of the blood, or the augmented irritability of the heart: whence an increase of friction, and consequently a production of morbid murmurs.

Being engaged with Dr. Marshall Hall in a series of experiments on the effects of loss of blood, he took the op-

portunity of studying the stethoscopic phenomena of the circulation under all the circumstances of collapse, reaction, &c.

Several dogs were bled more or less frequently, and the general result obtained was, that exactly in the ratio of the quantity of blood lost, within certain limits, was the degree of whizzing or sawing murmur heard over the heart, the arterial thrill and purring murmur being, at the same time, distinguishable in all the larger arteries, as the carotids, femorals, &c. "The animals being extremely nervous and irritable, the pulse was instantly accelerated ten or fifteen beats per minute by the slightest excitement, as that of being moved or startled; and the murmur and jerk sustained, in consequence, a remarkable increase." (p. 73.)

3dly. He shews that whenever these sounds occur as a result of functional derangement, there always necessarily exists "a greater or less degree of irritability, and jerking action of the heart and arteries." Dr. Hope considers all the murmurs of the heart, to which a multiplicity of names have been given by the French, as modifications of one and the same.

We are now arrived at the termination of the first part of this work, and must for the present conclude: we cannot do so, however, without expressing the great satisfaction we experience in introducing Dr. Hope to our readers. He has produced, in our humble opinion, the best work on Diseases of the Heart that has yet emanated from the public press. That portion alone of it which we have already analysed is, of itself, sufficient to give its author a deservedly high rank in professional estimation; and we have no hesitation in saying, that the principles which he has promulgated, and in a great measure *originated*, in reference to the rationale of the heart's action and sounds, will be found the means of greatly simplifying the diagnosis, and improving our general knowledge, of a class of diseases that has hitherto been involved in much uncertainty.

We shall take another opportunity of returning to the subject.

CONTRIBUTIONS

TO

THE PATHOLOGY OF THE BRAIN,
IN INSANITY, EPILEPSY, AND
GENERAL PARALYSIS.

BY WILLIAM DAVIDSON,

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County Lunatic Asylum.

“Quisquis enim artificiosè corpora humana
secare novit, eorumque singulas particulas diligen-
ter inquiri, ex his latentium morborum causas et
sedes faciliè intelliget, necnon accommodata reme-
dia prescribet.”—*J. Riolan, Anthrop. lib. i. p. 15.*

[Concluded from page 711.]

CASE X. Old.—Sarah Gibson, ætat. 75. Died Oct. 12, 1831. Prominent symptoms, referrible to the mental and corporeal functions, were gradual sinking of the vital powers; constant confinement to bed; marasmus; continual excitement; violent fits of rage; wild contempt of all external objects.

Autopsy, Oct. 13, 1831.—About four ounces of sero-sanguinolent effusion into the cavity of the arachnoid. The arachnoid of the convexities of the brain was dyed red, from bloody infiltration into its tissue. The pia mater was readily detached from the surface of the convolutions. Cortical substance very soft and red, and exhibited numerous bloody points when cut into. Its surface was bathed with blood, and presented several small distinct patches distinguished from its general colour, which was also much heightened, by a more intense degree of redness. The medullary substance was unusually soft throughout, without being actually disorganized, except in its horizontal fibres in the central parts of the brain, constituting the corpus callosum, septum lucidum, body of the fornix, and the commissures, which were reduced to the condition of white semi-fluid ramollissement. Minute vascularity, red colour, and diminished consistence of the pia mater generally, which was so easily lacerated as scarcely to bear its separation from the surface of the brain; with sanguineous infiltration into its tissue between the anfractuosities. The other cavities were not inspected.

CASE XI. Recent.—Thomas Philips, ætat. 26. Died Oct. 18, 1831. Pro-

minent symptoms, referrible to the mental and corporeal functions, were general muscular atony and disinclination for exercise; about two months before death, symptoms of pulmonary consumption, marching steadily to a fatal termination; diarrhœa; dementia; confirmed imbecility of mind; without exacerbations or tendency to excitement.

Autopsy, Oct. 19, 1831.—Unusual prolongation of the left posterior clinoid process of the sphenoidal bone. Patches of organized lymph, connecting the arachnoid with the pia mater, scattered here and there over the convexities of the hemispheres and along the mesial lines, from the crista galli to the torcular Hierophili. The arachnoid was slightly vascular only, and came off with the pia mater with considerable difficulty generally; and in some detached spots, in both the anterior lobes, was actually adherent to the cortical substance, the inner layer of which was torn up along with it, and presented bleeding surfaces. The convolutions were distinctly shallower than natural. The medullary substance was very firm, and its horizontal and vertical fibres beautifully displayed. The choroid plexus of both sides large and turgid.

Thorax.—About three quarts of serous effusion in the cavity of the chest. The upper lobes of both lungs were extensively condensed and tubercular, and hollowed out into numerous tubercular abscesses of various sizes, lined with cheesy matter. The lower lobe of the right side contained two masses of crude tubercular matter, about the size of a large walnut. The pulmonary tissue surrounding these morbid deposits presented the condensation of chronic pneumonia.

Abdomen.—The mucous coat of the ascending and transverse colon presented extensive patches of inflammation; the ileo-cœcal valve was much thickened and inflamed, and a few of the mucous follicles of Peyer were excavated into deep ragged tubercular ulcerations, about the size of a split pea. Several minute pustules were scattered over the lower portion of the ileum, exhibiting, in a very interesting manner, the disease at an earlier period of its progress.

CASE XII. Recent.—Elizabeth Sayers, ætat. 50. Died Oct. 20, 1831. Prominent symptoms, referrible to the

mental and corporeal functions, were a general febrile state; very rapid pulse; towards the end typhoid pyrexia; cold sweats; gradual sinking of the vital powers; violent convulsions preceding death, and terminating in coma; religious enthusiasm, of some duration, giving place to acute mania; constant agitation; frequent screaming, alternating with rapid talking; sensibility obtuse; a wild disregard of all ordinary sources of excitement.

Autopsy, Oct. 21, 1831.—Cranium remarkably hard and heavy. Dura mater much injected, and adhering intimately to the inner table of both parietal bones, so as to be split into two layers when the calvarium was removed. The dura mater immediately connected with the longitudinal sinus, about an inch from the torcular Hierophili, was much thickened, knotty, and fungous, and surrounded with a number of white granular excrescences, of which several exceeded the diameter of a split pea. The veins of the pia mater, as they opened into the sinus, were obstructed by firm coagula of dark-coloured blood, which had the appearance of having existed previous to dissolution. The sinuses of the brain were rather empty, and what blood they contained presented the usual characters of coagula formed after death. The arachnoid was opaque over the hemispheres, from white albuminous effusion into its tissue: this opacity was most apparent in the triangular spaces between the convolutions and along the course of the large vessels of the pia mater, where the membrane had the exact colour, consistence, and almost the thickness, of the buffy coat of inflamed blood. About six ounces of reddish sero-purulent fluid were found in the base of the cranium. The pia mater was removed from the brain with great difficulty. The cortical substance was highly inflamed, of a pink colour through its whole depth, and its surface when exposed to the air, after the removal of the membranes, presented innumerable red points, which gradually coalesced, and gave it the appearance of having been daubed with red paint. Its vessels also were much dilated, and there were a number of small, bloody, punctuated extravasations throughout its substance. The medullary substance was also much inflamed, and where its cut surfaces were exposed to the air, they presented the same

pink colour as the cortical substance, although in a slighter degree. On the mesial edge of the middle lobe, at the angle formed by the Sylvian fissure of the left side, the medullary and cineritious substance of two convolutions, through their whole depth, was broken down and of a brown colour, from small effusions of blood which had taken place close to each other. The choroid plexuses of all the ventricles were much injected, and unusually developed. Over the convexities of the convolutions the pia mater was found extensively injected; its minute vessels showed the most delicate scarlet arborescence, while the large vessels were filled with dark blood; in the intergyral spaces the celluloso-vascular web of this membrane was literally infiltrated with blood, which trickled from it in all parts, when it was raised from the brain. The lateral ventricles were unusually small,—a condition which seemed to be connected with an excess of volume in the medullary substance composing the centrum ovale of both hemispheres. The other cavities were not inspected.

CASE XIII. Old.—Violet Campbell, æt. 75. Died Nov. 28, 1831. Prominent symptoms, referrible to the mental and corporeal functions, were occasional excitement, with screaming, pain, and afterwards difficulty in moving the right arm; ultimately complete palsy of the same arm, and subsequently of the right leg; muscles of the face drawn to the left side; apoplectic symptoms a few days preceding death, and terminating in profound coma; marasmus; marked cancerous complexion; fatuity gradually becoming more complete.

Autopsy, Nov. 29, 1831.—The bones of the cranium were unusually thick and dense. The grey substance of the convolutions was intimately adherent to the pia mater throughout. Through the whole of this substance the most minute vascular ramifications were penetrated with injection, and in several places the blood had mingled with the pulp, so as to form a sort of homogeneous bloody detritus.

Towards the mesial edge of the posterior lobe of the left hemisphere, the dura mater, with its falx, the arachnoid and pia mater, were united, in one confused mass, with a reddish-grey, knotty tumor, about the size of a small hen's egg, which, when cut into, or pressed with the fingers, resisted like fibro-

cartilage, creaked under the knife, and had the exact appearance of a section of the crude scirrhus mass usually developed in the female mamma. Its substance was of a bluish-white colour, extremely hard, in some points of a homogeneous cartilaginous texture; in others, and these forming the greater portion of the tumor, the molecules which entered into its composition exhibited a different arrangement, being of an areolar structure, and presenting very minute cells, containing a dirty puriform fluid. Connected with the base of the tumor was a nucleus, of a dirty-white colour, which, when cut into, presented a homogeneous tubercular appearance. A small lobule of the scirrhus tumor had penetrated to the optic thalamus and striated nucleus of the same side; and the posterior extremity of the former, constituting the corpus geniculatum externum, was reduced to a state of pink-coloured ramollissement, which reached to within a line of the corpora quadrigemina, in following the transit of the tractus opticus. The brain surrounding the greater part of the tumor was completely disorganized, and exhibited a similar pink-coloured ramollissement, into which blood in several points had been infiltrated, so as to form a number of small reddish-purple masses.

About six ounces of limpid serum were found in the ventricles.

The medullary substance unconnected with the tumor was everywhere unusually firm, and in a highly vascular condition. About an inch of the course of the superior longitudinal sinus was rendered completely impervious* by the pressure of the tumor, so as to be incapable of admitting the introduction of a hog's bristle, a circumstance which satisfactorily accounted for the very remarkable condition in which we found the veins of the pia mater, which were much dilated and gorged with dark-coloured coagulated blood, and had the exact appearance and form of varicose subcutaneous veins. Notwithstanding this obstruction, not the slightest traces of serous effusion could be detected, either in the cellular web of the pia mater, or in the great cavity of the arachnoid.

Carcinomatous affections of the brain are certainly of very rare occur-

rence, so much so that almost every author who has written on this morbid structure appears to have considered the brain as a privileged organ, possessing a sort of exemption from the disease. As a proof of their extreme rarity, Dr. Baillie does not once allude to them in his work on Morbid Anatomy, and up to the recent researches of the French pathologists, I am aware of but one authentic instance of the true chondroma of the brain, published by Mr. Copland Hutchison, in the second volume of the *Medico-Chirurgical Transactions*, and another by Mr. Wade, in the *Medical and Physical Journal* for 1826. Although the apparent rarity of the disease may arise in some measure from our want of observation, and many cases of real scirrhus may have been mentioned under a different head, yet there is every reason to believe that tumors of the brain have been described by Morgagni, and other writers, under the term *schirrus*, which had scarcely any quality but some degree of hardness in common with this formidable affection. "In the brain especially," says Dr. Craigie, in his learned and truly excellent work on Pathological Anatomy, "all authors have comprehended, under this general appellation, every change of texture which was harder than that of the surrounding organ, without much regard to the anatomical characters of the new substance."

In the same work, Dr. Craigie has collected five examples of what appear to be indubitable cases of scirrhus of the brain, (the nature of that by Rostan being uncertain, from a deficiency in the anatomical details) which, with those above alluded to, and the one now published, are the only unquestionable instances of this structure with which I am acquainted.—(See Dr. Craigie's *Pathological Anatomy*, p. 466—469.)

Nothing remarkable was met with in the other cavities of the body.

CASE XIV. Old.—Margaret Gregory, ætat. 55. Died Dec. 25, 1831.—Prominent symptoms referrible to the mental and corporeal functions, were dementia, terminating in complete fatuity; excitement, with frequent screaming; constant jactitation; blindness of both eyes; complexion indicative of organic disease; marasmus.

Autopsy, Dec. 26, 1831.—About four ounces of flocculent serum were found in the base of the cranium.

* This obliteration of the sinus was accompanied with thickening of its parietes, and the deposition of tubercular matter on their internal surface.

Optic nerves of both sides atrophied, of a dark-grey colour, and of the consistence of fibro-cartilage, with obliteration of the arteria centralis. This diseased structure extended from the optic foramina to the termination of the nerves in the corpora geniculata and nates. The arachnoid was thickened and opaque, and had assumed a white milky hue where it passed over the convolutions, and in its loose duplicatures situated at the base of the brain, and behind the fourth ventricle, and commissure of the optic nerves. The pia mater was so intimately adherent to the convolutions that, in removing the membranes, an external layer of the cineritious substance, uniform in depth, and rather thicker than a wafer, was brought away with them throughout its entire periphery.

Both surfaces of this layer were of the colour of venous blood, and had the exact appearance and consistence of fungous bloody granulations. Although the cineritious substance was in a softened condition generally, yet some portions, towards the anterior lobes of the brain, were observed to be, through their whole depth, in a state of complete ramollissement, and of a light red colour. The septum lucidum and fornix were completely disorganized from greenish yellow ramollissement, and the ventricles contained about two ounces of sero-purulent fluid. Both eyes were carefully and minutely dissected, but, beyond a slight degree of atrophy, presented nothing abnormal.

Nothing remarkable was found in the other cavities.

This separation of the layers of the cortical substance, associated with softening, redness, intimate adherence of the pia mater to its surface, and other evidences of inflammation, and inflammatory irritation, are amongst the most common morbid appearances to be met with in old cases of insanity, and are attended either with general paralysis or profound dementia. Dr. Bright says that this tendency to separation is occasionally so marked in elderly persons, and in those who have laboured under symptoms of imbecility, as to be obviously a morbid state. "This state of the cineritious substance (he goes on to say) has appeared to me sometimes to be the result of habitual excess in fermented liquors, and although this condition is by no means uncommon, I do

not know of any author who has referred to it except Dr. Foville, of Rouen*." Previous to the publication of Dr. Bright's work, I pointed out, in a paper published in the second number of the North of England Medical Journal, the frequency of this morbid appearance in the brains of old lunatics, and took occasion then to add my testimony to the correctness of Dr. Foville's observations. I may again take this opportunity of remarking that I know of no treatise on insanity which contains so rich a fund of pathological research as this gentleman's memoir on mental aberration, inserted in the first volume of the *Nouveau Dictionnaire de Médecine*.

This writer, whose researches into cerebral physiology and pathology have been conducted in association with MM. Delaye and Pinel, Grand Champ, on a very extensive scale, has come to the following conclusions:—That the brain is a compound organ, of which one portion presides over the locomotive powers, and the other over those of intelligence; "that the cineritious substance is the seat of the active functions of the brain, and that the mind is chiefly influenced by derangement of that part, while the white matter is composed of fibres, which probably act as conductors, communicating the energies to the different parts of the body." These views are further developed and illustrated in the author's beautiful memoir entitled "*Encephale*", in the same Dictionary, where the reader will find such a mass of information, relative to this obscure subject, compressed into a narrow compass, as scarcely leaves any thing to desire.

Here terminate my labours for the present, and, in conclusion, I have only to add, that should these cases be favourably received by the profession, and no other person, better qualified for the task, enter upon the same field of investigation, and take up the application of the same principles, I may hereafter be induced to resume the subject. My sole reason for submitting these cases to the medical reader has been a desire to furnish him with a few authentic facts, from which I am anxious that he should draw his own conclusions.

That they are both meagre and im-

* Bright's Reports of Medical Cases, Vol. II.

perfect I am quite willing to allow, but I must, at the same time, claim for them, what is of no slight importance in pathological details, the merit of accuracy and minuteness, and I trust that they are well calculated to shew the reader that a fruitful field lies before him, ready to yield a rich harvest for any labours he may bestow upon its cultivation, and that by bearing steadily in mind that in every form, and in every transformation, and termination of mental disease, its close connexion with the nervous system is constantly to be detected, he has means within his reach adequate to the removal of most of the difficulties which now apparently so thickly beset his path.

EXAMPLES OF CHOLERA SPREADING BY CONTAGION.

To the Editor of the London Medical Gazette.

Huddersfield, 21st Feb. 1832.

SIR,

ALTHOUGH both the professional and non-professional public are generally, I believe, of opinion that cholera, under certain circumstances, is contagious, still, I think, the following account of its introduction into Hawick, in the county of Roxburgh, worthy of a place in your pages. Hawick contains nearly 5000 inhabitants, is 12 or 14 miles from the English border, and about 55 miles from the town of Morpeth, in Northumberland. A cattle-dealer from the former place, attending the extensive market held in the latter, put up at the same hostelry with a traveller from Newcastle, who was seized with and died of cholera. The dealer, immediately on his getting home, was attacked, and sunk under the complaint. His brother, nephew, and servant, were likewise affected, but all recovered. The next persons were a woman, who washed some of the clothes of the deceased, and her child. Both got well; but the husband of this woman, a man in infirm health, and not of very steady habits, was carried off after three or four days of suffering. The only other victims were two children, who were attacked about 3 A.M. and did not receive medical aid until nine. One died soon afterwards, and the other in the after-

noon of the same day. Both were just convalescent from fever, and consequently in a weak state. One of the medical attendants was seized whilst sitting at dinner, and his sister two days afterwards. They happily recovered. The total number of cases which have occurred in the place is 17, and all could be traced to the same source. Of these four died. At the date of my communication, the 15th inst., no fresh seizures had taken place for seven or eight days, and the town was pronounced free from disease.

From this short history we may form a tolerably clear idea of the manner in which the malady spreads from place to place; and we have also a satisfactory explanation of its often apparently eccentric course; how it passes over towns and extensive districts, and locates itself at a great distance from the point whence it emanated. Hawick is certainly a great stride from Morpeth, yet had the home of the cattle-dealer been a stage or two further, I think it pretty evident that he might have carried the complaint to the end of his journey without affecting the intermediate country. I consider, indeed, that the disease may leap over whatever distance an individual can travel during the latent period, or time of incubation, which, perhaps, rarely exceeds two or three days. If this be correct, why should we wonder at its progressing one time with, and another against, prevailing winds; and at the immunity of some places, whilst others undergo reiterated assaults. If a town can be inoculated once, it may be inoculated twenty times in a similar manner.

However clear the progress of cholera may appear in the preceding narrative, it is not always so easily traced. Had the illness of the cattle-dealer been slight, and had he recovered without much assistance, it is not likely that his case would have been set down as a case of cholera. At the same time, few will doubt that he might have imparted the malady to others, and from the first link in the chain of causation being lost, the origin of the complaint would be involved in mystery, and give rise to endless disputes.

Before conclusion, I wish to say a few words on the prophylaxis of this scourge of the human race. Separation of the sick from the healthy is justly advocated by all; but this is not enough. Those

who have been much exposed to the contagion should be prohibited all intercourse with the public. I especially allude to the family or inmates of every dwelling in which a case of cholera has presented itself. This appears to be the plan adopted in Edinburgh, and hitherto with the best results. Indeed, I feel confident that if it could be strictly followed, the malady might either be extinguished, or so kept under as to be of little importance. Some may possibly object to this, and contend that nothing can justify putting any restraint upon those who have not as yet shewn signs of disease—that it is too great an infringement upon the liberty of the subject; but surely the convenience and interests of a few ought to give way to the security of the many—surely the public safety should be paramount to every other consideration. Besides, I cannot see the hardship of a family being removed into a well-prepared building (of course, apart from the sick hospital) where their comforts, in all probability, would much exceed those they had previously enjoyed. A quarantine of seven or eight days, or perhaps four or five, would be sufficient to decide whether the poison had been inhaled; and during this period I would have their houses thoroughly cleaned and white-washed, and their beds, clothing, &c. exposed to considerable degrees of heat, as recommended by Dr. Henry. Vain, in my opinion, are all efforts for arresting the course of cholera, if some such plan be not generally adopted. Is this temporary restraint—this loss of time—to be weighed for a moment when the lives of thousands are at stake? How different is the method pursued in London! Were it desirable to fix upon some way of extending the ravages of cholera with greater celerity and greater certainty, what could be more likely to accomplish this than the practices reported by the press to have occurred in the modern Babylon—such as permitting unrestrained communication with the sick and their families, allowing the public to view the bodies of the dead, and the thoughtless race of Erin to wake over the corpse?

Let me, then, urge the wealthy portion of society in places not yet visited by cholera, to have in readiness, not only hospitals for the sick, but likewise *quarantine houses* for those who have been much exposed to the contagion,

and to whom suspicion of having caught the infection may justly attach. Had such a plan been adopted at Sunderland, possibly the disease would have been strangled in its cradle. I by no means cast any blame upon the authorities there; the complaint was new, and little understood; now, however, that we are better acquainted with it, and especially with its mode of propagation, great will be the responsibility of those who do not make use of the only preservative measures that have as yet been found really efficacious.

The first outlay, it is true, will be considerable, but ultimately, by curtailing the range, and shortening the duration of the pestilence, the saving, both in life and money, would be immense. Again, I treat the inhabitants of manufacturing districts, above all others, to take the subject into serious consideration: a suspension of trade for two or three months, which is certain to take place if cholera prevails alarmingly amongst them, will drain their pockets much more deeply than such preparations as are here recommended.

To sum up the prophylaxis in a few words, provide an hospital and quarantine house; feed the hungry, clothe the naked, suppress vagrants, and encourage sobriety, cleanliness, and ventilation: these are within the reach of man—these accomplished, await cheerfully the decrees of an unerring God.

I am, Sir,

Your obedient servant,

WM. TURNBULL, M.D.

Physician to the Huddersfield Infirmary.

EFFECTS OF TREATMENT ON THE MORTALITY IN CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

I HAVE observed a quotation from the Medical Gazette, in the Times, in which it is stated, that though at different places the mortality from cholera has been very different, that such difference was in no way attributable to difference of treatment. Whatever may have been your source of information, you may rely on this:—

At the village of —, situated on the coast, about eleven or twelve miles

from Newcastle, up to the early part of last week there had occurred thirty-six cases, of which thirty-five died. The practitioner is a middle-aged man, ignorant, and very often drunk. In a neighbouring village and colliery there had occurred about sixty cases where the patients were carefully attended to; not a single death occurred. This requires no comment from me.

Yours, &c.

J. W. EARLE.

14, Old Broad-Street.

N.B.—There are also three men treating cholera in the same neighbourhood, who are neither members of the Apothecaries' Company nor of the College of Surgeons. I could give their names.

CARRIAGE FOR CHOLERA PATIENTS.

To the Editor of the London Medical Gazette.

SIR,

I HAVE sent you two drawings of carriages invented by my friend, George Glover, Esq. surgeon, which I think will be instrumental in saving many lives should the cholera spread to any extent in the metropolis. In the interior, the patient lies on a moveable

couch, which is kept warm by a mattress of heated salt. The salt is kept heated at each of the station-houses, by means of a sand-bath, and, upon information being given, the hot salt is put into the mattress and covered with blankets, and the carriage is immediately driven off to take up the patient. The advantages of such a conveyance are, 1st. The curative process commences the instant the patient is put into the carriage. 2dly. A great deal of time is saved, which can be put to the utmost advantage in rescuing the life of the patient. 3dly. The patients may be driven to the hospitals so speedily that the hospitals may be less numerous and at a greater distance from each other, and removed from a crowded part of the town to a more wholesome locality; so that the medical attendants, in this way, will be less exposed to contagion. It will give me great pleasure to see them copied in your next Gazette, and I remain

Your obedient servant,

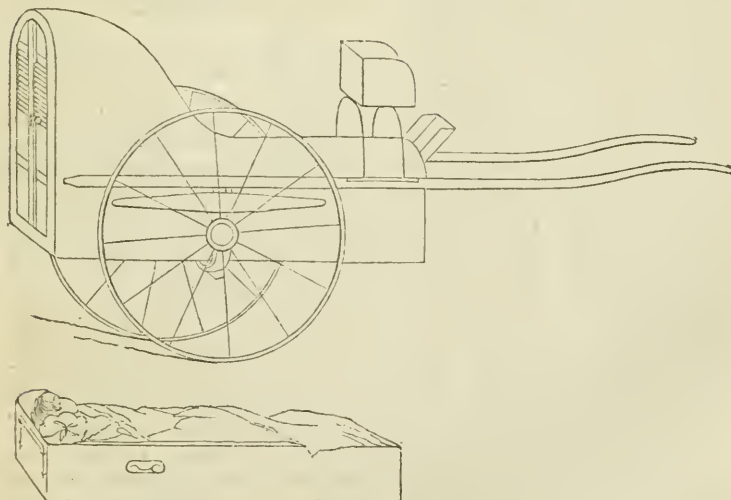
J. KNAPP.

February 20, 1832.

Last Report, Edinburgh. — Total cases, 24.—Deaths, 15.—Cures, 6.—Remaining, 3.

Musselburgh. — Total cases, 432.—Deaths, 191.—Cures, 233.

Feb. 20th. — No new cases. Death, 1. Cure, 1.



The length of the carriage is nearly seven feet, and the litter rather more than six feet. The body of the vehi-

cle ought not to be more than sixteen or eighteen inches from the ground.

CASE OF CHOLERA TREATED BY
MEANS OF GALVANISM.

*To the Editor of the London Medical
Gazette.*

SIR,

GALVANISM was used for the first time in Edinburgh on Thursday, 16th February. I am sorry to inform you that in both cases it was unsuccessful. In one of the cases it was only applied for ten minutes; indeed the man was all but moribund when they applied it. The other case I shall send you an abridged account of, with the autopsy.

Jenna Stewart, æt. 25, was admitted February 16th, at half-past twelve, P.M. She had been sleeping with a woman convalescent from the cholera, and was attacked this morning, at twelve o'clock, with vomiting and purging. Has been a week complaining of general languor, with tendency to diarrhoea, and has frequently been intoxicated during this period. She was admitted at half-past twelve, and at this time was suffering from cramps in lower extremities; the surface was cold, the pulse imperceptible, and the extremities were livid and shrivelled, especially the superior. Breath warm; tongue rather altered in appearance, but not diminished in temperature. Eyes depressed, and surrounded by a livid areola. The lividity of the face increased considerably an hour after her admission, but in a great degree disappeared prior to death.

Treatment.—Sumat quamprimum haustum emeticum cum pulvere Sinap. cochl. duabus. Mittatur sanguis. Eight ounces of blood were with great difficulty abstracted, after a vein in each arm had been opened. Blood drawn dark coloured, and, on coagulating, little or no serum was seen. She was ordered a stimulating draught of landanum, and sulphuric ether, &c. This appeared to have no effect. The emetic was again given, which produced vomiting, but no reaction. Galvanism was then applied assiduously for two hours, but without effect. There were 32 plates, about four inches in diameter, in each trough. One wire was applied to the stomach, moistened at its extremity with a solution of salt and water, and the other to the spine. I saw the galvanism applied for about ten minutes, and, though friction with

oil of turpentine was simultaneously used, the only effect produced was a slight motion of the upper eyelid, and I understood a small quantity of blood once issued from one of the open orifices of the punctured veins. She died three hours and a quarter after admission—i. e. in three hours and three quarters from the commencement of the attack.

Autopsy.—The arterial system was in a state of congestion. The thoracic and abdominal aorta, with their branches, were gorged with dark-coloured blood. The left ventricle was contracted and contained air, as also the thoracic aorta and vena portæ. The right ventricle was natural in appearance, and empty. Nothing unusual was seen in the spinal cord, or in its membranes. The mucous membrane of the stomach was paler than natural, and that of the intestines not altered or softened. The gall-bladder was partially filled with bile of the usual colour, and the liver was congested with blood.

If you think the above cases worthy of a place in your next Gazette, they are entirely at your service, and

I remain,

Your obedient servant,

J. KNAPP.

Edinburgh, February 25, 1832.

STATE OF THE INTESTINES IN
CHOLERA.

*To the Editor of the London Medical
Gazette.*

SIR,

I BEG leave to send, for your inspection, a portion of the duodenum, including the head of the pancreas, taken from the case of cholera which occurred in this parish last week, and of which a postmortem examination was made in presence of Sir William Russell and a deputation, including Dr. Young, from St. Marylebone Board of Health. You will perceive that the isolated mucous glands are remarkably developed, being about as large as mustard seeds, rather more opaque than the surrounding intestine, and disseminated, in parts, to the number of from six to twelve in the space of a square inch. The whole mucous surface, though at present rather pink from exposure to the air, was of an unusually pale straw-coloured white, and did not present the slightest vascularity, or other vestige of

inflammation. The glandular development prevailed throughout the duodenum and two-thirds of the jejunum, and reappeared in the colon.

Accompanying the specimen is a drawing, which I made from a corresponding portion of the canal, taken from one of the sufferers by the Clapham cholera two (?) years ago, and obligingly sent to me by Dr. Chambers, who, with Dr. Latham, was in attendance on that occasion.

On comparing the two, you will perceive that the morbid alterations are identical.

As, after very numerous and minute examinations of the alimentary canal, I have only met with the alteration in question in the two instances alluded to; as, on making inquiry amongst my medical friends, I find, that according to their observations in this country, they consider it equally rare; as Sir William Russell assures me that he has frequently found it in the cholera of India; as Dupuytren has noticed it in sporadic cholera, and in that only; and as some of the old authors, particularly the German, have described it in their histories of epidemics attended with *diarrhœa mucosa*, it perhaps becomes a pathological question of interest and importance whether the morbid alteration in question is not *peculiar* to that species of intestinal irritation which accompanies the more aggravated forms of cholera.

Should such appear to be the fact, it would countenance the idea that the unusual disease at present prevailing in the metropolis, is not *necessarily* an imported affection; since, in the Marylebone case, at least, it was identical in its symptoms and postmortem appearances with the Clapham cholera, which indubitably originated in a local cause—the effluvium from an opened cess-pool.

I am, Sir,

Your obedient servant.

J. HOPE.

Marylebone, 13, Lower Seymour-Street,
February 27th, 1832.

CHOLERA IN MARYLEBONE.

To the Editor of the London Medical Gazette.

SIR,

I DID not send you, last week, the case of the boy in James-Street, who had

just died under my care, of cholera, because, as the details had been sent to the Central Board, I was not disposed, by further notice of the case, to swell the cry of "*mad dog*;" which, as applicable to this disease, there is but too much reason to say has been unnecessarily raised, and is but too much kept up by those who, in their wild attempts to subdue it, only re-echo the alarm.

That the case, however, may not be turned, by misrepresentation, into one got up for the support of *any* peculiar doctrines, and may have whatever value in the elucidation of truth it possesses, properly authenticated, I am induced to send you the following observations on what I hear has been said of it, and to subjoin some particulars of the case.

1st. It has been attempted to make out the affection to be a chronic disease of, I think, *two years'* standing. It is just so far true that the boy was ill in the beginning of last year, and that he had afterwards "*bad eyes*;" but for *four* months he had been well. He had no symptoms of chronic disease when I saw him, and not a trace of any appeared on dissection.

2dly. It has been reported that the medical attendants had held this case up as a clear case of infection, through the parents having been in the Borough. So far is this from the truth, that, though the parents passed both the preceding Sundays there, it was the particular request of Sir Wm. Russell that this should not be mentioned in reporting the case, lest it should appear the fact was unnecessarily pressed forward to support the doctrine of contagion.

3dly.—It has been urged, that my having omitted to summon my colleague to view this case, made my report of it suspicious. Had I been in the way of asking his assistance, this could scarcely have been a fair deduction from the fact; but, not having done so in other cases, it is surely by no means strange that, in the few hours which elapsed between the time of my seeing the boy and his death, I should be more occupied in obtaining the assistance of those who had seen the disease, and be satisfied with taking all the friends to see the case who accidentally fell in my way.

4thly. It has been asserted that post-mortem inspection was made in secret. This is equally true with the preceding statements—there having been *nine* medical men there, more than one-half

of whom came prepossessed against its being a case of cholera, but went away, I think I may say, satisfied from what they saw, that it had been so.

5thly. It has been held, that as contagion could not be traced, either as a cause or consequence of this case, it must be admitted by those who believe in the propagation of cholera by that means, either that this was not a case of the disease, or that it arose from another cause. A complete *non-sequitur*—as, though contagion cannot in many cases be traced, we do not doubt its having been the exciting cause; while, as its probable consequence in this case, it yet remains to be told that every member, but one, of this boy's family have since had bowel complaints, which, though arrested by the ordinary treatment, may have been the first stage of the same disease.

The case was shortly this:—

Charles Connell, about four years of age, living with his parents and three brothers and sisters, in a small back-room on the second floor, in No. 28, James-Street, Manchester-Square, was, for about ten days preceding my seeing him on the 22d February, at 2 P.M., affected with bowel complaint; which, after going to bed in good spirits on the preceding evening, suffered a sudden increase between one and two in the morning, when vomiting of a pale thin fluid also came on, attended with great pain and drawing up of the limbs; and both continued, at intervals, till seven in the morning, when they ceased, and his extremities became colder—of a decided blue colour; the countenance oppressed; the eyes sunk, and only half shut, and the child could with difficulty be roused to attention.

All of these symptoms had been increasing, and were in existence at the time of my visit; and, in addition, I remarked the tongue covered with a yellowish brown crust; the præcordia tumid, and uneasy when pressed; the respiration oppressed and hurried; the heart's beats quick and unequal; and that the pulse at the wrist had sunk to a mere flutter, and could at times not be felt at all.

There had been no vomiting since seven o'clock; and the mother's impression was, there had been no purging, while, as she thought, the urine had passed frequently without consciousness; but as, on examining some of the

wet things taken from under the boy, they were strewed with flakes of a dirty pale slime, it seems more probable the wet came from the bowels.

I ordered the assiduous application of external heat and friction, gave him calomel and opium in repeated small doses—in the intervals a warm cardiac mixture, containing rhubarb and magnesia—and finally endeavoured to bring about reaction by the use of wine; but all to no effect; the child became more and more unconscious—the breathing more and more oppressed—the circulating action less distinct—the face and hands more discoloured—the nails at last looking as if stained by ink—and the temperature throughout gradually sinking, till about seven P.M., when he could no longer be said to live. I leave the description of the morbid appearances to one of those present, and am, sir,

Your obedient servant,

J. BARTLET, M.D.

MEDICAL GAZETTE.

Saturday, March 3, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

ANOMALOUS CONSTITUTION OF THE "CENTRAL BOARD."

THE events of the past week must have tended to remove the veil from the eyes of all but the wilfully blind—those who stand committed by the over-confidence with which they expressed their opinions that the malady which prevails in London was not the cholera—and those who find themselves in the still more awkward dilemma, of having recorded, in their prophetic wisdom, the flattering but fallacious assurance, that the scourge of the East would not shew itself in the "happy climate!" of England, or appearing at all, would at least assume a very subdued and mitigated aspect.

How far a meretricious press, interested in concealment, may prostitute itself for lucre, and how far a credulous public, ready of persuasion where it

is anxious to believe, may be misled, it would perhaps be foreign to the objects of a scientific journal minutely to inquire; but as regards our own profession, we hold the public conduct of its members to be fit subject for our notice, and the fault rests not with us if such notice must in the present instance be accompanied with animadversion. Candour compels us to acknowledge, that if the charges of interested and unworthy motives be lavishly made against us by the public, these accusations receive but too much countenance from the intemperate clamour raised by some of our own body on the first announcement of the cholera in London; and even now that the existence of *something* is placed beyond all doubt, it is mortifying to observe the disingenuous efforts made by the same parties to hoodwink the simple, by maintaining that this something—be it what it may—is not, and cannot be, cholera. Instead of co-operating in exertions for the general welfare in the hour of calamity, they think only of their own petty interests—how the fallacy of their boasted prognostications may be concealed, and their own poor reputations bolstered up a little longer. They trust to the daily press, which has debased itself by becoming the vehicle of their puffing, and defrauded the revenue by giving insertion to their advertisements in such a shape as to avoid paying the duty; but they trust to a fickle and a worthless prop—the press is already trimming, and will probably soon treat them with the contempt which it already feels that they deserve. Of the only portion of the medical press which has joined in this disgraceful outrage upon common sense, it would be easy to shew the imbecility,—but it is useless; for ere the words which we pen shall have reached our more distant readers, the production to which they would apply, like the reeling braggart who boasts as long as he can arti-

culate, shall have sunk into oblivion, and this passing expression of our contempt remain almost the only record of its existence.

Turning from these general observations to the questions which the presence of a malignant disease among us naturally suggest, we have to remind our readers that, in describing the cholera and anti-cholera parties in London, we stated our impression that both were deficient in candour, and that neither enjoyed the confidence either of the profession or the public. The one maintain, either that no disease, except what is customary, exists, or at all events, that the only extraordinary circumstance consists in the degree, not in the nature of the malady: the other hold, not only that the disease is the Asiatic cholera, but that its appearance is absolutely recent, and is to be dated from the occurrence of a particular case which they specify. The want of any thing like a temperate and rational investigation of the subject by the former party, we have already illustrated; and had it been otherwise, events of recent occurrence (we allude to the progressive increase, and to the great mortality of the disease) unfortunately render their hypothesis too palpably fallacious to require any farther refutation.

It is principally, therefore, of the other party which we purpose at present to speak; and it is our object to inquire why, with truth on their side—at least as to the main subject of debate—they have failed to acquire general confidence, and owe whatever hold they may have upon the public mind rather to the weakness of their enemies than to their own strength. It will, we presume, be apparent that we allude to the Central Board of Health; and we believe that we are expressing the general opinion, in offering our own—that it does not, and as at present constituted, never can, command the degree of confidence which

is absolutely necessary to carrying its recommendations into effect, even supposing these to be in themselves unobjectionable. If any apology for our remarks be necessary, we may use the common one of all journalists, when they are going to be censorious, and say that we are "nothing, if not critical." In the prosecution of our vocation, then, we shall take leave to point out the circumstances which, in our estimation, have prevented the Board from meeting with that cordial support which would have been so desirable in an emergency like the present.

Now, the first point to be noticed, is one at which we formerly hinted—we mean, the elements of which this body is composed. The circumstance in which the Central Board is most conspicuously ill-contrived is as to the number of medical men whom it contains: these are three only—all contagionists, all military, all unacquainted with the diseases of the lower orders in this and the other large cities of the empire. A due proportion of contagionists would have been of advantage, to secure the requisite precaution, and an admixture of army-surgeons, to organize the machinery necessary to co-operation, would have been desirable; but it is quite a mistake to imagine that civil practitioners will submit, with a good grace or a docile spirit, to the governance of men, however well-qualified, who unanimously entertain opinions at variance with those of a large proportion of their brethren, and none of whom belong to their own "order." This evil we formerly pointed out, and it is obvious, from the strong measures just adopted to procure returns of the sick, that it has both a real and an enduring existence; nor can it ever be abated till a certain number of civil practitioners take their seats at the Board. Since our former articles were written, we have perused a

paper by Dr. Christison, of Edinburgh, containing opinions so analogous to our own that we cannot resist the temptation to quote them.

"The medical profession, whose good opinion it is of the greatest moment to court and secure in the present emergency, had a right to expect that, in a body intended to guide their opinions, and perhaps even dictate their operations, they should be represented by men whose names and reputation would at once, and without a murmur, command general respect. I may appeal unhesitatingly to my professional brethren to say, whether such is the present composition of the Central Board. Without wishing to appear, in the least degree, desirous of calling in question the respective merits of its members in their several spheres, I may ask, through what peculiar merit, as shown by their professional career, they have been thought *alone* worthy of being placed in so prominent and responsible a situation? Why, I repeat, was not a selection added to their number from among the many celebrated practitioners of the metropolis—of seniors, to give weight and authority; and of juniors, to impart spirit to their proceedings?"

This appears to us to be precisely what is wanted; and we would not only have some of those who occupy prominent places in public estimation, but representatives besides, from each district of the town, consisting of hospital and dispensary physicians; the latter especially would be found extremely useful, as familiar with the condition of the poor and the localities of disease.

The members of the Central Board seem to have been conscious of their weakness; at least this is the most natural way of explaining the want of moral courage which they have displayed. It will be remembered that a long period elapsed, after the cholera had appeared in the north of England, before any kind of information beyond the numerical returns was afforded to the public. The true explanation of this, it now fully appears, is to be sought,

not in any desire to conceal useful observations, but in having none to communicate;—nevertheless, a general cry for information having been raised, it was at length resolved to send forth what is called the Cholera Gazette, the contents of which certainly have afforded an ample apology for the backwardness of the Board in bringing them before the public. But ere we proceed farther, we must direct attention to the plan of the work in question. The former Board, like other bodies of a similar kind, had published certain reports made to them, together with the opinions, inferences, and recommendations which, in the estimation of the members, these data warranted. But the Central Board have adopted quite a different method, and have issued a non-descript in literature, consisting of certain papers by others, for which they hold themselves responsible, and of certain observations made under their sanction, and of course containing their opinions, but for which they do *not* hold themselves responsible. This ingenious combination of acknowledged control and disavowed responsibility, has been ostensibly effected by the employment of another party to edit their publication. How far any one so situated can be looked upon otherwise than as the mouth-piece of the Board, we leave it for our readers to decide; merely remarking, that the few observations which have hitherto been made on quarantine and certain other points, are purely responsive to the doctrines which emanate from the Council Office. Thus we have the singular anomaly of a public body sending forth, "*by authority*," certain official papers, together with sundry observations in approval of their measures thereunto appended; but of which tribute to their own merits, be it remembered, the Board expressly tell us they are to be held entirely innocent*.

On the publication alluded to making its appearance, the Lancet commenced a furious attack upon the Central Board, accusing them of "imposture," "fraud,"—"obtuseness of intellect,"—"perversity of prejudice," and "truly scandalous proceedings*." At the time when these complimentary observations were published, our *respected* contemporary, suspecting that the Central Board had fixed upon the gentleman to conduct their publication who was known to have been employed by him to write the articles on cholera for the Lancet, naturally apprehended that he was about to lose a valuable coadjutor. When this furious philippic against the Central Board appeared, it is evident that the writer did not fully appreciate the liberality of its members, or contemplate even the possibility of the same person being suffered to conduct the Cholera Gazette and the corresponding department of the Lancet. A little time, however, served to shew the injustice he offered them by this supposition; and, in the very next allusion to the subject, the language of the bully was laid aside—a totally different view prevailed—and the Central Board was spoken of with respect, while the tone of commendation has waxed warmer in each succeeding number, till it has become so apparent as to force itself upon the notice of all, that the Cholera Gazette and the Lancet are identical in sentiment and in style—the same data being alluded to in both, the same inferences deduced from them, and expressed nearly in the same language. Similarity so striking of facts, of opinions, and of diction, can only receive one rational explanation—viz. that they are the product of one pen. Another little fact, of importance only as shewing the liberality of the Board, is, that the gentleman to whom these articles are attributed has been appointed inspector of one of the

* See Cholera Gazette, No. I.

* Lancet, January 21, p. 595.

districts of the metropolis. In alluding to these circumstances there is one point with regard to which we are desirous not to be misunderstood: it is, that we do not entertain, or wish to convey, any impression unfavourable to the gentleman alluded to, in consequence of the double duties he has undertaken; indeed, it is but justice to remark, that his contributions to our contemporary are easily distinguishable by their entire freedom from any display of that violent and vindictive spirit which pervades the compositions of the editor *par excellence*. It is simply and exclusively as illustrative of the effect which this coincidence has had with regard to the Board of Health that we have adverted to the circumstance; for the result has been, that the whole of what the *Lancet* has said, and may say, in their favour, is looked upon with suspicion, being, as it is, but a development and illustration of opinions more concisely stated in their own paper:

As an illustration of the doctrines peculiar to the above publications, and we must say, in our opinion, indicative of a want of candour, we may mention that which fixes the date of cholera in London to its appearance in John James, who died on the 8th ult., and which holds that Daniel Barber, who expired on the 7th, after an illness of seventeen hours (*with blue skin, senile countenance, cold breath, imperceptible pulse, spasms, suppressed urine, and vomiting and purging of watery fluids*)*, was cut off by some other disease. As now stated, though it is done with perfect fairness, this opinion appears little short of ridiculous; nor is this impression much diminished by the grounds assigned for it, which are merely that the post-mortem appearances were not identical with those described in the *Cholera Gazette* as "invariably present." This is, indeed, to

split hairs; and, in fact, a reference to the best authors will shew that the appearances, though wonderfully alike, are not always precisely the same, but vary with the duration of the illness which has proved fatal, the length of time which has intervened between death and the pathological examination, and other circumstances. We believe to perfect moral conviction, that if the Board had acknowledged the existence of cholera in London at the time the cases of Barber, Sullivan, or Webb, were reported, they would not have hesitated to call them by that name; and we therefore regard the contrary doctrine as a mere piece of special pleading on the part of the *Cholera Gazette*, and its echo, the *Lancet*.

To us it appears that there is much unnecessary refinement, if not something of affectation, in this attempted precision, and this claim to superiority of discrimination. Cases, such as hereafter will receive the name of cholera, have unquestionably been for some time showing themselves in London, though, perhaps, neither in such numbers, nor so exquisitely marked, as now; and to deny this appears to us just as uncandid, as it is to maintain that there is nothing new in the present visitation. The epidemic may, and, we think, does, come under the description given by some of the older writers, and it is just possible that sporadic cases approaching to it may have been witnessed, as in the singular malady which occurred a few years ago at Clapham, from opening a cess-pool: but when we hear gentlemen gravely maintaining that it is nothing more than they have been accustomed to see in London, we do as Montagne did, in recording a story which he did not quite believe—we "charge it to the conscience of him by whom it is related."

These circumstances, then—the radical defect in its constitution—the manifest

* See *Cholera Gazette*, No. 3.

indiscretion evinced in some of its acts, and something like disingenuousness in others, have contributed greatly to lessen the confidence with which the public might otherwise have received the announcements emanating from a Board of Health differently conducted; and have led to a spirit of opposition even to their most eligible measures, so that, in fact, nothing worth mentioning has been done towards arresting the progress of the malady. In consequence of these impressions, a public meeting of medical practitioners, it is said, is to be called together (see a letter signed Londinensis, at page 838), with a view of inquiring into the present state of the metropolis; but from such proceeding, if it take place, we apprehend that little benefit will accrue, the recent system of quackery to which the occasion has given rise having rendered the respectable part of the profession unwilling to interfere. The only remedy that suggests itself to us, is the immediate addition to the Board of Health of ten or twelve hospital and dispensary physicians of established reputation, whose names would have that weight with the profession and the public which the present Board does not possess, and, as now constituted, never can obtain.

CHOLERA VARIETIES.

WE have learnt various detached circumstances connected with cholera, which are perhaps of sufficient interest to be recorded, though scarcely of importance enough to form separate articles.

Cholera Hospitals.—Among these, that in St. Giles's, under the direction of Dr. R. Pinckard, is one of the most complete and efficiently got up of those which have as yet been prepared. The arrangements are exceedingly good, and the officers most zealous. We also

hear very favourably of the one opposite Bedlam.

Skilful Prognosis.—A medical man—of course a non-believer in cholera—visited the St. Giles's hospital two days ago, where he found a man and a woman, the latter convalescent, having recovered under the use of a mustard emetic, followed by stimulants simultaneously with external heat. The woman, he said, “had nothing the matter with her, and as to the man, the same would very soon be the case with him.” The former opinion was nearly true, and the latter proved entirely so, for in ten minutes after this happy prognosis had been given, the patient was dead.

Injections into the Veins.—Delpech, who has been in Scotland for some time, has tried the effect of injecting a solution of opium into the veins in cholera. It has not been attended with any benefit. It will be perceived by the letter of our indefatigable correspondent in that city that galvanism has also failed.

Contagion?—Our readers are aware that two cases of cholera occurred in St. Pancras last week: one of the bearers employed in carrying the bodies to the grave was seized with the disease on Wednesday. In the majority of cases, so far as our information at present extends, it has not been found possible to trace any chain of connexion between the patients; but it is to be kept in mind that this often happens in diseases acknowledged to be contagious, —as small-pox.

Choleroïd Cases.—Many cases of something like cholera are to be met with. All the family, save one, of the boy who died in Marylebone have had diarrhœa. A physician informed us that he went down the river a few days ago to the floating hospital. All the cholera patients were dead. He returned also by water, the day being cold and

raw. He was seized soon after with a peculiar and very unpleasant sensation in the chest, implicating the breathing and circulation. The heart's action was so enfeebled that it could scarcely be perceived, and the pulse was so weak and fluttering that it could with difficulty be felt at all. He was in this state at the College meeting on Monday evening, without the appearance of any moral impression to account for the phenomenon. Dr. Burne said at the Medical Society the other evening that he doubted whether any one who had visited a cholera patient could sleep next night for spasms, and evidently spoke from personal experience. Are these circumstances to be attributed to malaria, infection, or moral causes?

Mortality from Cholera.—We have no doubt that the rate of mortality in London is very much over-estimated, because many cases wherein recovery has occurred have not been reported at all. Nay, more, it is a curious fact that up to the end of last week the number of deaths within the bills of mortality were under that which preceded. During the week ending February 14th, the deaths were 479; and during that ending February 20th, only 385, being a decrease of 94. At Rotherhithe the deaths last week amounted only to 7; but the week before they had been 14; the average number is 12. We have heard some drawing conclusions from the above, but this is rather premature.

Mortality among Horses.—At Limehouse there is a large brewery, (Taylor's, we believe) at which not fewer than twelve horses lately died of some rapid but obscure febrile disease, in the immediate vicinity, and within a few days after the cases of cholera which have been recorded made their appearance.

Cholera at Clapham in 1828.—Our readers will find in our first volume an account by Dr. Chambers of an

irruption of cholera in a school at Clapham, apparently from opening a cess-pool. Twenty boys were taken ill; one died in three hours, and another in twenty-four. We have a drawing of the appearances found in the intestines after death; they are peculiar, and identical with those found in the boy who died a few days ago of cholera in James Street. The drawing of the duodenum from the boy at Clapham might serve for that of the child in Marylebone, (see the letters of Dr. Bartlett and Dr. Hope in the present number.)

Order to Report Cases.—The following order has been issued by the Privy Council. After continuing the Boards of Health at present constituted, it is added, "And it is further ordered, that every practitioner of medicine within every city, town, or district, in which every such Board of Health is, or shall be constituted, by order of the Lords, and others of His Majesty's Privy Council, shall, and he is hereby required and commanded, to make to such Board a daily report under his hand, containing a numerical report of all cases, deaths, and recoveries, of every person attended by such medical practitioner who may be affected with the said disease, or with any other disease anywise resembling the same. And all medical practitioners who shall neglect, or omit to make any such return at the time, or in the manner or form required by the Board of Health of the city, town, or district in which they reside, or in which the patient they attend resides, or who shall, in such return, wilfully make any false statement, are hereby warned and admonished, that the penalties and punishments consequent upon any such disobedience to this order, and to the provisions of the before-mentioned act of parliament, will forthwith be enforced against them."

We presume that the case of Colonel Evans, who presented what has been

called (though not quite fairly) the no-cholera petition, will be reported. He was seized on Monday night with symptoms very much "resembling the same." If he had fallen down at St. Stephen's, instead of the Athenæum, the "sense" of the House would very speedily have been taken on the subject of contagion. We suspect, he would have been in the minority.

LONDON UNIVERSITY.

THE pupils of the Medical School at this Institution gave a public dinner to their Professors, at the Freemason's Tavern, on the 24th ult. when about 130 gentlemen were present. Mr. Eisdell took the Chair. The usual routine of toasts was gone through, and some rather hyperbolical displays of oratory made,—but which we cannot afford space to record.

COLLEGE OF PHYSICIANS.

Monday, Feb. 27, 1832.

SIR H. HALFORD, BART., PRESIDENT, IN THE CHAIR.

A VERY brilliant assemblage met to grace this, the first evening meeting of the season. Among the distinguished visitors present, we noticed the Archbishop of Canterbury, the Bishop of London, Lord Westmoreland, the Dean of Westminster, the Vice-Chancellor, &c.

The President, as soon as the company were arranged, rose and said, that in pursuing the plan which had been adopted on former seasons, of reading on these evenings papers which tended to adorn medicine with literature, he was proud of being able to introduce, as an associate of their classic toils, a dignitary of the church, who had favoured them on the present occasion with a learned paper. Sir Henry then proceeded to read—

Some Observations on the Plague of Athens; with a Comparison of that

Malady with the Plague of the Levant. By DR. IRELAND, Dean of Westminster.

This dissertation, of which we shall give an account in an early number, was received with much applause; and on the motion of the President, thanks were voted to the very reverend author.

WESTMINSTER MEDICAL SOCIETY.

To the Editor of the London Medical Gazette.

SIR,

THIS doubt about cholera or no cholera is to me more tormenting than the disease itself. It was with this impression that I determined to avail myself of the first opportunity that offered of procuring the most ample and able information on the subject. So seeing an advertisement from the Westminster Society in a *Times* of last week, I rode up to town on Saturday, and just reached the old scene of some of my former troubles—the Hunterian school, in time for the commencement of the discussion; indeed, I had not a moment to spare for changing my dress; and full booted and spurred as I was, I ventured to enter the museum—a confession which I humbly hope will serve for my apology for intruding so cavalierly among so polite an audience. Well, sir, but to the point. I listened to Dr. Epps (who was on his legs when I went in) with considerable attention. He went on very fluently and confidently; but I am sorry to say, I can put but little faith in his account of the appearance of cholera in London. Dirt, destitution, and dram-drinking, may make a very pretty alliteration, but they will not prove to be matters of fact when summoned up to form a groundwork for the present novel phenomenon—novel, certainly it is, whether a *nova pestis* or not; for I would merely ask, was there ever an epidemic known in this part of the world more distinctly marked, or so uniformly severe? As for the supposed dirt and destitution of the infected districts, those who know these localities well, can vouch for their never having been *more* cleanly nor comfortable within the memory of man: much *less* they undoubtedly have often been.

I am myself, sir, rather "unaccustomed to public speaking," or I should have risen to reply to the gentleman "who had just sat down." However, I believe if I had done so, I should not have succeeded in getting a hearing; for up rose, as by a cue, another speaker on the same side—Dr. Webster.

A foreign gentleman, whose name I could not learn, next proceeded to read some cases and postmortem examinations which he had noted at one of the cholera stations; but the detail appearing likely to be lengthy, the society was growing impatient, and the chairman was about to cut short the foreign Dr. when a voice, with a strong peculiar intonation, uttered oracularly, "that's an important case," and the narrator was permitted to proceed.

When he had done, a Dr. Sigmond, who seems to be persuaded that there is nothing new under the sun, and that Hippocrates, and Aretæus, and Sydenham, were intimately acquainted with the Anglo-Indian cholera, rose to account for the origin of the complaint in one of the cases just related by the foreign member. The patient, he said, with great gravity, was "one of those unfortunate females who live dependent on the public caprice," (a periphrasis which, by the way, excited a general smile): this woman was actually in a state of starvation—had eaten nothing but potatoes for some time—and for the week preceding her death had had nothing to subsist on except a penny loaf, which the poor creature shared with her husband. But, unfortunately for Dr. Sigmond's pathetic story, a Mr. Hooper completely spoiled it. He told the society that *he* had dissected the woman in question, and so far from presenting any appearances of starvation, she had a thick layer of fat on the muscles of her abdomen—was plump generally—not even was her visage contracted, and in her stomach and bowels were found a quantity of raisin-stones.

I had much curiosity, sir, to hear Dr. James Johnson giving his views of the present state of things, with reference to the disease in London. I had never heard—though, to be sure, I had often heard of, this gentleman before; and was not a little astonished to find that his was the nasal intonation which I have just now alluded to. The opinions of Dr. Johnson I was already acquainted

with, especially his notions about "contingent contagion," and his "earthy exhalations," and his assertions of "how he had seen this very cholera in India seven and twenty years ago," &c. All this I had picked up from a series of letters of his (but rather abruptly terminated) in the *Courier*; and it was, I can assure you, sir, with no small satisfaction that I now found myself in presence of this "eminent authority." It was not long, however, until I perceived that he had a curious summary mode of proceeding in an argument. He dealt wholly in cool announcements, and dogmatic statements, backed with certain pledges of his veracity, which seemed to be extremely ridiculous, but exceedingly familiar in his mouth. *He* had visited most of the cases—I am not sure but he said *all*—now in London, and "*upon my honour*," said the Doctor, "I could not discover in them any foreign or unusual symptom." He had been in India, he reminded the society, seven and twenty years ago, (and "practised there by sea," as an old gentleman added presently after) and comparing the disease in London with what he had seen in India; "*upon my word and honour*," exclaimed the Doctor, this is not the Indian cholera." And with respect to the contagiousness of this London cholera, *he* had examined very carefully into the proofs for and against it, and "*so help me God*," swore the Doctor, "I have not been able to trace out, in connexion with it, a single case of contagion!"

This was, no doubt, in the highest degree convincing, and seemed to settle the question to the satisfaction of many who were present. But I must confess I was far from being prepared to find so lively a representative of my lord Peter in the respectable editor of the *Medico-Chirurgical Review*.

I cannot say, sir, upon the whole, that I am much satisfied with myself for having come to town upon such a wild-goose chase; though I should not be surprised if I came up again for the adjourned discussion on Saturday night next. Another trial may be more tolerable. I make bold, meantime, to send you these hasty memoranda, as you formerly, about the beginning of the season, did me the favour of inserting some other notes of mine, and as I infer from the remarks in your last num-

ber that you do not intend to give regular reports of the proceedings of which I have here attempted to supply a sketch.

Believe me, ever,

Your very obedient servant,
RUSTICUS.

February 27th, 1832.

ANATOMY BILL.

To the Editor of the London Medical Gazette.

SIR,

THE great respectability of your correspondents, Mr. Todd and Dr. Wayte, induces me to address you once more on this subject. It appears that these gentlemen take a violent objection to my proposal, to make anatomy legal only in licensed schools.

The special object for legislation in the matter before us, is the protection of the public; first, from the hand of the assassin; secondly, from that of the uneducated practitioner of surgery.

There are many things so desirable in an abstract view, that a compromise appears inadmissible: hence it may seem to those who know its importance, that the pursuit of anatomy should be without constraint or limit; but this, I maintain, is incompatible with the public safety. Such an arrangement would render impracticable the main provision of the bill—the employment of inspectors. Suppose any, or every medical man at liberty to practise dissection in his own house, what becomes of this safeguard? After what has recently passed, no medical man would be safe from the insults of the populace, placed in the very sinister position of attending a patient, obtaining possession of his body, whether claimed or unclaimed, and then privately dissecting it, without the intervention of a formal certificate, and a special register of it, by a responsible officer. But I am still of opinion that the objection is hypothetical—that not five in five hundred would dissect at home, were every facility granted. It is said that surgeons in general practice, and remote situations, are so seldom called upon to operate, that therefore their anatomy needs refreshing. This argument tells both ways; for where

occasions are few, preparation to meet them is indolent in proportion; and, in the present era of society, that which is of frequent requisition is always easy of attainment. But when these gentlemen speak of cases which admit of no delay, and offer no alternative, such as strangulated hernia, &c. they commit a strange oversight; in their zeal for anatomy they overlook surgery, or the use of anatomy; it is not a knowledge of parts only, but of the mode of performing operations, derived from the habitual practice of them, that is required; it is the latter more than the former attainment of which men so situated stand in need. Anatomy, it is presumed, they have once learned; operative surgery they have had no opportunity of learning. But if their anatomy be at fault, and they are anxious to refresh it, do not postmortem inspections afford excellent opportunities? They are not in any way affected by the licenses. These investigations of one region at a time, may be as minute, and as frequent, as their engagements permit; and I will venture to add most beneficial, without inroad upon professional duties, or the feelings of society. If, indeed, their actual knowledge of parts be so deficient as to require, for conscience as well as reputation's sake, that they should return to school, it may be an individual hardship, but it is a public duty, which they neither can nor ought to resist. In short, the strongest argument for licensed schools is furnished by these very complainants, since the resort to them in the first instance, for solid and well-methodized instruction, will be a paramount obligation, and men will not settle in practice content to pick up their anatomy by hook or by crook as they go along.

I yield to no man in the strength of my conviction of the indispensableness of anatomy to a surgeon; but it should be taught by competent persons, in properly appointed places, and conducted with such observances as its importance, the dignity of its objects, the preservation of public safety, and professional respectability, demand. No honest votary of the profession will object to pay the price that such stipulations exact. As to the hard epithets, "monopolizing," "narrow-minded," "illiberal," &c. not meaning to appropriate, I do not notice them.

I have now done with the subject. My motives in venturing to offer my opinion were—first, a sincere belief that the bill now before Parliament is no security against crime; secondly, an unwillingness to lose a fair opportunity of setting the character and claims of the profession before the public in their true light.

So far from being an exclusionist in principle, or objecting to a multiplicity of schools, providing always for their due qualification, I should hail the day with pleasure, when every principal market town in the kingdom had its anatomical as well as its grammar school.

I am, sir,
Your obedient servant,
B. TRAVERS.

Bruton-Street, Feb. 29, 1832.

COPY OF THE PETITION,

Signed by 73 Medical Men, and presented to the House of Commons by Colonel Evans, on Thursday the 22d of February.

THE humble Petition of the undersigned Physicians and Surgeons, residing in London, sheweth,

That we feel it to be our duty respectfully to approach your honourable House, to pray that an inquiry may be instituted, for the purpose of ascertaining the nature and mode of propagation of a disease said to be new in this country, and which at present causes so much alarm in the public mind.

To the Editor of the London Medical Gazette.

SIR,

As considerable misapprehension exists in the profession, as well as in the public mind, regarding the prayer of the Petition to the House of Commons, it is but due to the gentlemen who signed the same, that the facts of the case should be known; and you will therefore oblige the petitioners by inserting the above in your next number. It should also be remembered, that on this occasion the important announcement was made by Government, that all inter-

nal quarantine would henceforth be discontinued, shewing one good effect, at least, of petitioning.

ONE OF THE PETITIONERS.

February 28, 1832.

PROPOSED PUBLIC MEETING.

To the Editor of the London Medical Gazette.

SIR,

ALTHOUGH I join in the very general feeling of attention and admiration excited by your leading article of last week, yet you would not have heard from me upon the subject, had I not a *spice* of remonstrance to communicate to you. This, you know, is the way of the world, and I therefore expect my compliment to pass as a sufficient introduction to my further remarks. Your "little men, but out in all weathers" are, whatever may be said to the contrary, the only persons who to the public stand forth to rescue the profession from the opprobrium of inactivity and carelessness: despised, and, if you please, despicable as their efforts have been, they are yet preferable to the death-like stillness of the "*great guns*!"

On the score, too, of discordance and difference of opinion, although it is both ludicrous and lamentable to see the extremes—one man describing cholera as a new creation, grisly and irresistible as the monster of Frankenstein, while another would persuade us that we have been labouring simply under an attack of *blue devils*—yet this collision I prefer to an inflexible and factious adherence to any one opinion or theory. No breach of delicacy and courtesy will be committed if I join you in expressing my hope that some investigation of this matter will ere long take place by a select body, composed of men more conversant with the varying epidemics of our country, and more known to the metropolitan profession and public, than the present Board of Health. Our ancient "pastors and masters," the leading men in departments of the art of healing—our popular lecturers, the acting men at prisons and fever hospitals, and the visiting attendants at dispensaries, should have a voice in this matter; the materials for thinking, accumulated now from every quarter, should be ob-

tained through the minds of men of every mode of thinking, if such a combination could be effected.

And how, it may be asked, can this committee of criticism be elected? I see but one way, free from all reproach of partiality and prejudice—to call a public meeting of the profession, not to deliberate, for that is out of the question, but to elect and sanction a competent and sufficient *jury*, or sub-committee, to draw up a report; and when this is done, to refer back to another public meeting, to report progress and receive fresh instructions. I have some reason to think that such a course of proceeding, though likely to meet on the threshold with some opposition, would in the end be satisfactory to all parties. Let me be understood to mean a public meeting, entered upon without pledge or party-spirit, and where the majority would have it in their power to crush any thing like cabal or unfair dealing.—Yours,

LONDINENSIS.

ROYAL INSTITUTION.

Friday, Feb. 17, 1832.

WHITLOCK NICHOLL, M.D., VICE-PRESIDENT, IN THE CHAIR.

“On Volta-Electric and Magneto-Electric Induction.”

MR. FARADAY this evening gave a very lucid summary of his “*Recent researches in Volta-Electric and Magneto-Electric Induction*,” researches which would have stamped him as a first-rate experimental philosopher, even had he been hitherto unknown. These observations followed very opportunely the exposition given last Friday by Dr. Ritchie of his views of the action of the voltaic pile, in which there is much that is novel and much that is important.

Mr. Faraday commenced his experimental illustrations by giving a few apposite examples of ordinary electric induction, shewing that the phenomena are produced neither by the addition nor subtraction of any thing, but by the disturbance of the electricity which the body acted on inherently possesses. He next proceeded to demonstrate the analogy that exists between the intermittent sparks of common, and the continued spark of voltaic electricity. This led him to the proof that electric phenomena

might be induced by the galvanism battery as well as by the ordinary electric machine; and the apparatus which he has devised for this purpose displays great ingenuity and philosophical acumen. A coil of wire, such as is used for forming magnets by electricity, has another wire coiled round the same modiolus, but not any where in contact, so that the helix is formed of two spiral wires; through one of these a current of voltaic electricity is sent, and the other, which is unconnected with the galvanic pile, is connected with another helix, in the hollow of which a small rod of iron, or piece of wire, such as artificial magnets are made of, is placed, and then the *volta-electric* induction, from the galvanized to the non-galvanized spiral of the double helix is rendered manifest by the non-magnetic wire in the distant helix being rendered magnetic. The same very curious and important phenomenon was also rendered evident by a newly-invented instrument called a *galvanometer*.

To establish the course of the voltaic influence, or as Mr. F. expressed it, the *current*, he shewed a very beautiful experiment, in which two magnets were made from iron wires by introducing them alternately into a helix, the one when the connexion with the voltaic pile was complete, and the other at the moment of the circuit being broken, when the ends thus introduced were found to have opposite poles, indicating the forward progress, and the return of this mysterious power.

Satisfactory as the above evidence must be admitted to be of the point in question, Mr. F. did not rest content with what would have satisfied almost any body else, but as he had shewn the reality of volta-electric induction, and the formation of magnets being induced by voltaic electricity, he reversed the experiments, and made magnets themselves the subjects of investigation, and succeeded, as he informed his much interested auditory, in obtaining from them a spark, similar to the spark of common and galvanic electricity. A more beautiful and philosophic series of experiments we have never witnessed, and minute and delicate as were some of the manipulations, the operator, even under the disadvantage of a large and crowded theatre, was eminently successful in rendering nearly the whole visible and intelligible to almost every one.

At the close of the demonstration, Mr. Marsh, of Woolwich, exhibited his very powerful magnetic apparatus, in which, by a current of voltaic electricity, conveyed from a pile contained in a vessel not larger than a half-pint mug, so powerful a magnet was formed that it suspended between five and six hundred weight, and had the weights been put carefully into the scale, it not improbably would have sustained much more.

Now as this tremendous power, formed from a battery contained in so small a compass, can be immediately annihilated by breaking the circuit, and immediately restored by renewing the contrast, it does seem to us that the suggestion of an eminent professor present may not be without its importance, viz. that such a force might be used as a mechanical power. We grant that its application to machinery may be difficult, but perhaps not more so than the application of steam was formerly; and if it can be so applied, how vast the advantage, for how manageable would the agent be! "Knowledge is power," we repeat for the hundredth time, for of a truth this apophthegm of Bacon can never become a truism.

In the library we noticed "numerous domestic," and other articles brought by Captain Lyon from the Esquimaux country, and original drawings made by the same gentleman during his travels in those northern regions."

Some very elegant models of plants, greatly magnified, shewing the natural structure, and various stages of vegetable metamorphosis, were placed upon the table by Professor Burnett, of King's College; and Baron Draï was also present, to add to the intellectual entertainment of the evening, by explaining in person his very curious tachygraphical machine, which, however, we had not time to examine, and of the capabilities of which we cannot, therefore, particularly speak.

The announcement for Friday, 24th inst. is "*On the Genius of Extemporaneous Poets, and on the Art of Improvisation, by an Italian Improvisatore.*"

MEDICO-CHIRURGICAL SOCIETY.

MR. LAWRENCE IN THE CHAIR.

THE last two evenings of meeting (February 14th and 28th) were occupied by the reading of a paper on *Diseases of the Larynx and Trachea, requiring Bronchotomy, with practical suggestions relative to that operation*, by Mr. Wood, late house-surgeon of St. Bartholomew's. On Tuesday last there was a most respectable attendance of members, to hear the practical part of the paper read: the venerable Dr. Babington was present, and took an animated part in the discussion. However, we must postpone our report until the paper is concluded, which will be, we understand, on the next night of meeting.

On Tuesday, March 1st, the annual meeting was held for the election of officers of the Society for the year ensuing.

REPORT OF CHOLERA IN LONDON, UP TO THE AFTERNOON OF FRIDAY, MARCH 2, 1832.

New cases since our last report, 111
Deaths 62

	Cases from Commencement.	Deaths from Commencement.
Afloat in the River	18	12
Poplar.....	2	1
Bermondsey	10	6
Southwark	70	42
Newington Butts...	14	8
Chelsea	3	2
Lambeth	7	6
Christchurch	8	3
Westminster	4	2
St. Marylebone ...	2	1
St. Giles's	13	5
Whitechapel	6	5
Bethnal Green.....	1	0
Total.....	158	93
Cases reported from other places	16	15
Grand Total.....	174	108

Total number of cases throughout }
England since the commence- } 5733
ment of the disease }
Deaths... 1760

METEOROLOGICAL JOURNAL,

Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.

Feb. 1832.	THERMOMETER.	BAROMETER.
23	from 25 to 37	from 30.30 to 30.18
24	20 37	30.14 30.04
25	21 37	30.07 30.00
26	30 43	30.13 30.06
27	28 41	30.05 30.09
28	29 37	30.12 30.13
29	30 43	30.13 30.05

Prevailing winds N.E. and N.W.
Except the afternoons of the 24th and 26th, cloudy,

CHARLES HENRY ADAMS.

NOTICE.

A notice of the letter from Edinburgh, about the cases of cholera, was prepared, but by some oversight omitted to be sent to press till too late. It shall be inserted next week.

W. WILSON, Printer, 57, Skinner-Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MARCH 10, 1832.

LECTURES

ON

THE THEORY AND PRACTICE OF
MEDICINE ;

Delivered at the London University,

By DR. ELLIOTSON.

PART I.—LECTURE XXIII.

The subject of Fever continued.

I WAS speaking, gentlemen, at the last lecture, of certain affections which very commonly attend ague, and I mentioned those which usually accompany it when it is a recent disease. It is not uncommon in ague to see great affection of the head, great oppression, even pain of the head, and delirium. It is common to see the abdominal organs affected, so that persons have vomiting or diarrhoea, or cholera. The liver is particularly affected, so that in some countries ague and remittent fevers are called *gall-fevers*.

Ague Cake.—But the chronic form of the disease is very frequently attended by other affections; and frequently after ague has ceased, other diseases make their appearance. It is very common, for example, when ague has continued any time, for the spleen to become enlarged; so common is this in some parts that the tumor is called by the vulgar *ague cake*. The tumor formed by the spleen occupies the left hypochondrium, or perhaps the whole left half of the abdomen, and is called “ague cake.” Dr. Clegboro, who wrote a very excellent work on the diseases of Minorca, and which is well worthy of perusal when you have time, mentions having seen a spleen after ague which weighed 80 ounces. Dr. Gregory used to speak of one that weighed 12 pounds. You will frequently see this in the case of

infants. I have seen many infants with enormously large spleens, who had been the subjects of ague.

Various other diseases attendant on Ague.—When this affection has existed any time, it is common also to see ascites as well as enlargement of the spleen. You will frequently see enlargement of the liver and jaundice. The enlargement, both of the spleen and the liver, are common, but particularly the former. In the case of the spleen, the patient is for the most part pale, falls into a state of anæmia, bloodlessness; and in the case of the liver, he becomes more or less jaundiced. Very frequently you will have anasarca, as well as ascites, and not unfrequently, without any ascites, you will have anasarca, and these affections generally remain for a longer or shorter time after the ague has ceased. They will occasionally come on while the ague exists, and occasionally not till it has ceased, and in the former case they are all the worse for the continuance of the ague. It was formerly imagined that these affections arose from the ague being stopped, but this I am convinced is a mistake. Persons will have them who still have ague very violently, and these complaints are the worse for the ague. Patients will bear them much better if you cure the ague. The ague shatters the constitution, and renders it a more easy prey to any visceral disease that may exist. I do not believe these affections are the effect of ague; on the contrary, I am satisfied that they are the result of the same cause which produces ague, and of which I shall presently speak, because I have seen people labour under enlargement of the liver, enlargement of the spleen, and dropsy, of great intensity, who had had but one fit of ague, and that has borne no proportion to the severity of these other complaints. It appears to me that the same cause which produces the ague—a poisonous exhalation, produces likewise these various effects, and, indeed, they render the ague the more obstinate. Although sometimes it is almost impossible, or quite so, to cure an ague while these affec-

tions exist; yet if you remedy them in the best way you can, by mercury, bleeding, purging, and the usual remedies of chronic inflammation, you will frequently cure the ague without any trouble. If they arose from the ague being *suppressed*, you would not have them while the ague *continued*; and if they depended on the *ague*, and not on the *cause* of ague, they ought to bear some proportion to its degree and continuance, but they appear to bear a proportion in some measure to the continuance and intensity of the cause of ague. Still the continuance of ague makes them worse, by debilitating the system.

Another circumstance mentioned by some authors as an effect of ague, or perhaps I should say, of the cause of ague, is idiotcy. The faculties of the mind frequently become impaired by a long exposure to the cause of ague. Sydenham mentions a degree of idiotcy, or at least of impairment of the mental faculties, which he calls *amentia quartana*, quartan fatuity. There can be no doubt that the continuance of ague increases these affections, by debilitating the constitution, but I do not believe that ague gives rise to them, but that they proceed from the same cause which produces ague.

Vernal, or spring intermittents, those which attack between the winter and the summer solstice, are chiefly attended by affections of the chest, that being the period at which affections of the chest are most prevalent. The autumnal intermittents—those which take place between summer and the winter, are particularly attended by abdominal symptoms, by inflammation, and sometimes by intense bilious vomiting, purging, and jaundice. At this time of the year, too, ague has a great tendency to become continued fever—that is to say, the excitement is disposed not to cease but only to remit—When the autumnal intermittents prevail, you have as many remittent fevers as agues, and some of these become continued. This conjunction most probably arises from the causes of these various local excitements, and also of continued fever, co-existing with the cause of ague. Without any ague at all, we are all disposed to affections of the bowels, and of the stomach and liver, in hot seasons of the year; and if, therefore, the causes of ague are joined with the causes of these common affections, you will of course have ague accompanied by these other diseases. Still it is possible that the cause of ague may be modified, so as to produce peculiar effects at this season.

Diseases cured by Ague.—Ague, however, sometimes, so far from being accompanied by other diseases, actually puts a stop to some which had previously existed. Dr. Gregory used to mention that he saw palpitation cured by ague; that the man lived afterwards for many years, and died free

from the palpitation which he had suffered till the ague seized him. Dr. Fordyce mentions that he has seen ague cure many diseases. In fact, ague has been thought so capital a thing that some writers contend it never should be cured, and a proverb once prevailed that,

“An ague in spring,
Is fit for a king;”

not, however, I think, for a *subject*. You will find that Boerhaave says, in his aphorisms, that it disposes to longevity, and clears the body from inveterate diseases. His words are—*Ceterum (febres intermittentes) nisi malignæ, corpus ad longævitatē disponunt, et depurant ab inveteratis malis.*

Pernicious effects of Ague.—However, there is no doubt that agues shatter the constitution; they shorten life, they may be speedily fatal, and if any other disease exist at the time, they for the most part aggravate it. Dr. Macculloch believes, that when they appear beneficial, it is generally not by curing other diseases, but merely by their assuming a regular type. Ague, I mentioned in the last lecture, is sometimes obscure, does not declare itself out, and is then called *dumb* or *dead* ague, and under these circumstances you sometimes have extraordinary symptoms, which will disappear if the ague come on in a regular straight-forward style. It is therefore mentioned by Dr. Macculloch, and it appears very plausible, that when benefit appears to arise from ague, it is from the disease having existed previously, but in so strange a manner as to produce all sorts of symptoms, and then having all at once become regular. It cannot be one thing and another thing at the same time; but when it becomes regular, then, for the first time, a person thinks he has got an ague, and lost some other disease.

Usually the continuance of ague causes emaciation of the body, unless it produce hepatic or splenic disease and dropsy, and then the body will look bloated, but still it is pale, and evidently greatly diseased. It is even said, that in some parts, of Flanders especially, ague has a tendency to produce obesity, and sometimes very rapidly, but I imagine that the fat cannot be what people call *good* fat. There is a sort of bloated blubbery fatness; and when it is remarked that such a person has got fat, people will shake their head, and say it is not good fat. This, I think, must be the case in Flanders; and it is said that this obesity will occur there whether the disease has been acute or is chronic, and when there is no suspicion whatever of organic disease. It is said, too, that if the disease return there, or if the seasons be very unhealthy, and a patient becomes emaciated, the obesity returns when all this is over. Many fat Dutchmen live in places where malaria prevails,

but still they do not look healthy. The ill effects of the disease are so manifest, that no one who has an ague ought to be much displeased with the doctor for curing him of it. Oliver Cromwell and James the First are said to have died of ague, caught in London. Dr. Caius says, that so fatal was ague in London in the year 1558, that the living could hardly bury the dead. Bishop Burnett, in his History of the Reformation, says, that so universal was it in London, and so contagious, (which was a mistake) that it raged like the plague. Sydenham and Morton both say that it was the most fatal disease in London from 1661 to 1665. At Walcheren, in the time of our untoward expedition, in no more than five weeks—that is to say, the last week of September and the whole of October, our effective force was reduced by ague, or by remittent fever—agueish disease, to one-third, and ten thousand men died of what was called *Walcheren fever*, but which was neither more nor less than an aguish fever.

If it should so happen that a much worse disease than ague is arrested by its occurrence, we might allow the latter to go on; but I never saw such a case; and even then I should be inclined to stop the ague gradually, and to use at the same time the remedies calculated to remove the other disease. For example, it is preferable, I suppose, to phthisis; and if a patient were actually in a state of consumption, and the consumptive symptoms ceased on the occurrence of ague, one might be disposed to mitigate only the ague, but not altogether cure it; however, I never met with any of these cases. A very eminent man, Dr. Wells, one of my predecessors in St. Thomas's Hospital, fancied he had good ground for suspecting that phthisis and ague were opposite, so that where ague prevailed there was no phthisis, and *vice versa*. It really appears that there is something in this,—that ague does lessen the tendency to phthisis; but still the matter is not so important as Dr. Wells imagined; the opposition of one disease to the other is nothing like what he fancied it to be. You will see the question argued in a book by Dr. Southey, on Consumption, where he has taken great pains to collect documents from aguish parts, and proves that there is phthisis enough in those parts. I have seen several persons die of phthisis while they had more or less ague. Some have carried Dr. Wells's opinions so far, as to send a patient who was half-dead with phthisis to stand in a ditch at night, in order to catch an ague.

Aguish Face.—With respect to the effects of ague, you will find one shewn in the countenance. The face in ague, and long after, is often very peculiar; it is of a dirty straw colour, and is so well characterized, that it has been called *agueish face*. The face is not yellow, nor of the colour of clean straw,

but presents a sort of dirty straw-coloured look. Without the occurrence of ague, the mere residence in an aguish place gives the same hue, so that it is the effect of the cause of ague.

Predisposing Causes.—I shall now speak of the causes of ague, and among the predisposing, must be first mentioned bad health. If a person be out of health in any respect, he is more likely to be affected by the causes of ague, as is the case with respect to all other diseases. A general weakness of body, independent of the presence of any particular disease, must have the same effect. Certainly high health, in general, is one of the best safeguards against it. The father of the late Dr. Gregory who was one of my preceptors, the physician who wrote two very excellent works, distinguished by refined ideas and a high tone of moral sentiment, entitled, *A Father's Legacy to his Daughters*, and *The Duties of a Physician*, used to mention that when he was a student at Leyden—for then, if persons wished to get a good medical education, they were obliged to go to the continent—there were 24 students who always dined together, and were very much in each other's society. Twenty-three of them regularly drank a bottle of claret each every day, but one confined himself to water; the twenty-three escaped, while the poor water-drinker caught an intense ague. This strikingly exemplifies the use of keeping up the vigour of the body, when persons live in an unhealthy situation. It was claret which they drank, not port, but that was quite sufficient to keep up a degree of excitement of body, and strength of mind, which enabled them to resist the disease. Depression of spirits will have just the same effect as too low living. Any thing which lowers the body at large, whether it be over-exertion of the body, or of the brain or mind—whichever you choose to say—or the want of good food and cloathing, or the want of pleasurable excitement of mind—the presence of any thing which can depress the spirits, will have the same effect.

It is said that the effects of the mind were seen at Walcheren, and probably they were, from this circumstance: on the men arriving there, all full of hope of military glory, they for some weeks continued healthy; but at last, when they saw that there was nothing to be done—that the expedition would fail, they soon became the victims of disease. It is, however, to be considered, that latterly they had much greater fatigue to go through, and the longer they remained there the more they were exposed to the morbid cause; but still, when they lost all mental excitement, by the failure of the hope of what is stupidly called "military glory," they nearly all fell victims to the disease.

The good effects of wine in keeping up the

strength against this disease are shewn in people finding it indispensable in the marshy parts of France. They cannot resist ague there unless they drink their wine. Sir John Pringle mentions, that wine and full diet are the best safeguards against it. The Dutch are in the habit of exciting themselves by taking spirits before they go out in the morning; and it is highly necessary, exposed as they are to the exhalations of their putrid ditches.

Though all ages appear liable to the disease, infancy and extreme age are less liable to it than the middle period of life. Certainly the greater number of persons whom I see labouring under ague are neither infants, nor those much advanced in years. It is, however, to be remembered, that the extremes of age are much less exposed to the cause of ague than other persons. Women, too, less frequently have ague than men; but I should think this arises, not from the female constitution being less liable to it, but because, from spending a greater portion of time at home than men, females are less exposed to its cause. Sir Gilbert Blane states, that during the ten years he was physician to St. Thomas's Hospital, he had 192 cases of ague, only 33 of which were in females. I made an estimate of my cases of ague at St. Thomas's, during one period of five years, and found that I had 142 cases, among which 23 only occurred in women.

It would appear that persons who have not been exposed to the cause of ague are more liable to suffer when they by chance are exposed, because those who had formerly lived among them, if exposed at any subsequent period, will continually escape, while they suffer: hence it is said, that some farmers in aguish parts have made fortunes by marrying a succession of wives from healthy districts; coming fresh upon an aguish part, the malaria has taken hold of them, and a good *kind* husband has been enabled to look out for a second, and a third to *love*. However, although persons may become habituated to it, as they may to any thing else, yet it more or less injures the constitution, so that those who live in aguish parts, unless they are well fed and in good spirits, suffer more or less; they seem to be gradually poisoned by the causes of ague, although these may have not been sufficient to excite ague itself.

Exciting Cause; Malaria.—The exciting cause of ague, the true indispensable cause of it, I believe to be an exhalation from decaying vegetable matter—what is sometimes called *phyto-septic, vegeto-putrescent, miasm, marsh miasmata, malaria*. However, *marsh miasma* is an improper expression; it will come from a marsh, but you may have it without the existence of any marsh at all.

It is said that the causes of ague were

not known to Sydenham nor to Stahl, and that they were first assigned by an Italian author (Lancisi), in 1717. It is said that neither Boerhaave, who wrote in 1747, nor Baron van Swieten, who wrote a few years afterwards, were aware of the cause of ague. Dr. Cullen, however, from extensive reading, satisfied himself that there was but one real cause of ague, and that it was the exhalation which I have now mentioned. The ancients were quite aware that marshy districts were exceedingly unhealthy. The least acquaintance with the classics will furnish you with lines enough to prove this.

Now the reason for ascribing the aguish remittent or intermittent fevers to these exhalations—*malaria*, as they are now generally called—to vegetable matter in a state of decomposition—is, that the disease prevails in the neighbourhood of marshes; that it declines in proportion as marshes are drained; that ague rarely appears in any one who has not been evidently exposed to the exhalations of decomposing vegetable matter. Every one has heard of the ague of the low countries,—of Flanders; every one has heard of the aguish fevers of the Banks of the Ganges; every one has heard of the ague of the fens both of Lincolnshire and the hundreds of Essex, and also of the lower parts of Kent. I believe I mentioned that the disease is not by any means confined to marshes; it springs up around marshes where persons are exposed to their exhalations, but every spot which contains decomposing vegetable matter may excite the disease. I should prefer the term *malaria*, which is now used, to *marsh miasmata*, on this account.

Moisture.—It is not mere moisture which produces the disease, for there is no ague, however wet the weather may be, till swampy land is approached, or exhalations arise from a swampy part. No drenching by rain will produce it, unless a person has had ague, or been exposed to malaria, before: but when a person has once had ague, taking cold (whether there be moisture or not) may re-excite it, or may render the system susceptible of the effects of malaria which it had resisted before. In the thick fogs on the banks of Newfoundland, people never experience ague. The waters there come from the gulf of Mexico, are called the gulf stream, and are from six to ten degrees higher than the temperature of Newfoundland. In consequence of this, the vapour which passes from the water is immediately condensed; and therefore there is a constant fog, so that the fishermen who reside on the great bank are six months employed in fogs so great that they can hardly see from one end of the ship to the other; they are constantly enveloped in moisture, and yet they are never known to have the ague. There is more rain in the western than in the eastern parts

of England, and yet the inhabitants are not proportionately unhealthy; on the contrary, it is on the eastern side that ague particularly prevails, as I shall mention hereafter. So much with respect to mere moisture.

But, with regard to exhalations, I may mention that Minorca has a rocky bottom, has no marshes, but it is subject to ague, because, in different parts of the island, there are pools of stagnant water. The water being stagnant, much vegetable decomposition takes place, and the island is in the same predicament as though it were marshy land. The soil of the Campagna of Rome is dry in the most fatal season, but then the Campagna is intersected with ditches and drains of water, which is impregnated with vegetable matter quite sufficient to explain the aguish fevers of that country. The fevers of Egypt begin with the subsidence of the Nile, and those of Bussorah with the subsidence of the Euphrates; and the Arabs, to revenge themselves upon the inhabitants, once let loose the waters, and thereby produced a disease which destroyed 14,000 persons. In Artois, in Flanders, the south and south-west winds come over swamps, and though the part itself is not swampy, they bring aguish complaints to it; and as soon as the wind sets in from the sea and the north, these aguish complaints all disappear;—and I may mention that these north winds, which do not come over swamps, are accompanied by very dense and durable fogs, but no ague occurs. The same is the case in many parts of France and the Mediterranean. In 1528, the French army, in attempting Naples, was reduced from 28,000 to 4000 men, simply from choosing an injudicious encampment near the Baïæ. Infinite is the mortality occasioned by ignorant or careless generals in this way, by encamping an army near a marsh, and by governments permitting marshy lands to continue in the neighbourhood of cities. The instance of Walcheren must ever be a disgrace to the government of this country. “There was no reason apparent,” says Dr. Macculloch, “for perpetuating Calcutta, when, from the very hour almost of its foundation, by Charnock, its destructive situation had been demonstrated. That Holland should have persisted in inhabiting that Batavia, which it had studied to render more poisonous than nature had already done, by the model of its own pestiferous forefather, is a problem which Holland must be allowed to explain as it best can.”

But although moisture is not the cause of ague, yet moisture is indispensable to the decomposition of vegetable matter, and to the disengagement of the miasma. Decomposition requires a certain degree of moisture, and if you dry any thing thoroughly, whether it be a vegetable or an animal production, you prevent decomposition. A cer-

tain degree of moisture is necessary for the production of ague by the fermentation and putrefaction of vegetable matter, which gives rise to the exhalations. Hence, in a moderate swamp, you see why dry weather may put a stop to the disease—namely, by putting a stop to putrefaction; and you also see why, in extremely wet situations, there is often, because too much fluid also impedes putrefaction, no ague; but this wet, by dry weather, may be reduced to just sufficient swampiness for vegetable decomposition to take place, and malaria to be produced. Just as dryness would prevent decomposition, so extreme wetness and moisture will likewise stop it. If the matter which is to be decomposed be diffused in a very large quantity of water, the putrefaction of course ceases, or is not evident; so that some places which were very wet and healthy have been made unhealthy by being dried just sufficiently for putrefaction to go on vigorously; and again, other parts which were dry, and which never gave out any exhalations, have been caused to do so by a certain degree of moisture falling upon them. Thus you see that, according to situation, the same additional moisture may produce an ill or a salutary effect. High grounds may therefore suffer from the same cause which removes all unhealthiness from low grounds. A certain degree of rain falling upon high ground, will not all remain there, but will roll down; still it has moistened the parts sufficiently for decomposition to take place above; whereas, when it comes on the low ground, and there collects, it may be so abundant as to dilute all the vegetable matter and prevent it from putrifying, and so put a stop to the unhealthiness of the part. In this way an aguish fever, whether intermittent or remittent—for I intend to include both when I say aguish fever—has been put a stop to by inundating a swamp; and on the other hand, inundation has moistened a dry part sufficiently for exhalations to take place.

By recollecting this, you may explain a number of circumstances which otherwise would appear contradictory. A river, by breaking loose over a marsh, has sometimes prevented it from being any longer the source of noxious exhalations; where there was a marsh before, it has produced a sort of lake. Again, a change in the course of a river may drain a marsh completely; and in that way a part has become healthy which before was exceedingly unhealthy. Sometimes the very reverse has taken place; by breaking over dry land it has caused sufficient moisture for putrefaction to go on. The sea has done the same; it has sometimes produced the greatest unhealthiness in a neighbourhood, by making its way upon the country and producing a permanent swamp; whereas, in other cases it has produced something more than a swamp—it has deposited such a body of water that no exhalations could be

given out, although before they were emitted in great abundance. The Godwin Sands are an instance of a cured marsh, though the land is lost.

As every river carries down a quantity of solid materials, so every river has a tendency to fill up its bed ; it becomes more or less filled with solid matter coming down from high land ; and a river may be completely filled up, so that the banks must be raised, or the country will become inundated, a marsh will be produced, and great unhealthiness take place. This is frequently remedied for a time by embanking a river, so that it shall still be confined within its proper bounds. I say for a time only, because the higher the river becomes, the more slowly it runs, and the less it drains the country, till at last it is useless. But in some countries people are prejudiced, or careless, and parts which were exceedingly healthy become the seats of the most dreadful disease. There is an instance in Switzerland, of a part becoming very unhealthy to all around, and aguish fevers extending over a great district, as far as Zurich, from the lake of Wallenstadt. A river there (the Linth) pours its water into another river (the Mag), and that again into the lake. The Mag became stopped up, through neglect, and the neighbourhood all round, a few years ago, became one of the most pestilential districts in Switzerland, 6000 acres being thus converted into a marsh. At last the authorities had their attention forced to it, and it was remedied simply by turning the Linth into the lake, instead of allowing it to empty itself into the Mag and by widening and deepening the Mag ; the lake being a deep spot, receives all that comes down from the mountains, and the whole has now a free course. The obstinacy of the people, in living in the midst of a pestilence when they had it in their power to remedy it, is hardly to be conceived ; but it is no less true. I could give you ten thousand illustrations, but they all come to this point—that a certain degree of moisture is necessary to the disengagement of malaria ; that if you diminish the moisture, you diminish the malaria in one case, and increase it another ; that if you increase the moisture, you may also produce either of these effects ; and thus it is that circumstances the most opposite will have the same effect, and circumstances which are exactly the same will have the most opposite effect, according to situation.

Temperature.—But a certain degree of moisture is not all that is necessary—there must also be a certain degree of temperature. We can preserve any thing from decomposition by an exceedingly low temperature ; and by an exceedingly high temperature we produce such changes that no decomposition can take place. We know that if meat be spoiling we

have only to cook it, and the putrefactive process is immediately arrested ; and so it is with respect to the putrefaction of vegetable matter. A certain degree only of temperature is proper. If the temperature be increased, then you may have extreme decomposition, and disengagement of malaria ; so that while the cold weather lasts the people will be comparatively healthy, but when the warm weather arrives, the decomposition is such that the most fatal fevers prevail : hence it is that aguish diseases are far more severe in hot countries than they are in ours ; and that they are far more severe in autumn than at any other period. This, at least, is one reason, but there is another ; and that is, in hot countries, and in hot seasons, there is far more vegetable matter to decompose. This is certainly to be taken into the account ; but when we consider that an increase of heat produces a great increase of decomposition, we also see that fever must greatly be ascribed to the facility with which decomposition goes on in a warm temperature. A low temperature, however, may be short of that which is sufficient to stop the disengagement of malaria ; and therefore, if a country be very unhealthy, and very swampy, you will in cold weather have aguish diseases. The cold is not such as to stop putrefaction, but the land is so moist as to produce a very considerable disengagement. The reason that ague prevails so much in the spring is, that the soil which was caked and dried by the cold of winter is now moistened, and the temperature of the weather has increased ; the cake has consequently been broken up, and therefore you will have vegetable exhalations take place in abundance. The vegetable matter which was left on the ground the preceding autumn, becomes decomposed, by the increased temperature. The reason that ague is so fatal in the autumn is, that the summer, of course, has produced a fresh stock of vegetable matter. The surface of the earth is covered by vegetable matter ; it is in great part dying, and lies upon the ground to rot ; the rain comes in the hot period ; and what with the heat, the moisture, and the dead vegetable matter, exhalations of course take place ; and the great abundance of the vegetable matter to be decomposed, together with the heat, renders the autumnal fevers far more severe, and far more numerous, than the vernal.

The effect of moisture has been frequently seen in a striking way, when rain has begun suddenly after a dry season. When a quantity of vegetable matter has been lying upon the surface of the earth, and has become caked, a sudden rain has been attended on many occasions by the most fatal diseases. By moistening the vegetable matter, putrefaction has instantly taken place, and an abundance of exhalations been emitted. Again, the reason that when the autumn

is over these fevers cease is, that the greater part of the vegetable matter has been decomposed, and the temperature of the atmosphere falls; and therefore there is less matter to putrify, and there is not the temperature which is sufficient for putrefaction to go on with any vigour.

Pasture lands, from being constantly moist, are very aguish, and yet on breaking them up and cultivating them they are infinitely more so; and all from the same reasons that I have now mentioned. Although pasture lands must be unhealthy, for they are all moist—and they cannot be good if not moist, for the grass will not grow without moisture—yet when you break them up you make them more unhealthy, because you kill all the vegetable matter in them. Before, there were only portions that died, but when they are broken up, nearly all the vegetable matter is killed, and it rushes into putrefaction; so that it is always found that the breaking up of pasture lands is attended by far more disease than existed before the land was so broken up. It may, however, be a fact, that malaria is let loose from the earth by breaking it up. It is said in many countries to be a notorious fact, that on the breaking up of the surface of the earth, these fevers are sure to prevail. It is said that in the West Indies this process has been known for two centuries to be exceedingly dangerous. Cassan mentions, that fevers, like the plague, will take place on land being broken up, so that labourers will sometimes die on the spot if they remain there all night, whereas no harm arises if they be not broken up. Volney, the traveller, mentions this as an invariable observation in America. It is found also, and the fact is precisely similar, that on clearing away woods the disease especially prevails. A quantity of malaria is let loose from the forest when the woods are cleared, and more than that, a quantity of vegetable matter necessarily lies on the surface of the earth, which, although kept moist enough for decomposition, is prevented from decaying much, from the inability of the sun to penetrate to it; but on clearing the wood, the heat of the sun is admitted to the surface where the vegetable matter is lying, and it then falls into a state of rapid decomposition. Hence it is found that clearing woods is always a dangerous process, and it may be so from two circumstances—first, the malaria which is pent up in the woods is let loose; and, secondly, the sun has access to the vegetable matter which lies on the surface, and effects its decomposition. Besides, much vegetable matter must be killed by the process. It is said that the same effect will take place in Italy if certain bushes be cut down. Whatever malaria may be in them has then an opportunity to escape, and fever to a certainty prevails. Removing woods has frequently been injurious in another point

of view—by removing a protection. Some villages have been free from ague while a wood existed between them and a marsh; but on cutting down the wood, they became from that moment exceedingly unhealthy, the wood having served the purpose of a screen. Some have imagined that it was the knowledge of this circumstance—of this advantage of woods—that caused them to be so venerated, and held so sacred in ancient times.

Stagnant Water.—It has been said that merely stagnant water will produce ague without any vegetable matter being in it; but we are to remember that this stagnant water will putrefy, which it certainly would not unless it contained animal or vegetable matter. Water itself, pure oxygen and hydrogen, cannot undergo putrefaction, but soft water will putrefy, and as that is the case, it must contain dead earthy matter; and therefore if stagnant water, although nothing can be seen in it, does produce ague, we may explain the occurrence by supposing that it contains vegetable matter. It is well known that the Thames water, when put into casks, however good it may look, will stink; and it is said that the decay of the wood in the interior of the cask is quite sufficient to explain this—that if you put pure water into a cask, yet the wood of the cask being vegetable matter, and being next the water, becomes more or less acted upon by the water, and more or less undergoes the process of putrefaction, and so affords malaria. It is on account of this liability of the wood to be destroyed, that casks are always charred within, by which they are enabled to resist the putrefactive process.

As this subject is rather long, I must resume its consideration at the next lecture.

CASE OF APOPLEXY, WITH OBSERVATIONS;

Taken from a Clinical Lecture delivered at the Middlesex Hospital.

By DR. WATSON.

GENTLEMEN,—In conformity with my general plan, of considering first those cases which may have afforded us, during the week, the means of investigating the consequences or the conditions of disease in the dead body, I beg to direct your attention to that of J. Milton.

This man, a sort of pedlar, said to be about 45 years of age, but looking older, was brought into the hospital at six o'clock in the evening of the 29th December, in a state of deep insensibility. His countenance was suffused with a dusky flush; his pupils were

largely dilated, and immoveable; and frothy mucus, occasionally tinged with blood, was issuing from his mouth. There was no contortion of features. The surface of his body was cold and mottled; the muscles of his extremities were rigid, yet affected with slight tremors; his respiration was loudly stertorous, short, and convulsive, but deeper and more natural at every fourth or fifth movement; his pulse 100, full, hard, and labouring. The insensibility was said to have come on suddenly an hour before.

One of the jugular veins was immediately opened, and xxviii . of blood were suffered to flow. The bleeding reduced, in some degree, the volume of the pulse, without abating its hardness; it was followed by no amendment of the general symptoms. As he had been brought from a public-house, and something had been said about his drinking, it was thought right to use the stomach-pump; but there was no odour of spirits in the fluid withdrawn from the stomach. Twelve ounces of blood were then taken from the temples by cupping; his head was shaved and covered by a blister; and ten grains of cathartic extract, with a drop of croton oil, were placed upon the back part of his tongue, and *swallowed*. A turpentine enema was also injected, and a mustard poultice applied to the epigastrium. The bladder was found to be distended, and three pints of limpid urine were drawn off by a catheter. He made some feeble attempts, with one hand, to resist the introduction of that instrument. This action, the deglutition of the pills, and the labouring respiration, seem to have been the only remaining evidences of sensibility to external impressions.

He remained for some hours in nearly the same condition, and then gradually sunk. He died at eight o'clock in the morning of the 30th, fourteen hours after his admission.

Such is the substance of Mr. Corfe's account of him, for I did not happen to see this patient while he was alive. It was looked upon as a case of apoplexy, and treated accordingly; and such, in fact, it turned out to be.

But some time after his arrival here, a more particular account of the circumstances of his attack was obtained; and this tended to throw a degree of doubt, and therefore of additional interest, over the case, the real nature of which was not satisfactorily cleared up until the body was examined.

It was stated—and this was subsequently confirmed before a coroner's jury—that the patient, having applied to the keeper of a public-house for employment, had been set to clear an obstructed pipe leading from a privy, and forming a communication between it and a large drain, which ran below the cellar of the house; that the stench from the obstructed pipe was horribly offensive; and that the man had for some time previously

been fasting. He was occupied in endeavouring to clear the pipe for two hours, but failing to accomplish his object, he descended into the cellar, in order to open and examine the drain. In about ten minutes after the time when he left the pipe to go down into the cellar, he was heard moaning, as if in great distress. A person who went immediately to his assistance found him nearly senseless, but still on his legs, and supporting himself by the door, and a post that he had laid hold of. He was removed from the cellar, and swallowed about a table spoonful of brandy that was offered him; and he was afterwards brought to the hospital.

Now this history gave rise to a suspicion that the state of insensibility in which the patient remained up to the time of his death, *might* have been the effect of the inhalation of some of the poisonous gases, which are well known to be often generated by the soil of a "necessary."

The exhalations which proceed from these places consist chiefly of a mixture of sulphuretted hydrogen and ammonia. When breathed in a concentrated state they prove rapidly destructive of life. In a smaller dose, or a more dilute form, they are apt to occasion inflammation of the conjunctiva, headache, a tendency to syncope, nausea and vomiting, colicky pains in the bowels, and diarrhoea. These minor effects of the noxious effluvia are well known by our *night-men*, who are said, by Dr. Paris, to depend mainly upon emetics for their removal or relief. I am told that the men employed in the coal mines in the north of England have recourse to a similar remedy when affected by the *choke-dump*, and that the employment of *mustard* by them as an emetic first led to its adoption in certain stages of the cholera, a practice which seems to have been attended with considerable advantage.

Dr. Paris mentions the *death* of four persons, as having occurred a few years ago at Brompton, from an attempt to clean out a privy there. Fatal accidents of this kind are, however, more common in France, where the privies, for the most part, terminate in closed vaults, which are occasionally emptied of their accumulated contents, at considerable hazard to the workmen employed. There are also large pits, or cess-pools, in Paris, into which many sewers discharge themselves; and the operation of clearing these out from time to time is very dangerous, as well as disgusting.

The symptoms produced by exposure to these noxious vapours, not only vary according to the intensity of the effluvia, but differ apparently also, where that intensity is the same, in different persons.

Amongst the symptoms which have been enumerated by those who have witnessed accidents of this kind, are sudden weakness and insensibility. The patient falls down;

sometimes dies outright and immediately ; sometimes becomes delirious and convulsed ; and when death has ensued after a certain interval, it has generally been preceded by a state of coma, a fixed and dilated pupil, difficult respiration, coldness of the skin, and the expulsion of frothy mucus, sometimes tinged with blood, from the mouth.

Now this description applies very closely to the condition in which our patient was found upon his arrival at the hospital.

There were some circumstances, again, which were less in accordance with the supposition that this might be an instance of what is called asphyxia, from the inhalation of a poisonous gas. One of these was the state of the patient's pulse, which was full and hard ; whereas the insensibility produced by the mephitic exhalations from privies and sewers—*i. e.* by sulphuretted hydrogen and ammonia—seems to be rather of the nature of syncope, and to be characterized by smallness and irregularity of the pulse, or even by the total absence of pulsation in the arteries at the wrist.

Again, the circumstance of his having left the pipe whence the effluvia proceeded, before the insensibility took place, was against the likelihood of its being the consequence of those effluvia. I am not aware of any such effects having been actually observed to come on after the person has been removed from the direct influence of the noxious vapour. Neither the pipe nor the drain were open towards the cellar ; his object was, I believe, to open them, but he had not effected it, and was found about three feet distant from them.

Besides this, it was stated that he had formerly received, in some fray, a severe injury of the head ; and that, being habitually a spirit-drinker, he had been much more readily affected by strong liquor, and more boisterous and violent when under its influence, since that time. This part of his history was in favour of the presumption that the attack was owing to disease within the head.

Nevertheless, there was enough in the preceding account to make the examination of the body, to me at least, an object of more than ordinary interest. Certainly no positive or safe opinion as to the cause of the man's death, could have been given without such examination.

The following summary is given by Dr. Christison, of the appearances met with in the bodies of persons destroyed by the effluvia in question. "Fluidity and blackness of the blood ; a dark tint of all the internal vascular organs ; annihilation of the contractility of the muscles ; more or less redness of the bronchial tubes, and secretion of brown mucus there, as well as in the nostrils ; gorging of the lungs ; an odour throughout

the whole viscera like that of decayed fish, and a tendency to early putrefaction."

The body of our patient was examined 24 hours after death. The extremities were livid, and the muscles generally were rigid.

There was a trifling quantity of fluid in each pleural cavity. The lungs were simply gorged with blood, their structure being perfectly healthy. The heart was much enlarged, and its augmented size was chiefly owing to a great increase in the muscular substance of the left ventricle. Considered in relation to the general bulk and stature of the patient, the walls of that cavity were of nearly twice the ordinary thickness ; there was no increase of its capacity. The several valves, and the internal lining of the heart throughout, were healthy and natural ; but at the commencement, and along the arch of the aorta, there were seen many of those opaque and uneven patches which we have so often occasion to observe there, and which result from the deposition of a matter resembling cartilage, mixed frequently with scales or plates of bone, immediately beneath the inner membrane of the artery. These patches were more numerous and larger, around the orifices of the principal branches of the aorta, than elsewhere. There was a considerable quantity of dark, coagulated blood in the right chambers of the heart, and a small quantity in the left. There was no uncommon smell given out by the body, nor was the colour or consistence of the muscles and other vascular parts at all unusual.

There was nothing, then, in the condition of the thorax, or its contents, to confirm the presumption that the patient's death had been occasioned by the inhalation of a poisonous gas. Consequently the probability grew stronger that the case would prove to be really a case of apoplexy. The hypertrophy of the left ventricle of the heart was also an additional circumstance in favour of this belief.

And supposing it really a case of apoplexy, it became easy to predict that we should find blood effused, in considerable quantity, within the cranium, and occupying, most probably, the lateral ventricles.

I may as well mention here, that the contents of the abdomen, which were examined after the head had been inspected, were found perfectly healthy.

When the skull-cap had been removed, the longitudinal sinus was slit up : it contained a continuous string of dark coagulated blood. The appearance which the surface of the brain presented, after the dura mater was taken off, was characteristic. There was no effusion beneath the arachnoid ; the convolutions of the cerebrum were flattened, and, as it were, pressed together, and the sulci between them much shallower than

usual. The regular shape and symmetry of the hemispheres was disturbed. When you find the exterior of the brain in this condition, you may be certain that you will discover some cause of pressure within. The left hemisphere, which was the most protuberant and misshapen of the two, gave the sensation of fluctuation when handled. Accordingly, a large quantity of blood, partly in the condition of soft coagulum, partly of a red-coloured serum, was found occupying the situation of the left lateral ventricle; the septum lucidum had been broken down, and a free communication formed between the two ventricles. The hæmorrhage appeared, however, to have burst first into the left, which contained by far the larger part of the *coagulated* blood. The right ventricle was merely distended, and that by the bloody serum; whereas the brain in contact with the effused fluid on the left side, was soft, pulpy, and ragged, with here and there a small adhering clot. No particular spot or vessel from which the blood had been primarily effused, could be detected. Blood had passed downwards also, towards the infundibulum, into the third and fourth ventricles, and a firm coagulum was lying beneath the optic commissure. The cerebral substance elsewhere was firm, and its cut surface was marked by a few, rather large, red spots. There was no manifest disease in the principal arteries at the base of the brain; but the same cartilaginous deposition which had been noticed in the aorta was met with also in each of the common carotids.

I said, that supposing this patient to have died of apoplexy, it was easy to foresee that we should find a *large* effusion of blood in the brain, and probably in the *lateral ventricles*. This conclusion was drawn from the course of the symptoms.

When stupor and coma come on by degrees, increasing gradually until the insensibility is complete, you will find, in general, the ventricles of the brain distended with *serum*.

Again, when a person is *suddenly* struck down, and either does not lose his consciousness at all, or recovers it in a short time, but is deprived more or less completely of sensation and the power of motion in one side of the body, or in a single limb; then you may be pretty sure that *blood* has been effused into the *substance* of the brain, in *small* or in *moderate* quantity, and that it has not forced its way to the surface of the brain, or into its ventricles.

Again, when a person falls down of a sudden, apoplectic, and becomes at once, or in a very short time, comatose, and generally insensible (as was the case with our patient), you may reckon, first upon his speedy death, and next upon finding laceration of the cerebral substance, from a large effusion of

blood; the blood having forced itself a passage into one or both of the lateral ventricles.

These are the rules. They are liable to occasional exceptions. Thus coma, from the *slow* effusion of *blood*, may come on gradually; and this not unfrequently happens in cerebral hæmorrhage produced by blows or other external injuries of the head.

On the other hand, sudden hemiplegia may be independent of cerebral hæmorrhage, and result from serous effusion. So, at least, I believe. I once saw complete hemiplegia suddenly supervene upon strong convulsions, which were confined also to the same side of the body, in a lad of fifteen, in whom acute dropsy in various parts had come on, after exposure to cold, just as he was becoming convalescent from scarlet fever. In him the paralysis disappeared entirely in twelve hours; his gums and breath having begun to shew the influence of mercury, and his bowels having been freely purged, during that interval. As this boy recovered, I cannot be certain that the temporary palsy was the effect of serous effusion within the head; but, in my judgment, the presumption that it was so is very strong.

In at least three cases out of four, the effusion of blood in cerebral hæmorrhage takes place into the corpus striatum, or the optic thalamus, of one side, or into the parts on the same level with, and in the immediate vicinity of, these portions of the cerebrum; and where the quantity extravasated is large, the blood breaks through the substance of the brain, either to the surface, or (which is infinitely more common) into the corresponding lateral ventricle, or even in both these directions at once.

Blood is sometimes found also distending the lateral ventricles when no laceration of the cerebral substance is discoverable. The hæmorrhage *may* then have been the result of exhalation from the membrane lining those cavities; but I am rather of the opinion that the blood, in these cases, proceeds generally, if not always, from the rupture of some one or more of the vessels composing the choroid plexus. This source of hæmorrhage may easily be overlooked; but the vessels have been actually found broken; and they are obnoxious to well-marked disease of such a nature as to render them more than usually fragile. The arteries, for example, which belong to that plexus, are subject to a peculiar kind of alteration, which I shall presently mention as frequently pervading the whole arterial system of the brain; and the veins are not uncommonly observed to be partially enlarged and varicose. This latter morbid condition has often been mistaken, and the diseased plexus' put up in bottles as specimens of small hydatids of that part.

It is certain, too, that blood has been found spread like a cap over the surface of the hemispheres, in cases where the most careful scrutiny has failed to trace its source to any ruptured vessel.

In hæmorrhage into the brain, however, you are to remember that the rupture of a vessel is the rule; and exhalation, if it ever occur, is the exception. This is just the reverse of what is observed in hæmorrhage from most other internal parts, and especially from the mucous membranes: there, exhalation is the rule, and the laceration of a vessel of notable magnitude the exception. You will naturally inquire the causes of this remarkable liability to rupture of the vessels of the brain above all others; and it does not seem difficult to assign them.

In the first place, there are certain original peculiarities of texture and relation, which may be conceived to predispose the blood-vessels of the brain, more than others, to laceration. They are long and slender; the coats of both arteries and veins are thinner and weaker than in any other parts of the body; the middle tunic of the arteries has not more than one half its ordinary thickness, and the outer or cellular coat is of such extreme tenuity that doubts have been entertained concerning its existence. These vessels, moreover, are not protected as elsewhere, by investing sheaths of cellular membrane, and receive but slight support from the soft and delicate substance by which they are immediately surrounded; and as they lie near the heart, and in the primary direction of the blood as it is driven from the left ventricle, they have been thought especially liable to be impressed by the additional momentum arising from the more forcible contractions of that chamber, whether these are determined by occasional and transient causes, or depend upon permanent organic disease of the heart itself.

But, doubtless, the main predisposing cause of rupture of the cerebral blood vessels, is their great liability to disease. After the commencing portion of the aorta itself, there are no arteries in the body so frequently found in a morbid state as the arteries of the brain; and the change to which they are most subject is that deposition between their coats, sometimes of a substance resembling albumen, or soft cartilage, sometimes of actual phosphate of lime, to which I have already alluded. This earthy or cartilaginous deposit exists usually in whitish patches of an oblong form, disposed at various distances from each other; sometimes in a succession of small scales, or even rings of bone, with healthier portions of the artery between them. One effect of this morbid condition is to diminish the bore of the affected artery, and to make it of unequal capacity; and as this variation of calibre impedes the free passage of the blood, it tends indirectly

to increase the pressure of that fluid against the sides of the vessel. Another effect is to deprive the coats of the artery of their natural elasticity, and to diminish their power of cohesion; and thus to render them weak and frangible, and unable to sustain the increased impulse of the blood. This condition is met with in the minute ramifications, as well as in the larger trunks, of the cerebral arteries; and it may be present in the one part, whilst the other is apparently healthy. Small specks of earthy matter have been found in the torn substance of the corpus striatum. Nothing unusual was discovered in the arteries at the base of the brain in our patient; but it does not follow that they were in a sound state throughout all their smaller branches.

The dependance of cerebral hæmorrhage, for the most part, upon disease of the blood-vessels of the brain, explains very satisfactorily why sanguineous apoplexy is so peculiarly a disease of advanced life. Earthy concretions in the coats of the arteries are so frequent in the later periods of existence, that they are met with (according to Bichat) in seven individuals out of ten, of those who die beyond the age of sixty; and Dr. Baillie considered ossification to be much more common, in old persons, than a healthy state of the arteries.

It is to the fact of the rupture of vessels also, that we must chiefly look for an explanation of the peculiar liability to hæmorrhage of certain portions of the brain—the corpora striata, namely, the optic thalami, and the parts immediately adjacent to these. The corpora striata are not only of softer consistence than most other parts of the brain, but they are also traversed by more numerous, as well as by larger blood vessels, than are other parts. The knowledge of these circumstances, and of the probable explanation they afford of the fact, that the parts I have just mentioned are the most common seat of extravasation, is as old as the time of Morgagni.

I stated, when describing the appearances met with in the thorax, that the change which the heart had undergone afforded an additional reason for believing that we should find blood effused within the cranium. In fact, cerebral hæmorrhage, and increased thickness of the walls of the left ventricle of the heart, have been so frequently observed together, that the latter morbid condition has been, in such cases, assigned as the cause of the former. It has been supposed that the powerful contractions of a ventricle thus morbidly strong, may drive forwards the blood with such unusual force, as to strain and burst the cerebral vessels.

This conclusion must be taken with much allowance; or I should rather say, the reasoning which would connect these events with other, as cause and effect, is greatly to

be distrusted. When the arteries of the brain are ossified, the commencement of the aorta will (always, I believe) be found affected with the same kind of alteration, and *often* sensibly dilated. And with this change in the aorta, and apparently *in consequence of it*, there is often, though not always, to be seen, a marked increase in the thickness of the muscular substance of the left ventricle. But the strength of this chamber is not to be taken, *under such circumstances*, as a correct measure of the force with which the blood is propelled into the extreme vessels; it is, on the contrary, an index and measure of the *difficulty* with which the blood is circulated through the primary branches, and therefore through the entire system of the arteries; and it furnishes a beautiful instance of that tendency to compensation and adjustment, which is so conspicuous in the animal body under disease, and of which the purpose is eminently obvious and intelligible where it concerns the circulation of the blood.

I by no means intend to apply these observations to *all* cases of hypertrophy of the heart, but to such only as take place under the particular circumstances which we have just been considering.

I apprehend then, in general, that disease in the cerebral blood vessels is essential to their rupture, and sufficient to occasion and account for it; that it implies, moreover, with tolerable certainty, the concurrent existence of disease in the outset of the aorta, and the consequent probability of hypertrophy of the left ventricle. Under this view we have two phenomena, which, instead of bearing to each other the relation of cause and effect, are common effects of the same cause—the cerebral hæmorrhage resulting from a peculiar kind of disease in one part of the arterial system—the augmented bulk of the heart from the same kind of disease in another part.

I have but one more remark to offer in regard to the case before us, and it is one which involves a caution of some practical importance. Our patient was bled, and the volume or fullness of his pulse was thereby reduced; but it remained *hard* as before. This quality which we call hardness of the pulse, and which is distinct from its force or momentum, is the quality by which, more perhaps than by any other, we are apt to be guided in directing the performance or the repetition of venæsection. But it may prove a very fallacious guide; it would be so in such a case as that we have been contemplating. With so considerable an augmentation of bulk and strength in the left ventricle, its individual contractions would be smart, and would give to the pulse at the wrist that character which, when the artery is at the same time full and large, is called strength—when the blood circulates through it in a slender stream, is called sharpness, or sometimes

wiriness. The caution that [I wish to impress upon you is, that (when the heart is so changed) no amount of bloodletting will correct this quality. You may bleed your patient to death, and yet the last pulsations of the arteries shall be hard. The state, then, of the pulse, taken by itself, is no safe warrant for the propriety of bloodletting, much less of its repeated employment. You will do well, therefore, in all cases where you are tempted to bleed because the pulse is hard—and especially in persons of advanced life, and most especially when the pulse obstinately remains hard after such an abstraction of blood as the case may otherwise seem to require—you will do right in all such cases to investigate by your hand, and by your ear, the condition of your patient's heart. Fortunately any notable degree of hypertrophy of the left ventricle declares itself by very plain signs. I have so often had occasion to point out these signs to your notice, that I need not again enumerate them now.

EXCISION OF THE HEAD OF THE FEMUR.

To the Editor of the London Medical Gazette.

SIR,

IN one of Mr. Earle's Clinical Lectures on Disease of the Hip-joint, lately published in your journal, the following passage occurs:—"Mr. Earle next suggested the possibility, in some rare cases when the constitution was sinking under the discharge, of excising the head of the femur, on a similar principle to the operations performed by Mr. Crampton and others on the knee and other joints. It was, however, to be feared that such an attempt would not be successful, from the diseased state of the acetabulum, which often remained a permanent source of irritation and abscess after the head of the femur had become perfectly ankylosed." It may be interesting to many of your readers to know that this operation has been successfully performed by Mr. White, of the Westminster Hospital. Mr. W. removed four inches of the femur in a very bad case of hip disease, which in all probability would have terminated fatally; the boy's health improved after the operation, and a very useful joint was formed between the upper extremity of the femur and the pelvis. The limb was not so much shorter than the other as might have been expected from

the length of the removed portion of bone. At the time of the operation the boy was, I think, about fourteen; he lived eight years after, having the perfect use of the limb, and then died of phthisis. The whole pelvis, joint, and upper part of the thigh, were removed, and are deposited in the Museum of the College of Surgeons. I have given these few facts from recollection; but possibly it may be the means of inducing Mr. White to enrich your pages with the details of this interesting case, and of others from the ample stores of his extensive experience.

I am, sir,
Your very obedient servant,
LIONEL J. BEALE.

Bedford Street, Feb. 29, 1832.

IMPERMEABLE STRICTURE OF THE URETHRA,

Treated by Perforation with the Lancetted Stilette.

By R. A. STAFFORD,
Surgeon to the St. Marylebone Infirmary.

Case of Mr. P., Surgeon, R. N.

As this patient was a member of the profession, I requested him to take the notes of his own case, which are as follows:—

“In March, 1807, I had the misfortune to be shipwrecked on the coast of France, on which occasion I was several hours immersed in the water, the weather being extremely cold. All the survivors were made prisoners of war next morning, and in a few days after we were marched off for the *Dépôt* at Verdun, a distance of about seven hundred miles.

I had contracted a slight gonorrhœa, which was rather exasperated by the fatigue of the march, and the privations we were necessarily exposed to, and amongst other symptoms slight ardor urinæ was present. Arrived at our destination, rest, in the course of a few weeks, entirely freed me from all remains of unpleasant feelings. About 1812 I perceived that the stream of urine had become much smaller than natural, and I had recourse to simple dilatation by bougies, which in a few weeks restored the lost calibre of the canal. Their further use was neglected until 1815, when the urethra had gained its former

small diameter. Dilatation was again resorted to, with the desired effect. The stricture, I ought to have observed, was situated about four inches from the meatus externus. I paid no attention to my complaint until 1819, when I began to give it my serious consideration, and was not a little alarmed at the probable consequences. In order effectually, as I thought, to eradicate the disease, I had recourse to the bougie armed with lunar caustic. At this time there was a slight stricture two inches and a half from the external orifice, and a second and chief one, one inch and a half further on. To this one, I applied the caustic on alternate days, for a fortnight; finding it of no avail, I then applied it every day, sometimes keeping it close to the stricture for fifteen minutes at a time. This practice I continued for another fortnight, without the least destruction of stricture; indeed I scarcely could feel any thing from its application. I then left it entirely off, and returned to the plan of dilatation, which I found more effectual, as I after a time could pass a number six bougie on to the bladder. After some time I neglected the bougies, but in 1821 found it necessary again to have recourse to their use. They were from this period, till 1828, employed occasionally, and irregularly, the strictures gradually getting narrower, and less under the command of the bougie; and when force was used, it invariably aggravated them for a couple of days after. The stream of urine was now not larger than a pack thread. It happened at this time that I was exposed to considerable fatigue on horseback, and to cold and wet; and twenty-four hours afterwards I was attacked with a most severe shivering fit, which continued for three hours, and then the hot fit succeeded, and finally a sweating stage, the whole occupying about eight hours. This was accompanied by a complete retention of urine. Next day, as well as on the third, the shivering fit returned, about the same hour as on the first, and ran the same course. General bleeding, leeches to the perineum, purgatives, hip and warm baths, enemata, and opiates, were all employed, and on the third day the urine began to come away guttatum, which in a few more improved to a very small stream. During the next two years, that is to say, till January 1831, I used from time to time the smallest bou-

gies, but at length the strictures became impermeable, and their employment made matters worse. The repeated calls to pass water in the twenty-four hours, the straining, the dribbling away of urine involuntarily, and the ever present dread of the evil consequences, rendered both mind and body truly miserable.

I heard of Mr. Stafford's new mode of treatment, and procured his work. The plan seemed so reasonable, and the results appeared so satisfactory, that I determined to put myself under his care. I accordingly came to town on the 21st of January, 1831. On examination, he found a stricture two inches from the meatus externus, admitting the smallest sized bougie: an inch and a half further on a second presented itself, quite impermeable. The perineum was leeches, hip bath used, and a purgative taken, preparatory to the operation.

23d.—He applied his single-lancetted stilette, cut through the first, proceeded on to the second, divided it, and carrying the instrument an inch still further on, he found a third stricture, which he in like manner divided. I had conceived the operation to be a most formidable, painful, and dangerous one, but the pain was trifling, not more than a couple of tea-spoonfuls of blood were lost, and not the slightest unpleasant consequence supervened. He then withdrew the stilette, and was able to pass a metallic bougie on to the membranous portion of the urethra, when it was arrested. To be as succinct as possible, I shall only remark, that from this date until the 4th of February, I went daily to his house, a distance of a mile, to have the metallic bougie introduced; I observed regimen, and took an occasional purgative, and hip bath; during this time the urine came away only in a small stream, owing to the remaining stricture in the membranous portion of the urethra.

Ten days after having been operated upon, I was obliged to return to the country, from which circumstance Mr. Stafford thought it advisable to postpone a further operation till a future opportunity. I continued, in the meantime, to pass a No. 9 metallic bougie twice a week through the strictures already divided, down to the membranous portion, where the remaining stricture was situated. I availed myself

of the earliest opportunity to come to town, and place myself again under his care. Accordingly, on the 5th of April, I waited on him—He passed a No. 8 metallic bougie at once down to the stricture in the membranous portion, when its further progress was arrested. He then tried to pass the smallest sized silver catheter through the stricture, and after some difficulty, fortunately succeeded. He at once determined on not losing such an advantage, and fixed the instrument to the penis in the usual way, so as to leave it in the bladder for some time. About a couple of hours after its introduction, feeling an inclination to pass water, I withdrew the plug, and made fully a pint, to my no small happiness.

7th.—In a couple of days the silver catheter was withdrawn, and replaced without any difficulty, by a gum elastic one a size larger. No unpleasant feelings in the parts.

18th.—Urine flows only in a very small imperfect stream, whence it is concluded, there is inflammation of the membranous portion, with perhaps enlargement of the prostate gland. Eighteen leeches to the perineum, hip bath, and a purgative medicine. Urine to be drawn off twice a day.

25th.—Can pass only about four ounces of urine at a time, in a dribbling stream. The first spoonfull is mixed with purulent matter, and the last spoonfull brought off by the catheter is charged in like manner. Two ounces of the decoction of pereira-brava has been taken thrice daily, for the last two days. Castor oil occasionally, to keep the bowels regular: appetite defective: sleep pretty well. The catheter used in the morning and before going to bed; it brings off about ten ounces of urine each time. A No. 10 metallic catheter passed into the bladder to-day.

30th.—The stream of urine not improving: the bladder seems to be regaining a little its expulsive power, as by a continued effort only five ounces of urine remain in it. The mucopurulent sediment is a little diminished: uses the gum catheter three or four times daily, to empty the bladder. Bowels require aperient medicine every second day: appetite rather defective.

May 2nd.—Dined with a friend yesterday: was induced to take two or three glasses of wine mixed with water. Spent

a most restless, feverish night, and have felt feverish and uncomfortable during the day. Took some castor oil. On passing a No. 9 metallic bougie on to the membranous portion, it caused a little pain, and on withdrawing it some hæmorrhage ensued;—a warning against such imprudence.

I can at present only pass the urine in a very small stream, and there is usually about six ounces left behind. This inability is owing in part to the feeble expulsive power of the bladder, induced by long habit of not acting efficiently, but unfortunately, in great measure, to enlargement of the third lobe of the prostate, as ascertained by examination. There was always left behind in the bladder from eight to ten ounces of urine, after each effort to expel it in the natural way, during the first fortnight after the bougie could be used, and it deposited a considerable quantity of muco-purulent matter: (for this I have taken a decoction of the pareira-brava, I think with benefit;) the bladder is, however, somewhat regaining its natural expulsive power, as, although the urine flows in a small stream, yet by prolonging the exertion to pass it, from four to five ounces are only left behind.

But the mind being in great measure freed from the ever present horror of retention of urine, and being now able to call in the aid of the catheter, life is rendered, under all existing disadvantages, comparatively happy.

[I heard from this gentleman some time after his return into the country, and his bladder had nearly regained its power. A No. 12 catheter could be passed without difficulty.]

LIQUID HEMOSTATIQUE*.

To the Editor of the London Medical Gazette.

SIR,

YOUR interesting journal publishes the opinions of an eminent physician of London concerning the use of styptics and liquid *hemostatique* in hæmorrhages,

which M. Talrich and I have been the first to discover. We therefore trust that you will not refuse to insert in your *Gazette* the observations that I have to make to Mr. Cæsar Hawkins, as the discussions they may occasion will doubtless be conducive to brighten the science and those who practice it, rendering, at the same time, the reading of your paper more interesting, and securing to us that justice and impartiality which we have a right to claim as being foreigners in this land of hospitality and encouragement.

In your number of the 21st of January last, you inform your readers that the experiment tried in the case of hæmorrhage on the sheep at the London hospital, that the hæmorrhage had remained itself the seventh day; to which we have to answer, that we saw the animal on the fourth day after the experiment, at which time every thing seemed to confirm the success of the operation; but on considering that the animal had been left in a cold damp place, we conceived some fear of the hæmorrhage's reappearance, as it afterwards did occur. Had the animal been cured, say you*, his complaint would have assumed the nature of an aneurism; but this would have been the only instance of the kind, as we have plenty of proofs to establish its impossibility. You therefore conclude by saying, *that the experiment tried at the London Hospital completely failed*, instead of saying that they had presented every appearance of success till the seventh day, when the animal died, which is no positive certainty of the failure of the experiment, and even were it so, it cannot be denied that the hæmorrhage was evidently removed and cured during the seven days that the animal existed.

Now as to Mr. Cæsar Hawkins's experiments, I will take the liberty of asking whether they are of so infallible a nature as to destroy the confidence placed in the virtue of our *liquid hemostatique*? Our experiments in Paris always proved invariably successful. Since we have been in London, we have only performed operations on six animals, in every one of which the hæmorrhage was radically removed and cured. Two of them died, to be sure, of something like a relapse, but to this we have

* It will be perceived from some of the forms of expression that the following paper is from the pen of a foreigner: we have, however, preferred leaving it unaltered, except in one or two instances where it was ungrammatical.—E. G.

* We published the report sent to us by a gentleman who witnessed the case.—E. G.

to say, that it may have been occasioned by circumstances or causes entirely unknown to us, as the animals did not remain all the time under our immediate care. It is also said that Dr. Jones has proved that the coagulation of the blood is the only obstacle that opposes the hemorrhagy, of which we are perfectly aware, but it ought not to be expected that our *liquid hemostatique* should stop the hemorrhagy by any other course than by that which nature itself would bring about. One thing, of which we are positively convinced, by our own experience, is, that our liquid hemostatique powerfully assists nature; first, by determining the coagulation of the blood; and, secondly, by forming the little clot of blood. It seems as if Mr. Hawkins wished to attribute the result or success that we have obtained to the contraction or retraction of the artery, but this argument will prove quite erroneous when it is known that we never extended our incisions so far as to cut the whole of the artery, fully convinced that had we done so it would have given ground to make out that the pure retraction of the artery would have caused the suspension of the hemorrhagy; but instead of so doing, we merely abided by the transverse incision and removal of substance required (as must be known by Mr. Hawkins) in such operations, well aware that the retraction of the artery would considerably increase the difficulty of the hemorrhagy; therefore it is of the most striking evidence that Mr. Hawkins has no plausible ground whatever for insinuating, as he does, that our *liquid hemostatique* only possesses a secondary virtue in the suppression of the hemorrhagy; and we would think ourselves extremely favoured if Mr. Hawkins would further his observations on our mode of practice, and particularly be pleased to explain to us what he means when he says that our liquid hemostatique only possesses a secondary virtue. If we are to understand from his comments that our liquid stops the effusion of blood, and occasions its clot, we will then agree to say that his opinion on the subject is right so far, for in this case the first and principal object is accomplished by the formation of the clot, which therefore proves at once that the nature of our liquid is *hemostatique*.

Let us now take a view of Mr.

Hawkins's method of operating in the stoppage of the hemorrhagy, in the most part of cases, as well in those where he attempts it *without the use of styptic as with it*. Previous, however, to our entering any further on the subject, I must say that Mr. Hawkins is completely in error when he says that we place a ligature through the skin. We have never made use of such a thing except in one single instance, and that only to comply with the request of Mr. Keate. Mr. Hawkins, in his first experiment, did but very imperfectly check the existence of the hemorrhagy, for the sort of long wound inflicted in the artery did not cease bleeding till fastened up by means of a suture seam. In the second experiment Mr. Hawkins says that I had much difficulty to find the blood-vessel, *which I only sought for after the failure of his attempt* to discover it, during which time the animal lost such a quantity of blood that it actually swooned away before I could perform that which Mr. Hawkins and I happened then instantly to be contending for, which was the stoppage of the artery. This, however, not having been attained in time, the state of syncope continued till the clot formed, the consequence of which was that the hemorrhagy stopped;—leaving, therefore, the practitioners reputation entirely open to public opinion, but probably well valued by those impartial persons who will no doubt take all the circumstances of the case into minute consideration. In the third experiment the hemorrhagy could only be stopped by the use of the suture seam. In the fourth experiment the animal died of hemorrhagy on the third day, notwithstanding the use of the suture seam. In the fifth experiment the wound was entirely sutured, but the animal died also of hemorrhagy on the third day.

Therefore these different results prove clearly that Mr. Hawkins has not been able to stop the hemorrhagy, though he has made use of *styptics*, and though he has attempted it by his own mode, which is *without styptics*, so that out of five experiments that he has tried the first case only turned out successful, and that owing to the rather long nature of the incision, and to the suture seam in the second experiment, I will say that if the hemorrhagy stopped as it did, Mr. Hawkins must certainly

not attribute it to his management, since it was occasioned by the *syncopal state of the animal*, which is known to every surgeon to be a hemostatique state. In short, in the three other experiments the animals died of hemorrhagy notwithstanding the use of *noix de galle*, of plain water, of Ruspini's liquor, of sutures, &c. &c. Mr. Hawkins concludes by saying, *that it is possible to stop the hemorrhagy of an arterial wound by pressure during the space of ten minutes*, as also by the application of small compresses, which may be dipped in a variety of liquids; but I will observe that it is not by such experiments as those above-mentioned that Mr. Hawkins can plausibly adopt such conclusions, for they would evidently be erroneous, when it is seen that out of five experiments that he has tried, not a single one of them turned out satisfactory, though he made use of the compresses he mentions, for he could not avoid having recourse to the suture seams in every case that he attempted. We know that *pressure*, a *coagulum*, &c. &c. can sometimes stop the hemorrhagies; but are there not cases in which such means cannot be made use of, and then is it not very painful to be in want of a remedy absolutely hemostatique, when all other means as yet known prove fruitless? Mr. Hawkins afterwards describes the effects produced by styptics in the stoppage of the blood; but here again he quite mistakes when he includes our *liquid hemostatique* in the number of styptics. Our liquid is not styptic, for it is not astringent; but, on the contrary, is *hemostatique*; for, as it is very justly acknowledged by Mr. Hawkins, it possesses the virtue of coagulating the blood to such an unequalled degree, that the surgeon who uses it has only to apply an imbibed tampon, or compress, to the wound, and it stops the hemorrhagy immediately, without even subjecting the practitioner to stain his fingers with the gush of blood that generally occurs in all other modes of proceeding.

Mr. Hawkins is perfectly right when he says that the *action of the styptic seems only a secondary one to that of the pressure on the parts to stop the hemorrhagy*. And again, I repeat it, our liquid is neither *styptic* nor *astringent*, but its virtue is that of coagulating the blood; and as to its nature, it is per-

fectly single, and bears no sort of similarity whatever to any preparation that may be known to Mr. Hawkins or to any other persons; and as to its efficacy, we have good reasons to believe that its virtues are far more extensive when applied to the human body than to that of a sheep, which is the animal that we fixed upon to try our experiment, as being of a nature to live on grass, and having a poorer and more limpid blood than those animals that live on flesh; therefore had the experiment been tried on a man, it is of all probability that the desired effect would have proved instantaneous and radical.

We will now beg leave to lay before the reader an exact statement of the experiments and results that we performed and obtained in Paris. We will proceed to do it without giving the names of the many distinguished surgeons that were present; being, therefore, certain of our success, and of the good faith and frankness of what we set forward to so enlightened a society as that of London, we have not the least fear of being contradicted either by our fellow-practitioners in Paris or by those not less eminent in London.

The incisions in the artery were longways, and described as follows:—

Nature of the Incisions.	Lived.	Died.	
Longways	11	11	0
Transverse	15	15	0
Deperdition or loss of substance	8	5	3
Pricked	1	1	0
Amputation	1	1	0
	36	33	3

It will be observed that we several times opened the carotid artery of one of the sheep thus operated on, and we performed an amputation on its thigh, which the animal survived, and four months and a half elapsed before we killed it to extract the artery, which we still have in our possession. I now come to the letter addressed to you by Mr. Hawkins, and published in your Medical Gazette of the 11th of February, 1832; and we will say for ourselves that we are by no means ignorant of the experiments of Dr. Jones, as Mr. Hawkins seems to think it, for

we agree that these experiments, on some points, have thrown a degree of light, particularly concerning physiology and pathology; but relative to surgery, I will ask Mr. Hawkins who is the surgeon that would have recourse to the use of an external suture seam on the artery, to stop the hemorrhage? Has Mr. Hawkins ever performed the like on a man? Should he have done it, or have an opportunity of doing it, we would feel very grateful to him for his notice to allow us to witness it.

He now says that there are many persons who ought to know more of the principles of treating and stopping hemorrhagy, who have been deceived by being induced to place confidence in the *soit disant* or *supposed efficacy of Messrs. Tabrich and Halma-Grand's styptics*, &c. We will merely reply by this plain and plausible argument—why did not Mr. Hawkins's method of operating prove satisfactory in the five experiments that he performed, and of which *only one* seemed to exhibit some kind of success? We will, however, not enter any further on the subject of Mr. Hawkins's *rather too direct insinuations* of our having attempted to speculate on the credulity of a public well known for its encouragement and lenity to foreigners and their productions. How could we ever have come forward with such views (admitting even that we had no regard for principle or reputation), when all our demonstrations and experiments have been performed, and are still offered to be performed, publicly; in presence of a scientific body of practitioners, and, in fact, in presence of any one that may appear to us capable of valuing the advantages and relief that our discovery offers? Therefore we shall drop the argument, and wait the result that will doubtless by and by constitute the opinion of the high-minded and impartial society in the midst of which we stand, and to which we confidently appeal; observing again that our liquid hemostatique is *not of an astringing but of a coagulating nature, that clots the blood without any incorporation whatsoever*. We will therefore conclude by saying, that we hope to find so distinguished a man as Mr. Hawkins more impartial in future, particularly if he would have the kindness to use his powerful influence to assist us in pro-

curing opportunities to further the performance of our experiments, which, no doubt, would convince him that our *liquid hemostatique* is quite a new thing; that its power is positive, its virtue very great, and, in fact, a very beneficial discovery for humanity; which motives, we trust, will be justly appreciated by so eminent a professor as Mr. Hawkins. We will now lastly say, that we will always be found ready to witness or perform any experiment that may be suitably requested of us; adding, at the same time, that all our proceedings will be guided by *honour, integrity, and energy*, feeling ourselves perfectly entitled to assume the title of medical men; therefore we will not be found to draw back from any circumstance or scientific discussion whatsoever that we may be offered to participate in or originate. Should this appeal to our fellow physicians of Great Britain in general, but more particularly to those residing in London, remain unnoticed, we would consider it a very unfriendly act, and as a proof of their inability to co-operate with us in the search or practice of such scientific discovery as ennobles the mind and assists humanity. Meanwhile, I have the honour to remain,

Sir,

Your most obedient

humble servant,

HALMA-GRAND, M.D.

Professor of Anatomy to the University of Paris.

32, St. James's-Street, Piccadilly.

[We have given insertion to the above, chiefly because it is intended as an answer to the paper by Mr. Hawkins which appeared in this journal. Dr. Halma-Grand is probably not aware of the discreditable light in which, in this country, any one professing to have *secret* remedies must be content to stand; and we may add that had he not been a foreigner the circumstance alluded to would have prevented us from giving a place to his letter.—E. G.]

SMALL-POX AFTER VACCINATION.

To the Editor of the London Medical Gazette.

SIR,

IN some remarks upon vaccination, by Dr. Howison, in your number for the

14th January, there is a letter from Dr. Sanders to Dr. H., in which Dr. S. maintains "that the real small-pox never occurred, either in its complete form, or under any modification, in a person who had undergone vaccination." I am aware of the great difficulty of deciding this point. It must, however, be allowed that authors of great eminence have stated their belief in, and even given instances of this occurrence. Yet Dr. Sanders tells us "that in the writings of the anti-vaccinists, as well as in those who believe in the occasional failure of vaccination, there is not one instance of the small-pox." In his quotation from the *Dictionnaire des Sciences Médicales*, I presume he has written *varicella* for *variola*. Will the following case, which I watched with peculiar care, and now extract from my note-book, for the first time, afford him any satisfaction?

--- aged 22, when a child, was vaccinated at the dispensary, and from strict inquiry, seems to have undergone the disease very favourably. The marks still remain very large and distinct, one on each arm. On the 20th May, 1831, was seized with febrile symptoms, accompanied with vomiting and headache.

21st.—Fever more considerable, face flushed, tongue foul, eyes red, much thirst.

22d.—An eruption appeared this morning, first on the face, then on the neck, arms, and chest. In the evening it extended over the trunk and part of the thighs and legs. Eruption resembles small inflamed acuminated pimples.

23d, 2d of eruption.—The entire body now covered, and the eruption is very thick on the under extremities and parts liable to an accumulation of heat, as in the groins. Very thick on the face. Pimples of yesterday have a whitish clear small vesicle at top.

24th, 3d of eruption.—Pustules still thicker over the body. Those of yesterday more opaque, white fluid at top, surface plain. One or two, which have been rubbed during the night, broad, depressed, and out of all character, especially two on the left thigh, one of which shews, as it were, a large perforation in the centre.

25th, 4th of eruption.—Pustules of a dirty pearly whiteness to within one-third of the base; plain at top; suppuration distinctly marked.

26th, 5th of eruption.—Pustules still

flatter at top, and several of them coalesced; those on the face very much so, forming a thickish yellowish green exudation, or soft crust.

27th, 6th of eruption.—Pustules of the face more coalesced, and thin crusts forming.

28th, 7th of eruption.—Vesicles on the body becoming turbid; face somewhat swollen; throat sore, and considerable difficulty of swallowing; eyelids shut up.

29th, 8th of eruption.—All the symptoms regularly progressing; inside of the mouth very red and sore; sleep much disturbed.

30th, 9th of eruption.—Restlessness much relieved by opiate.

31st, 10th of eruption.—Feet beginning to swell; pustules here of a very dark colour; throat better; secretion of the mouth becoming very thick and viscid.

June 1st, 11th of eruption.—Feet very sore, and considerably swollen; pustules of the soles of the feet have a bruised appearance; feverishness, which had much subsided, again returning.

From this time the disease went on in a regular form, every symptom being minutely compared with what generally occurs in common small-pox; but merely to look at the patient was enough to convince any one of the genuineness of the disease. You will observe, also, that the above was not a very mild case, the pustules being on many parts confluent, and the secondary fever severe. I am also persuaded that had it not been for the previous vaccination, the disease might have proved fatal, as it shewed more than one symptom of a malignant tendency.

What I understand by a modified form of small-pox, in contradistinction to Dr. Sanders, is not that it is a new disease, compounded of something between cow-pox and small-pox, but the genuine small-pox in a mild or softened form, and this is corroborated by Dr. Sonderland's view of the identity of the two diseases.—I am, sir,

Your most obedient servant,
JAMES RANKINE, M.D.

Hanley, Feb. 6th, 1832.

PAPERS AND CORRESPONDENCE

ON

CHOLERA.

OPINION OF HUFELAND

ON THE

ORIGIN AND PROPAGATION OF
CHOLERA *.

THE cholera is originally the product of atmospherico-terrestrial re-action; but it also, in its highest degree of development, generates a contagion which may be communicated from person to person. It thus unites miasma and contagion, aerial and human infection. It is propagated from place to place in two ways; the one by progressive atmospheric infection, which particularly follows the course of rivers, as was recently shewn in the communication of the disease to Berlin, which did not take place overland, but by the water course of the Warte, the Finnow Canal, and the Havel; the other by infected persons and things, whereby it is especially to be observed, that the infection is extremely conditional, and therefore rare, very few patients attaining the height of the infective power of the disease, and very few persons possessing the requisite susceptibility for taking the same.

To me it is, in fact, inconceivable, how among physicians there can be any dispute on this point, or how they should, as in some instances, have been divided into two parties, contagionists and anti-contagionists, standing in hostile opposition to each other. Is it not a long-recognized truth, that a disease may originate from epidemic influences and then develop a contagion, and that the same disease may at the same time be produced by the atmosphere, and by a contagion? No one doubts that catarrh may be epidemically produced, by generally-prevailing damp cold air, and as little that an individual labouring under the highest degree of catarrh may, by a kiss, impart it to another. Is not this equally the case with scarlet fever, with dysentery, and with whooping-cough, &c.? Taking into consideration

all that experience has hitherto proved in Prussia, and here in Berlin, the whole may be summed up in the few following conclusions.

First, The cholera may be received either from the air, or by infection.

Secondly, The first mode of communication is much more frequent than the last.

Thirdly, To take it in either way, a peculiar susceptibility of constitution is requisite.

Fourthly, This susceptibility may be produced, or favoured, by certain influences which are under our control; these are, overloading the stomach; the use of sour, flatulent, fermenting, cold, indigestible food and drink; over-indulgence in spirituous liquors; taking cold, getting wet, residence in damp air, and depressing emotions. Experience has, in innumerable instances, shewn that immediately after the operation of such powers, (*potenzen*) cholera has shewn itself; while, on the contrary, no example exists in which, without such previous operation, cholera has taken place.

Fifthly, The prevention of cholera, by keeping off the infectious matter, is only partially possible, as the communication thereof through the atmosphere cannot be prevented at all, and that through personal contagion, can only be prevented in part. Still, the measures adopted by Government are laudable, and deserving of our thanks.

Lastly, The most effectual method, therefore, of protection against the disease is, preventing the susceptibility thereto by avoiding those causes which favour it.

Berlin, Dec. 1831.

PROPER MODE OF EMPLOYING
GALVANISM IN CHOLERA.

To the Editor of the London Medical Gazette.

March 5, 1832.

SIR,

IN the last number of the Medical Gazette two cases of cholera are mentioned, in which galvanism was employed without benefit. My attention from various circumstances having been directed to the employment of galvanism

* Translated from the original German MS. in our possession. — E.G.

HEATED MATTRESS.

To the Editor of the London Medical Gazette.

Royal Military Asylum, Chelsea,
February 21, 1832.

SIR,

I BEG leave, through the medium of your valuable journal, to submit to the notice of the profession a *tin mattress* which I have had made, for the purpose of applying dry heat, in an easy and expeditious manner, to a patient affected with cholera. It can be manufactured at a moderate expense—about 38s.

It is made of sheet-tin, is $5\frac{1}{2}$ feet long by 2 feet wide, 3 inches deep at the sides, gradually sloping to 2 inches in the middle, thus rendering it concave, the better to suit the back of the body; and has a foot-plate, 12 inches wide and 10 inches high, against which to place the feet. It is also very light and portable. This mattress is to be placed on a bedstead (a boarded one is best, wood being a bad conductor of heat); the patient is to be laid between blankets upon it, and it is then to be filled with *hot water, steam, or hot air*. The two latter are readily obtained by means of an apparatus containing a spirit-lamp, &c. sold by most furnishing ironmongers in London. This may be done while hot water is getting ready.

A regular and great degree of heat is thus applied to the back and feet of the patient, while friction and other means of giving heat can be employed on the front of the body.

The moderate degree of depth of the mattress—viz. three inches—is found quite sufficient; and it is obvious a less quantity of hot water is thus required.

I would recommend that several of these mattresses be kept ready at the places appointed for the reception of the poor when seized with the present prevailing disease.

I am, sir,

Your obedient humble servant,

S. G. LAWRENCE.

in the cure of diseases, I can without hesitation say, that these cases leave the question of its utility in cholera precisely where it was. Neither the manner of employing the galvanism, nor the circumstances under which it was employed, afforded any chance of its success. At the time it was employed the congestion of the vital organs had been established. It acts only as a stimulus on the vessels, and here could have no effect on them, because their excitability was lost; and without so exciting the vessels as to relieve the vital organs from the congestion of black blood, no healthy action could be excited in them. After this congestion has to a certain degree taken place, the healthy nervous influence itself, could it be supplied, would equally fail, for it also acts only as a stimulus to the vessels, supplying no part of their power, which depends on their own mechanism. The nervous power can only co-operate with the powers of circulation. When these are finally lost, its functions necessarily cease. Now galvanism can be successful on no other principle but that of supplying the want of nervous power, whose functions it has been found capable of performing.

Then, as it happens in extreme cases of cholera that many and sometimes all the vital organs are congested, what hope could there be from sending the galvanic influence in one direction through the chest? Could the fatal termination be averted by restoring the function of any one of these organs, were it possible to restore one while the functions of the rest were suspended? To be successful, galvanism must be employed before the congested state of the vital organs is established; and its application must be general, not to any one, but all the vital organs.

I am, sir,

Your obedient servant,

MEDICUS.

P.S.—It was said, in its first application at Haddington, to have been of great service; it was also of service in the case detailed by Mr. Finlayson. It is probable that it was on these occasions employed more properly, at least as to the period of the disease. But if the principles of its proper application were unknown, its benefit would soon be lost.

[A contrivance, very similar to the above, has been adopted in Edinburgh. It is thus described by Dr. Christison:—“A set of steam mattresses have been ordered, five feet seven in length, and two feet wide at top, and narrower at bottom; slightly curved to fit the back,

and composed of double tin-plates, three inches apart, which are kept firm by wooden supports inside. They are heated by a boiler, a gallon in capacity, which is placed on the ward fire, and from each of which a tin tube is led to two or three of the nearest beds. The steam enters by the head, and issues by a tube at the bottom when the case is fully charged. It has been ascertained, by experiment, that a bed may be sufficiently heated in four minutes, and raised to 200° in twenty minutes; and that then the adjoining bed may be heated to the same degree in even less time. This apparatus, which has been arranged by Dr. Mackintosh, is, I have no doubt, the most effectual mode of heating cholera patients yet devised. The cost of each mattress, with its apparatus, will be 4l. 4s. to the public *.]

CROTON OIL IN CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

CONVERSING the other day with my friend, Mr. Tegart, late Inspector General of Hospitals in the West Indies, on the all-absorbing question of cholera, he said, from the benefit he had found to arise from the croton oil, in some of the tropical diseases, which, running a rapid course, require the promptest measures of treatment, and still more from its influence in relaxing the spasm attendant on the endemic malady of some of the islands, called there "the dry belly-ache," and, from its admitting of being effectually administered when other remedies could not be swallowed, or would be rejected, he should like to see it tried in cholera. Strangely enough, I met, two days afterwards, a non-professional friend, lately returned from holding a high civil appointment at the Mauritius, who remarked—"I hear nothing, among you in England, of the croton oil, or tobacco clysters, as remedies for the cholera: they were the only things our medical men found effectual in the thousands who had the disease with us; the first in clearing the bowels, the other in relieving the dread-

ful spasms and cramps." Have any of your readers tried these remedies in cholera, or *will* any of them try them?

Yours, in haste,

JAMES BARTLET, M.D.

Benlinck-Street, March 1, 1832.

[The tobacco clysters have been tried, and *said* to be of service: we cannot help greatly doubting their safety.—E. G.]

CHOLERA (?) AT KENSINGTON.

To the Editor of the London Medical Gazette.

12, Lower Phillimore-Place,
Kensington, February 28, 1832.

SIR,

I HAVE been almost daily questioned for the last week, by some of my patients and others resident here, relative to three cases, which had been considered of a very suspicious character, and which appeared in this place towards the latter part of last week. As statements have been circulated here, tending to influence the parish authorities against me for the notice which I took of them, I have taken the liberty of enclosing an exact detail of the symptoms, as they presented themselves to me, and of the treatment which I pursued.

The parish, I understand, had previously determined, at a meeting of its trustees, to engage a house as a receptacle for cases of the prevailing disease; nevertheless, some individuals, I am credibly informed, are disposed to accuse me of being the originator of an expense to which, had the cases occurred to one of the older established medical men here, say they, they would never have been subjected. I trust that the steps which I took, so far from exciting the *panie* of which so many speak in such violent terms, and against the originators of which so many voices are found ready, will be considered the best calculated to prevent alarm. But the truth is, I was a *nouveau venu* here; the cases had not terminated fatally, but were in a fair way of recovery before mention was made of them to any individual; and because the people had not died, *ergo*, they could not possibly be cases of *cholera*. On the 10th I was called in very hurriedly to the man Wilkins, who, I was told, was just dy-

* Edinburgh Medical and Surgical Journal, Supplementary Number.

ing; they came for me after being unable to meet with the parish surgeon, and two other medical gentlemen upon whom they called. I found him labouring under the symptoms which I have detailed, and I employed the treatment which I have set down. Until Saturday evening things were going on well; I had visited him on the evening of that day, and had scarcely reached home before the girl, whose case accompanies that of her father, was brought to my house. I was startled by her screams, and on reaching the lobby, where she was sitting down, supported by the woman who had come with her, I found her as I have described in her case. The treatment which is detailed was pursued with her, and my assistant and I left her, about half-past one on Sunday morning, somewhat relieved, after having been in the house nearly four hours. At seven in the morning, my assistant was called up to a brother of this girl, who had been attacked with spasms of the limbs, great pain of bowels, and frequent retching: he ordered fomentations to be used, and directed some calomel and opium to be given to him. Now, sir, I beg to state to you, that, until the Sunday evening, no person, save my assistant, knew any thing about these cases; but, from the three having occurred with uniformly similar symptoms, and having succeeded each other (especially the two last) so speedily, I thought it now demanded that some notice should be taken of the matter, to prevent any imputation of blame to myself for secrecy, in the event of the disease spreading. I went in search of an experienced and much-valued medical friend, whose advice I intended to take as to the proper steps to be adopted. Himself I could not see; but met with a relative, also a medical man, of thirty odd years' experience in his Majesty's service (and who must have been conversant with the disease which is now spreading its ravages so extensively in this country, or he would not assuredly have received an order to hold himself in readiness to take charge of a district as inspector); this gentleman declared, on seeing the cases and hearing my statement of their first appearances, that "they were *well-marked* cases of cholera." This being stated to a mutual friend here, he urged the propriety of my mentioning the circumstance to some of the parish authorities;

and he very kindly offered to accompany me to one of them. I related to the person upon whom we called the exact state of the case—that the father had been under my hands since the preceding Thursday, and was in a very fair way of recovery; that his daughter was still dangerously ill, but that I did not at all despair of her safety; and, in fact, that I thought no alarm should be excited on the subject, as, from every appearance, all three would soon be well. I was requested to attend a meeting of the trustees of the parish, to be held the next morning at the workhouse; at which, accordingly, I was present; and judge of my surprise, sir (before I had been called upon to state the particulars of the case), at finding a written paper presented, and which the author requested might be read, wherein he said, "that he had seen the case (of the father) which had given rise to the unnecessary alarm in the place; that it was not cholera, but *epilepsy*; and he begged to assure those present, that the alarm had been perfectly unwarranted, there not being the slightest grounds for any whatever." The paper proceeded to detail the symptoms of epilepsy, but which, most assuredly, were *not* the symptoms of the case to which I had been called. The gentleman who handed in this paper, officiates, I understand, as the parish surgeon, and had visited the father *two hours and a half* after I had, and after re-action had been *completely re-established* by the various means which I had employed. Dr. Gilkrest saw these cases on Monday afternoon, two days after the attack of the last case, and five days after the attack of the father; still, from what he learned and saw, he said that they were decided cases of the prevailing disease. Dr. Daun saw these cases the next day, the *sixth* day of the disease, and, although at this period he could not find any thing *decidedly* indicative of cholera now present, he did not hesitate to say, from what he heard, that they were, "in all probability, *mild* cases of the disease, and *quite* sufficient to excite alarm." Dr. J. Johnson saw them the *next day* (the seventh); he said, that "there was *no cholera at that time*,"—no, *certainly*; but he said also, that it was a remarkable feature about the Indian cholera, that very frequently, if it did not prove fatal on the *second day*, on

the *fourth* the people might be seen on their feet again. To several of my medical friends I have related the particulars of these cases, and to men who now begin to be looked upon as veterans in our profession; and they unhesitatingly declared, that if the cases reported in the public journals were considered cases of cholera, then *unquestionably* these were also that disease.

Such, sir, is a true statement of facts; all that I am desirous of is, to know the opinion of my professional *confrères*, relative to the line of conduct which I adopted on this occasion. There cannot, I think, be any question of the disease here prevailing at the present time being a fever, with an unusually severe cold stage; and that it is during this stage, when there is evidently so completely paralysed a circulation and respiration, that bleeding will be found, in proportion to the earliness with which it is practised, of more or less certain benefit. I have long had an opportunity of judging of the paramount advantage of bleeding in the congestive stage of typhus, among the hundreds of destitute and miserable Irish emigrants who yearly arrive in Canada from this country, and among whom, from their wretchedness on board ship (being crowded together in immense numbers), and the scantiness and unwholesomeness of their food, fever almost invariably breaks out among them before, or immediately on, their arrival in that country. The plan pursued in the Montreal General Hospital, was first to bleed and then to plunge them immediately into a hot-bath; and so generally superior was this mode of treatment found among that class of patients, that it was almost without exception ordered on their admission into that institution.

Should space permit it, sir, in your pages, you will greatly oblige me by inserting the cases which I have ventured to enclose, with the reasons assigned for my wish to have them published.

I am, sir,

Your very obedient servant,

FRANCIS BADGLEY.

CASE I. — Wilkins, æt. 17, 18th February, 8½ P.M. was attacked with very acute pain in the left side, when walking down Church-Street; described it as similar to a blow from a closed fist; was completely prostrated by the seve-

rity of the pain, and would have sat down, but for a woman who accompanied her to my house, whither she had been coming for medicines for her father when she was attacked. While in the lobby, her body became almost doubled, and her cries were so loud as to alarm the house; the spasms were constant; retching, and vomiting of frothy mucus, incessant; respiration very low; pulse thready, and scarcely perceptible; begged that she might not be moved. The following mixture was given to her, and it was advised that she should be immediately carried home:—

R Tr. Opii, ℥x.
Sp. Ammon. Arom. ʒj.
Tr. Lavand. C. ʒij.
Mist. Camphor, ʒi. M. st. sumend.

As soon as possible after her reaching home, to which my assistant and myself hurried after her, a hip-bath was got ready, and while there a vein was opened, from which about twelve ounces of blood were taken; at the same time a table-spoonful of brandy, with twenty drops of laudanum, were given every half-hour. She complained of intense pain over the entire abdomen from the epigastrium. Fomentations were applied during the whole time she was in the hip-bath, and subsequently to her being replaced in bed. About an hour after her being wrapped up in the blanket in bed, the spasms and tremors diminished, a gentle perspiration broke out over the surface of her body, and she fell into a quiet sleep.

1. R Hyd. Submur. gr. vi.
Pulv. Opii, gr. ij. M. ft. Pil. ij.
stat. sumendæ.
2. R Tart. Potass. ʒss.
Tr. Hyosciam. ʒj.
Sp. Æther. Nitros.
— Ammon. Aromat. aa. ʒiss.
Infus. Sennæ, ʒij.
Aq. ʒiiss. M. ft. Mist. cuj. cap.
4tam part. 3tiis horis.

19th, 7 A.M.—Has slept for about four hours, although disturbedly; and when she awoke, complained constantly of pain of the abdomen and head. Has vomited once, about two ounces of fluid, much tinged with bile; feels very low and faint; pulse small and quick; bowels twice opened; passed no urine, but has considerable pain in the hypogastrium; eyes suffused; face slightly yellowish; tongue furred. At this time she was seen by my assistant, who had

been called up to her brother, he having been attacked, about half an hour after getting up, by violent pain at the epigastrium, extending over the whole surface of the abdomen; incessant retching; coldness of the surface; scarcely perceptible pulse; suppression of urine; limbs drawn up to the body by spasmodic contractions.

The following powder was given to him, after fomentations had been employed:—

R Hyd. Submur. gr. v.

Opii, gr. ss.

Pulv. Jalap. gr. vi. M. ft. Pulv. stat. s.

10 A.M.—Forehead and temples yellowish; eyes sunken; pupils dilated; tongue white and moist; skin dry and hot; pulse 90, hard and quick; passed about two ounces of high-coloured urine, and with considerable pain; tenderness at the epigastrium; no return of spasms or tremors since 4 A.M.; had two stools, very fetid and dark olive coloured; considerable thirst; pain over the eyes; slight delirium.

App. Lotio ex Aqua et Acid. Acet. front.

Cont. Mist. et add. Tart. Antim. gr. j.

Beef Tea and Orangeade for drink.

9 P.M.—Feels better; more tenderness at the epigastrium, but less over the other parts of the abdomen; less pain of head; less thirst; considerable perspiration on the whole surface; pulse more full, and soft; tongue white.

Cont. Medic. et Lotio.

20th, 11 A.M.—Continues improving; has perspired freely; no retching nor spasms; has passed water comfortably this morning; bowels twice open since last night, very dark coloured; pain at the epigastrium much diminished; less yellowness of skin; tongue still white; pulse full and soft.

R Nitrat. Potass. ℥ij.

Liq. Ammon. Acet. ℥j.

Træ. Hyosiam. ℥iss.

Spts. Æther. Nitros. ℥j.

Infus. Sennæ.

Mist. Camphor. aa. ℥iiss. M. Ft. Mist.
capt. 4tam part. Suis horis.

21st.—Convalescent.

R Tart. Potass. ℥iss.

Pulv. Ipecac. gr. iss.

Træ. Hyosiam.

Spts. Æther. Nitros. aa. ℥iss.

Infus. Gentian.

— Sennæ.

Mist. Camphor. aa. ℥ij. M. Ft. Mist.
capt. Cochlear. iij. mag. 4ta q.q. horâ.

22d.—Complains of weakness; no pain of head, or over the surface of abdomen; bowels open once; no dysuria; skin soft; pulse full and soft.

Cont. Mistura.

Beef tea, or mutton broth.

From this time forward the girl daily recovered her strength; she continued the tonic mixture until the 23d instant, when she did not appear to require any further medicines.

CASE II. The boy was slightly attacked in comparison to his father and sister, but the symptoms were similar although less violent. Calomel and opium, with a saline mixture containing tart. antim. were the only medicines which he had, until for the debility bitters were ordered during two days.

CASE III. John Wilkins, æt. 47, gardener.

Feb. 16th, 1 P.M.—The surface of the whole body perfectly cold and anæmial, giving it a deep purple hue; the skin of the hands and fingers corrugated as by long maceration; the nails blue; all the *fluxurs* of the extremities rigidly contracted by spasm, and the recti muscles of the abdomen prominent from the same cause; the abdominal surface hard as a piece of leather. The head drawn backwards and inclined to the left side; the jaws spasmodically fixed, with the mouth half-open; the tongue covered with a thick coat of white, cold and clammy; great anxiety of countenance; sunken eyes; extremely low and hurried respiration; inability to swallow; frequent spasmodic efforts of the stomach to reject the most trifling fluids administered, and a small quantity of frothy mucus discharged after each fit of retching. The action of the heart scarcely perceptible; the pulse only felt by using the greatest attention, twenty beats in the minute, and resembling vibrations of a minute thread; great pain over the whole surface of the abdomen, extending along the course of the latissimus dorsi to the nape of the neck,—of this he constantly complained most bitterly, on the hand being placed on the abdomen; *suppression of urine*. The voice at first was not very much

affected, but within half an hour of my seeing him, and previously to his being bled, it was converted into a very low whisper; *no purging*, but the bowels had been freely open in the morning before leaving home. No means being at hand for a general or hip bath, I directed four women to use incessant frictions over the whole body, and another was engaged in applying cloths, wrung out of almost boiling water, to the epigastrium. A table spoonful of brandy was given every twenty minutes, the last of which he had after I had opened a vein, which I did within half an hour of my being in the house. On opening the vein scarcely a drop of blood could be obtained; but by plunging the arm in a bucket of hot water, and moving it in different directions, as far as the spasmodic state of the muscles would permit, I succeeded in obtaining, after more than half an hour had been spent in the effort, about twelve ounces of dark-coloured blood. The pulse rallied somewhat; the spasms diminished so much that the limbs resumed their natural position, and the head returned to its proper direction. I now ordered ten grains of calomel to be given to him, and he was directed to have some beef tea, or gruel.

Four P.M.—Found him in a hip bath (which had been brought in from a neighbour's), the coldness having returned, and spasmodic action again manifested itself; the respiration was quick and forced. On his being replaced in bed, finding that the pulse and breathing shewed such laborious action, the vein in the right arm was freed of its bandage, and another in the left arm was also opened, and both were made to flow simultaneously. About twenty ounces of blood were received from the two apertures. The blood flowed more quickly, certainly, than on the former occasion, but still in a very small stream. No evacuations from the bowels or bladder.

R Sulph. Magnes. \mathfrak{z} j.
Tart. Antim. gr. j.
Infus. Sennæ, \mathfrak{z} vss. M. St. 4tam
part. 2dâ q.q. horâ.

Nine P.M.—Better; the breathing much relieved; profuse perspiration over the whole surface; pain referred to the right side of the abdomen, under the hypochondrium; pulse full and soft, 70; great thirst; tongue covered with

thick fur; complains of dizziness of sight and double vision; some delirium; vomited about an hour ago between four and five ounces of bilious fluid.

Empl. Lyttæ Nuchæ.
R Hyd. Submur. gr. iv.
Ext. Colocynth. gr. viij. M. Ft. Pil.
ij. stat. sumend.
Cont. Mist. Cathart.

17th, 4 A.M.—Complains of excessive lowness; the whisper is scarcely audible; perspiration still profuse; pulse low and weak; medicine has not acted.

Habeat. Enema Domest.
R Tart. Potass. 5vj.
Spts. Ammon. Aromat \mathfrak{z} iss.
Infus. Sennæ, \mathfrak{z} vss.
Aq. Cinnam. \mathfrak{z} j. M. Capt. 4tam part.
2dis horis.

Eleven A.M. — Feels considerably easier; respiration is free; pulse 90, full and soft; skin soft; perspiration gentle; had three copious, dark olive-coloured, and very fetid, evacuations; passed about an ounce of urine of very high colour; feels no pain, but there is tenderness on the right side of the abdomen; blister rose well, and has discharged freely; the blood neither buffy nor cupped.

Mutton broth, or beef tea.

Eight P.M.—Continues better; bowels have not been acted upon since the forenoon; has still some dizziness of sight.

R Submur. Hyd. gr. iij.
Pulv. Jalap. gr. vi. M. St. sumend.
R Sulph. Magnes. \mathfrak{z} j.
Tart. Antim. gr. ss.
Spts. Æther. Nitros. \mathfrak{z} ij.
Infus. Sennæ.
Aq. Cinnam. aa. \mathfrak{z} ijj. M. Capt. \mathfrak{z} iss.
3tiâ q.q. horâ.

18th, 7 A.M. — Exceedingly low; pulse small, and very compressible; perspiration copious; very anxious countenance; perfectly sensible. My assistant gave him some spts. ammon. aromat. twice during the space of an hour, while he remained with him, and he left him in a quiet sleep.

Ten A.M.—Very low, but not so much so as in the morning; pulse 60, very small and weak; tongue thickly coated with white fur; considerable thirst, and complains of being sick, but no retch-

ing; passed about three ounces of urine since last night.

R Confect. Aromat. ʒij.
Spts. Æther. Nitros.
Sp. Lavend. C. aa. ʒiss.
Træ. Hyosciam. ʒj.
Carbon. Sodæ, ʒij.
Infus. Gentian.
Aq. Cinnam. aa. ʒij. M. Capt. ʒtiam
part. ʒtiâ q. q. horâ, in statu effervescentiæ, cum Acid. Tartarico.

Eight P.M.—Much better; says he feels alive again; no pain; no sickness; made water twice, and freely; bowels not opened; pulse 80, fuller, and more natural; skin comfortable.

Repet. Mist. Effervescens. sed pro Confection. Aromat. immit.
Tart. Potass. ʒij.
Et pro Aq. Cinnam.
Infus. Sennæ.
Mist. Camphor. aa. ʒiss. 4tam part.
ʒtiâ q. q. horâ.
Hab. stat. Pil. Hyd. gr. iij.
Pulv. Rhei. gr. iv.
Pulv. Ipecac. gr. j. M. in pil. ij.

From this time this man went on perfectly well, his convalescence becoming daily more established; and the mixture prescribed for the daughter on the 21st, was the same that the father took from the 19th up to the day of my discharging him completely cured, with the exception of a little weakness.

CASES OF CHOLERA IN ST. PANCRA, WITH SOME OBSERVATIONS.

To the Editor of the London Medical Gazette.

SIR,

At a moment when not only the medical journals, but the press generally, is deluged with cases of cholera, and writers of all denominations are pressing forward to obtain public notice, or, more laudably, to contribute to the scanty stock of knowledge possessed on many points connected with that formidable malady, I feel reluctant to encroach on those pages which should be, and I believe are, held sacred to public usefulness; nor, indeed, would I present my observations to you, but in the hope that

they may tend to elicit such information on the pathology of the disease as can be obtained only by carefully-conducted examinations of the bodies of such as become its victims. It must be quite evident, that well-authenticated facts are the only convincing proofs that can be adduced of the identity of a disease whose nature, not less than its existence, has been stoutly questioned and denied; and it must therefore be of the utmost importance that no impediment should be thrown in the way of those ardent professional men who feel desirous of instituting investigations of the bodies of such individuals as die under suspicious circumstances, especially as we are taught to expect certain unequivocal appearances characteristic of cholera after death, although, during life, the history and symptoms of the disease may appear somewhat paradoxical.

Without offering any comment on those theoretical points which have disunited the medical profession to its manifest humiliation in public esteem and confidence, I beg to be allowed briefly to revert to some cases which I have had an opportunity of witnessing during the course of the malady, and of examining after death; and I do so, because the conviction impressed on my mind is, that, by repeated investigations, new facts will be brought to light, at first apparently of little moment, but leading to discoveries bearing, in an important degree, on the successful application of remedial agents.

The first patient was a woman, habituated to excessive indulgence in ardent spirits. About eight o'clock A. M. she felt generally unwell; but, as she had been under the influence of liquor the preceding evening, no notice was taken of the circumstance until eleven o'clock, when some trifling domestic medicine was applied for her relief; subsequently disorder of the bowels ensued, without sickness, but with coldness of the surface, slight blueness of the fingers, and no passage of water. She died at eleven o'clock A. M., fifteen hours from the commencement of the attack.

The post mortem appearances were as follows:—

Head—not examined.

Heart—healthy in its general structure, except slight hypertrophy of the left ventricle: the *right* auricle and

ventricle contained a large quantity of partially coagulated blood, of a thick quality, and very deep colour.

Lungs—both healthy, crepitated freely, not overcharged with blood, but that which was pressed out was of a dark colour.

Vena cava—distended with dark blood.

Stomach—a blush of inflammation on its lining membrane; it contained about four ounces of a fluid resembling biliary and feculent matter.

Intestines—omentum injected, colon contracted, so as to resemble a narrow cartilaginous band, the intestinal canal discoloured, distended, and vascular; the ileum contained abundance of fluid, resembling that obtained from the stomach; slight inflammation of the mucous membrane.

Liver—Ash-coloured outwardly, and congested within; the gall bladder full.

The Bladder was empty, and firmly contracted.

The Kidneys, on being cut, gave at a few points a small quantity of whitish pus-like fluid, which was subsequently found to resemble the particular gruel-like fluid found in the other patient.

The second patient was likewise addicted to the habit of dram-drinking, which, however, for the fortnight previous to her death she had been unable to indulge in, from her excessive destitution, her husband having had no employment. She was rendering such assistance as was required by the family of her sister, the first-named patient, when, twenty-six hours after her death, she was seized with vomiting, of greenish biliary matter, and purging resembling treacle, both which symptoms continued one hour, and were followed by cramp of one leg and foot, extending to the hip, and ultimately settling in the loins. In seven hours coldness of the extremities came on, with blueness and corrugation of the hands, fingers and nails, and general collapse; the tongue was cold and death-like—respiration impeded—the action of the heart imperceptible, even by the stethoscope—most distressing thirst—the sensorial functions morbidly acute—a leaden hue at length overspread the countenance, and she died twelve hours after the commencement of the attack.

Post-mortem Examination.

Head—general vascularity; effusion in the ventricles, and between the mem-

branes; opacity of the arachnoid, and paleness of the plexus choroides.

Heart—healthy in structure; the left auricle and ventricle filled with dark black-looking blood.

Lungs—engorged with blood of a dark colour, particularly the anterior portion of the left lobe.

Liver—ash-coloured externally, and highly congested; the gall-bladder full.

Stomach contained a large quantity of fluid, like thin gruel, having flocculi floating in it; and its lining membrane was marked by deep lines, as though corrosive poison had been taken into it.

The Intestines were distended with the peculiar gruel-like fluid, which was in large quantities; the colon was constricted, and the mucous membrane vascular.

The Bladder was firmly contracted and empty.

The Kidneys, on being cut, afforded the same fluid as was found in the first case, and which was observed to correspond with the fluid found in the stomach and intestines.

The impression on my mind, from a hasty consideration of the first case, was certainly not favourable to the supposition that it was one of cholera, and indeed there are grounds for some doubt, supposing the case to be insulated; but taking into consideration the predisposition induced by her intemperate habits, the rapidity with which life became extinct; the peculiarities discovered after death, and the attack of the second individual; I think few will feel disposed to doubt the identity of this case, especially as it is well known to every experienced practitioner, that modifications of the diseases with which we are most familiar, are daily throwing a mysterious character about them of a most perplexing nature.

The peculiarities of the second case were too strongly marked not to be instantly recognized as distinctly unconnected with any disease of this country: the collapse of typhus perhaps more nearly resembles the finale of cholera than any other disease; but instead of the high excitement, the muttering delirium, the subsultus tendinum, and the brown leather-like tongue of the former disease, you perceive the energies of the mind reigning triumphant amid the wreck of the whole system, and it is not till the patient ceases to exist that consciousness is lost, although the

power of utterance may long have ceased—this was particularly the case with the patient I am now referring to, whose collected manner two hours before death was remarkable, if considered in connexion with the state in which the brain was found; but from this fact I am induced to surmise that the effusion was the result rather of that peculiar action which takes place in the bowels and stomach giving rise to the fluid therein contained, than of inflammatory action of a dangerous nature, more particularly as, I believe, in the act of death extensive effusion is often observed to have taken place.

Another fact, worthy of observation, as arising out of the examination of this body, presents itself to my recollection, and is curious, at least as affording a variety:—it has been found, I believe, in experiments on the blood, that a deficiency of the alkaline salts exists, and further that the effused fluid in the alimentary canal abounds with alkali; now the fluid of the stomach and bowels, on being tested with litmus paper, *was powerfully acid*, as was likewise the small quantity found on making a section of the kidneys.

As I have, in this communication, desired to confine myself to facts, I have avoided any indulgence in the theory of contagion or non-contagion; but it is curious that the sister should become the subject of a more perfect form of the disease after the death of the first patient; that another female, who also waited upon her, should be ill with complaint of the bowels, spasm, &c. for two days; and, lastly, that a poor man who held a candle over the body of the second patient which was examined in the night, should die of the same disease after seventy-two hours illness.

I am, Sir,

Your obedient servant,

JOHN GEORGE MICHELE.

31, Upper Charlotte-street,
March 7, 1832.

EXTREME DESTITUTION AMONG THE POOR.

To the Editor of the London Medical Gazette.

SIR,

I AM anxious to direct the attention of the rich and benevolent, through the medium of your widely-circulated Jour-

nal, to the scenes of wretchedness, poverty, and disease, now existing among the poor in the Borough. In company with my friend, Dr. Gilkrest, I paid, last week, a visit to this part of our great metropolis, and never shall I forget the heart-rending scenes of misery which I there witnessed. In one house in Christ Church parish, we were requested to visit a child, represented to have been just then attacked with the cholera. Dr. Gilkrest and myself visited this child. We ascended a dark pair of stairs, and were ushered into a room more resembling the den of a wild beast, than the habitation of a human being. On entering this human den, we saw a tall woman, having the appearance of a living skeleton, seated on a box in the middle of the room. In this poor woman's countenance was depicted a sensation of acute mental suffering: her half-clothed body, her haggard and ghastly features, her palsied limbs, were sufficient to excite in the breast, even of the most callous of human beings, feelings which no tongue, however eloquent, which no pen, however powerful, could with justice describe. In this woman's lap was her child half naked, who had just been attacked with symptoms of cholera. By the mother's side sat another daughter, apparently half starved, partially covered with a dirty blanket. In the room in which this wretched family had vegetated for twelve years, there was no fire or fire-place, neither the slightest vestige of a bed or bedding. The air of this place was so highly contaminated, in consequence of an accumulation of filth in one corner of the room, that it alone, independent of the influence of contagion, was sufficient to give origin to the most virulent and pestilential disease. This family, we were told, subsisted on *two shillings a week*, which small sum they got from the parish in which they resided. I could not have thought for a moment, that in a city like this, renowned for its wealthy and benevolent inhabitants, a scene so revolting to human nature could have existed. In my opinion, the only effectual way of putting a stop to the progress of this pestilence, is to remove the *predisposition of the poor*. Why is this disease, it is frequently asked, confined almost exclusively to the wretched hovels of the destitute Irish? Because they are suffering from the most acute mental and physical depression. Give food to

the hungry, clothe the naked, remove the filth from the habitations of the poor, and the cholera will quickly disappear. We heard, some months ago, of sermons being preached, and collections being made, in every part of the United Kingdom, for the starving poor in Ireland. Have our clergymen no sympathy for the starving poor in this great city? Is there not *one* benevolent minister who will set the example, and devote one half hour in pleading in behalf of the miserable and starving beings in London? Eloquence might here find a theme on which to exhaust itself. Would that the departed spirit of the benevolent Howard could visit this city—what would be *his* sensations? I sincerely hope, Sir, that the daily press, instead of directing their time to the discussion of the question of the existence or non-existence of the Asiatic cholera in London, will endeavour to rouse the dormant sympathies of the benevolent, by pointing out to them the condition of the starving poor in this metropolis. The scene of wretchedness which I have just described, was not the only one which we had the pain of witnessing: hundreds of human beings are perishing for want of the common necessities of life. "Blessed is he," says the Psalmist, "that considereth the *poor*: the Lord will deliver him in time of trouble." Should you think this relation will have the effect of directing the attention of the rich to the destitute poor in the Borough, you will oblige me by inserting it in your Journal.

I am, Sir,

Your obedient servant,

FORBES WINSLOW.

London, March 5, 1832.

[An account of the disease which appeared in the Marylebone Infirmary will be found in the present No.]

PATHOLOGY OF CHOLERA.

Dissections conducted in Scotland, and shewing the State of the Ganglionic Nerves in Cholera,

By PROFESSOR DELPECH.

THE idea, already suggested by the celebrated Loder of Moscow, that the central parts of the ganglionic nerve might be the seat of the essential morbid affection of cholera, had often excited my attention before I knew the true character of its author. The nature and progress of the symptoms, moreover, led

me irresistibly to the same conclusion. Nevertheless, in leaving the continent to study the malady in Great Britain, I determined to give up this and every other preconceived idea; while, in the examinations of the bodies of persons deceased of the malady, I should not omit to pay especial attention to the organic apparatus so clearly pointed out by the symptoms during life.

Having attended several patients in Musselburgh, we availed ourselves of the permission given us, to leave Dr. Coste on the spot, to prosecute the anatomical examination of the deceased persons.

The first occasion presented itself on the 12th of February. Dr. Coste, not having attended the case during life, is unable to prefix its history to that of the dissection, which was performed by him in the presence of Drs. Dunbar, Maccabe, Ronaye, and Moir.

Dissection the First.

Thorax.—The pleuræ without effusion; lungs natural; pericardium distended by a gas, but free from serous effusion. The right cavities of the heart distended with black semi-coagulated blood; the left ventricle half-filled by a single clot; the left auricle empty. The venæ cavæ and their principal branches contained but a little black grumous blood. No blood, liquid or coagulated, existed in the pulmonary vein.

The arteries were generally empty, with the exception of the aorta, which contained a solitary clot.

Abdomen.—The liver, spleen, and kidneys, natural; their veins merely being distended with black blood; the gall bladder containing a small quantity of bile; the *urinary bladder* contracted and empty.

The stomach distended by a large quantity of a serous liquid, with white tenacious flakes, resembling that vomited during life. The same liquid was found in great abundance in the large intestines. In the small intestine it was almost deficient, and the intestine was in a state of collapse.

The peritoneum did not shew its usual serous exhalation, but was pale, and uninjected. The mucous membrane of the alimentary canal was free from any remarkable alteration.

In the solar plexus of the sympathetic nerve, the neurilema was infiltrated. The matter of infiltration was of a rosy tint and shining, hard, and solid when cut into, more abundant in the centre than towards the circumference of the plexus*.

Second Dissection.

The second case was that of a female, aged 70, named Janet M'Adam, a person in

* This examination having been the first performed, and having been carried on in an inconvenient apartment, the observations were not pushed to the extent fully required.

by no means miserable circumstances, but inhabiting an unhealthy part of Glasgow, named Goose Dubs. She was brought to the Cholera Hospital on the 13th February, at noon, in a deplorable condition. The disease had gone too far; the evacuations and cramps, previously intense, had now ceased. The body was cold, and despite of the assiduous use of a steam apparatus, the breast alone retained heat. The pulse was almost lost, and perceptible, with extreme difficulty, in the right arm; and she complained incessantly of epigastric pain. The collapse was, in short, complete. No medical treatment was of avail, and she died at two p.m. The body was examined at nine p.m.

Pericardium, pleura, and peritoneum, free from exudation, and in a natural state. The heart perfectly void of blood, and collapsed; it contained no gas; its right and left auricles half-full of black coagulated blood; the lungs congested with blood of the same character at their posterior border solely: elsewhere they crepitated distinctly.

The liver pale, attenuated, as if folded on itself, and free from injection; the gall bladder half-full of green bile, no trace of which occurred in the dejections. The intestines were white, transparent, and containing but little gas. Transparent and adherent mucosities lined the stomach, the mucous membrane of which was slightly injected. The same substance was found in the intestines.

The mesenteric vessels, the *venæ cavæ*, porta, and mesenteric, the arteries too, even the aorta, contained but little blood.

Both semilunar ganglia were voluminous, injected, red, infiltrated, their section shining and moist, though affording no discharge of fluid whatever.

The nerves of the solar plexus were swollen—their neurilemma red, and slightly infiltrated.

The pulmonary and cardiac plexuses were swollen and injected.

Third Dissection.

The third case was that of a man named James Phillips, labourer, aged 46, admitted into the Cholera Hospital at Glasgow on the 14th of February, at eleven a.m. The disease was already intense, though a full and firm pulse were remarked, and his tongue was red and dry at its edges; circumstances both unusual in this disease. At 7 p.m. complete collapse, and he died at 5 a.m., on the 15th of February. His body was examined eleven hours after death.

The surface of the body cold.

Old adhesions between the pleuræ; suppuration at the posterior part of both lungs; elsewhere these organs white, sound, and crepitant. Pericardium natural and without effusion. Right auricle of the heart full of black imperfectly coagulated blood. A mass of fibrine of dense structure lay loose beneath

the auriculo-ventricular valves; the right ventricle half empty, and collapsed, containing only black and fluid blood. A dense white mass of fibrine extended itself from the ventricle to the pulmonary artery. The left auricle and ventricle were almost empty, and contained no clot.

The liver adhering to the diaphragm and intestines; the gall bladder containing healthy bile in great quantity; the mucous membrane of the stomach injected, slightly softened, and ecchymosed, covered with mucus, and mixed in some points with bile. The intestines free from gaseous accumulation, and slightly injected on their external surfaces.

In the superior part of the small intestine there was a great quantity of white creamy matter, in places of a greenish tinge, and rather more consistent than the usual cholera secretion; near the cæcum this substance was more fluid, and more of the usual character.

The entire mucous membrane of the small intestine shewed decided injection and extensive ecchymoses; and near the cæcum broad extensive patches, produced as well by vascular injection as by deep-seated extravasation.

The great intestine normal, and containing a great quantity of the cholera fluid. The mesenteric glands white and natural.

The pneumo-gastric nerves increased in size, but of natural colour.

The solar plexus composed of nervous bands, larger and redder than natural; their neurilemma injected. Sections made into them shewed the matter of the injection to be of hard consistence.

Both semilunar ganglia, especially the left, voluminous, injected, and softened. The cervical portion of the ganglionic nerve flattened into the shape of a broad band.

The urinary bladder empty, contracted, and hard; the vessels of the brain gorged with black blood; sections into the substance of the brain produced abundant oozings of bloody drops; the superior ventricles contained an extravasation of serous fluid; and four or five ounces of serous extravasation tinged with blood, were found in the vertebral canal. The cortical substance of the brain was thicker than usual.

CASE IV.—Mary Hardie, a child aged seven, was brought to the Cholera Hospital, Glasgow, on the 15th of February, at eight p.m., her disease being already of fifteen hours duration; she was accordingly in a desperate state, and she died at two a.m. next day. The autopsy was performed at noon on the 16th. There was extreme emaciation of the body; injection of the vessels of the dura mater; section of the brain afforded oozings of blood; serous extravasations in the ventricles. The lungs charged with red blood, except at their anterior edge, which was crepitant. The right auricle and

ventricle of the heart full of liquid black blood, and in the same ventricle a concretion of soft, vesicular fibrine; the left auricle and ventricle almost void of blood. A similar concretion to the former extended from the ventricle to the aorta.

The stomach and small intestines charged with a creamy, greyish-white substance, tinted green at intervals. The liquid towards the ileum was tinged, more pultaceous, and not adherent. In the great intestines the fluid was more liquid.

The membranes of the intestines were healthy. The mucous membrane free from injection or softening, except in a single point near the ileum, which was thickened and red, but not ulcerated. In the ileum there was a loose invagination, of about an inch in length.

The liver was sound, and the gall bladder half-full of green bile.

The bladder empty and contracted.

The *semilunar ganglia larger than natural*, but white, and free from injection. The solar plexus and venal plexuses slightly vascular. The mesenteric glands large, but healthy.

CASE V.—Macdermot, aged 62, residing in the district of Goosedubs, Glasgow. His son died of cholera the preceding night. He was admitted himself into the Cholera Hospital at half-past ten A.M., on the 15th. His condition was very serious, though the face and eyes were unusually injected, causing a complexion very different from the blue tint of the ordinary cases. The pulse also was full; the body warm. The preceding symptoms, however, left no doubt of the illness being cholera, and he died at midnight. The autopsy was performed next day, at noon.

There was great emaciation of the body, although the man had been of very muscular form. The veins, especially of the surface, were swollen. General adhesions of the pleuræ at both sides, principally old, but some of more recent date.

Cadaveric soughillation at the posterior edge of the lungs; their anterior edge blue and crepitant. *The pneumo-gastric nerves perceptibly injected and large.*

The left ventricle full of black fluid blood. Both auricles and the right ventricle containing but little blood, and that of the same quality. Very firm fibrinous concretions in the same cavities.

The right jugular vein of uncommon size; the proper vessels of the aorta injected.

The peritoneum covering the stomach and intestines, strongly injected. The mucous membrane injected without ecchymosis; internally a large quantity of greenish, serous fluid. In some places a green matter precipitated on, and attached to, the mucous membrane. Near the cæcum, and in the colon, numerous and deep ecchymoses. Internally a still greater quantity of matter

like that in the stomach. Considerable general accumulation of gases in the intestines.

The solar plexus, as well as the semilunar ganglia, voluminous and red; the renal plexus natural.

The gall bladder full of natural bile; the liver healthy. The bladder contracted and hard, containing a few drops of urine.

The dura mater gorged with blood; serous extravasation in the arachnoid membrane. Sections of the brain demonstrated its increased vascularity, and an œdematous and softened state of its substance. There was also a very considerable extravasation of serous fluid in both the superior ventricles.

I shall not communicate here the details of any other anatomical investigation, since their results relate to other points of the question, which, at least comparatively speaking, can only be considered as accessory in their kind. I have only wished to fix your attention to a circumstance which appeared to me of capital interest, and to which we already had adverted, namely, the morbid state of the semilunar ganglia, the solar plexus, the renal plexuses, the inferior portion of the pneumo-gastric nerves, and sometimes even of the pneumo-cardiac plexus. These alterations, which had been predicted to exist by M. Loder, and which my coadjutors and myself have now demonstrated by dissection, receive additional force from their having been verified by yourself since our conversation on the subject. Such researches are difficult in their nature. It was to be feared lest, in the confusion ever excited by a wide-spreading and dangerous epidemic, they should be altogether neglected, and the progress of science on this point altogether retarded. But on the contrary, I feel convinced, from your ardent zeal and great information, that if there be any important truth in the facts now stated, it will be deemed efficaciously to the interests of science by the physicians of a country so enlightened as yours, and where the opportunities of such investigation are unhappily now so abundant.

The present nature of the malady may be understood by the relation between the morbid alterations now pointed out, and the symptoms of the disease.

A fixed, intense pain, confined to a very narrow space, corresponding exactly to the point of the linea alba, opposite to the extremity of the ninth or tenth rib, always precedes the other symptoms. The exact coincidence between the seat of this pain, and that of the central point of the ganglionic nerve, will be immediately perceived.

The diminution of the circulation is the first phenomenon supervening on that now described. No evacuations have yet taken place, and the blood retains its normal con-

stitution; nevertheless, the arteries beat feebly, their diameter diminishes, and this decrease in the circulatory actions at first occurs in alternate periods, corresponding to the transitory degree of intenseness of the epigastric pain. Again, in the interval the pulse regains a degree of force and fulness which too closely resembles that accompanying hernia, and every kind of peronitis, not to communicate the idea of an identity of origin in both cases. In both, indeed, it is the ganglionic nerve which suffers, and the same effects may result in all.

An unusual and most copious secretion is thrown out in the alimentary canal. To furnish it, the organs must necessarily receive a great impulse from the nerves on which they depend. Here again we are led to the ganglionic nerve as the certain source of this agency. If, indeed, this nerve be in a morbid state, can the derangement of its functions be a matter of surprise?

A convulsive state shews itself in the stomach and intestines. But we have just seen, and the fact may be easily verified, that the inferior part of the pneumogastric nerve is interested in the affection, at first peculiar to the semilunar ganglia and their plexuses.

The blood ceases to be arterialized, and the retention of its blackness of colour precedes the cooling of the column of exposed air; a symptom of a more advanced stage of the disease. But physiologists have long since classed the liver at the head of the apparatus of sanguification, and since the solar plexus, common to both ganglia, and which participates in their morbid alteration, furnishes to the liver all the nerves which excite it to action, it cannot appear strange that the functions of this organ should languish or cease. Whatever be the precise action of the liver on sanguification, whether or not it be the mere abstraction of the principles of bile from the blood, it, at all events, is worthy of attention, that the venous blood here traverses the liver, without experiencing any alteration therein; that the secretion of bile ceases, and that frequently the liver is found destitute of the beautiful injection which constitutes its natural condition.

The secretion of urine ceases. Accordingly we have seen that the morbid state of the ganglions extends perceptibly, under some circumstances, to the renal plexuses. In the cases even in which the morbid appearance is least marked, there is nothing contrary to logic in supposing that a morbid irradiation takes place from the other ganglia, capable of producing the same effects.

The general temperature of the body declines, at first in the limbs and all the appendages of the body, but it is retained in the trunk and the viscera, as may be presumed by the evidence of the patients them-

selves, their eager thirst for cold drinks, and the relief afforded them by the internal use of ice, as I have myself witnessed. Now, it is well known that the nerves of the ganglionic system accompany throughout the arteries of the limbs; that the temperature of the extremities is in proportion to the force of the circulation; that in the Cholera the circulation declines and ceases at first in the limbs, especially at their extremities, a phenomenon which naturally leads us to suppose the diminution of the influence of the ganglionic net-work which accompanies the arteries, and is readily explained by the morbid state of the central point of the sympathetic system.

Perhaps there is even no exaggeration in seeking in the same conditions the immediate cause of the cramps. They are, indeed, in direct proportion to the progress of the malady, and of its abdominal symptoms.

One of the latest effects of the disease is the cooling of the column of expired air, and the absence of carbonic acid therein. But did we deny the occurrence of the pneumocardiac plexus, in the effecting of the combinations of which respiration is the means, we could not conceive why a bellows should not produce respiration in a dead body. Nervous influence has, therefore, ceased in the bronchia, and hence, also, all chemical actions of respiration are prevented, as well the maintenance of the external temperature of the air expired, as its peculiar constitution. We may, therefore, rationally conclude that, since the communications of the pneumogastric nerve with the ganglionic apparatus, are the probable causes of the extension of the disease of the latter to the inferior portion of the former, that a like extension takes place to the pneumocardiac plexus. Thus it is that we find the beating of the heart to decline at the same period that the cooling of the expired air is first noticed.

This chain of physiological inductions would be but of trivial interest, did they not depend on a material and certain morbid condition. It is even of no ordinary value as regards the utility of our observations, that the *first* affection is thus positively defined, as well in its nature as in the seat in which it exists, to be an inflammation. And this kind of alteration, serious in its nature, tending to a rapid increase, affecting one of the principal systems of organic life, is more dangerous here, as well on account of the delicate and readily destroyed texture of the internal organs, as by the necessary functional lesions produced on the formation of blood, on nutrition, respiration, and circulation.

These considerations will perhaps turn the attention of practitioners to the effect which, in several visitations of Cholera, have followed the detraction of blood actually performed in the commencement of the malady,

or later in the disease, along with various stimulating methods. They may, perhaps, explain, why one or other of these proceedings, taken singly, and in an absolute sense, has had little or no success. They will also, probably, render it intelligible, why in different places, and sometimes even closely adjoining each other, in the course too of the same epidemic, equally respectable practitioners have been compelled, some to bleed from the commencement, others to abstain from this operation until reaction supervenes.

I might even conclude from these anatomical facts, and from some other unusual circumstances which will not escape your attention, the necessity of varying the treatment according to certain complications which these circumstances denote; but addressing *you*, it is unnecessary to enter into such details.

I shall confine myself, in conclusion, to alluding to the importance which such observations henceforth attach to the *anatomical* investigation of this disease. Such researches cannot be limited to the state of the membranes of the digestive organs, or the nature of the fluids therein contained. Already the investigating spirit of your estimable fellow countrymen has been directed towards the alteration of the blood, and the chemical nature of the dejections; and they have established the fact, that all that is deficient in the former, is found to exist in the latter. It is now for the *anatomists* of the medical profession to second the researches of the chemist, and to seek for the organic principle which effects this chemical derangement. In such a position of the question, *superficial* dissections can be of no value whatever.—*Cholera Gazette*.

Dupuytren's Opinion of Cholera,

M. DUPUYTREN has published a letter on the present epidemic, containing some rather curious remarks. He holds the chief seat of the malady to be the glands of Peyer and Brunnen. "Van Derundel says, that an attentive examination of these follicles with the naked eye, or, still better, with a glass, will discover in their cavity their structure, or their vicinity, or the matter secreted by them—the seat, and perhaps, also, the nature of cholera." What I say is not merely speculative—for I have many times opened the bodies of persons who had sunk under sporadic cholera, and I have constantly found the glands of Brunner and Peyer excessively developed, and that without any marked appearance of inflammation. I do not believe that there exists such a difference between the epidemic and the sporadic cholera as to occasion a decided difference in the seat and nature of the malady. In a word, the disease consists in an "irritation secretoire" of the glands above-named, accompanied by a particular set of symptoms sympathetic of the state of the alimentary

canal. M. Dupuytren proceeds to recommend leeches to the belly, and a strong decoction of poppy heads, sweetened with syrup of gum, and administered in repeated doses; and by sugar of lead, either in pills, or dissolved in the decoction. I ought to add, says M. Dupuytren, that preparations of opium are not so efficient as of poppies' head. I have succeeded with the latter when the former had completely failed.—*Gazette Medicale*.

(From Mr. Moir's *Observations on Malignant Cholera in Scotland*—a pamphlet which we can strongly recommend to the perusal of our readers. It will be recollected that Mr. M. is the author of the "Outlines of the History of Medicine," and one of the most distinguished contributors to Blackwood, &c. under the signature of Δ and the Modern Pythagorean.)

On the Contagion of Cholera.—Whatever cholera may have been in India, surely the mind must be strangely constituted which can shut itself against the fact of its being virulently contagious, at least in this climate.

No stronger corroboration of this position could be any where looked for, than the circumstances connected with the third case which appeared at Musselburgh, and which happened to fall under my charge. This was a girl whose mother kept a lodging-house, and whom I found in a state of complete collapse, on the morning of Thursday, the 19th of January—the day after the first appearance of the pestilence. She died on that afternoon, between five and six, and was buried by moonlight the same evening, in order that the fumes of the contagion might be more effectually counteracted, by having the body removed, and the house properly washed and fumigated. Notwithstanding every precaution, however, the mother, during the night of Saturday, was also similarly seized, and fell a victim on the following noon. Her sister, who had walked from Leith on the same morning, to condole with her in her family distress, was immediately affected on entering the house; but her symptoms being overlooked in the misery around her, medical assistance was not called in, until, on the return of the nieces from the interment, their aunt was discovered dead on the floor of the dwelling. Her husband, Baxter, a man of intemperate habits, came out to inquire into her fate; and, immediately on his return home to Leith, was seized with the distemper, and died. By the vigilance of the Board of Health there, the contagion, so far as this individual was concerned, was, by active measures, successfully checked, and cholera thus for a while held in abeyance.

Since the publication of the first edition of this pamphlet, however, a sequel to this family category has shown itself in Edin-

burgh, in a case which seemed at first to delight the hearts of the anti-contagionists.

Of the previous three well marked and violent cases of cholera asphyxia, which had occurred in Edinburgh, all had been persons recently returned from Musselburgh, while the disease was raging. This fourth one was a widow, M'Millan, who had not been out of town for months before her death. She was seized on the night of January 27th.

In a precognition before the Sheriff, the following overwhelming facts have, however, been since elicited. It turns out that the son of this woman slept, on the nights of the 18th and 19th of that month, at Musselburgh, in the house of Mackay; and it will be seen by reference to the Appendix, that B. M. (see case first) died on the afternoon of the latter day. This circumstance, however, was mendaciously concealed.

On the 22d he was, after his return to Edinburgh, seized with diarrhoea and vomiting; and was seen by Professor Alison and other medical gentlemen, who, from the nature of his evacuations, immediately set down his case as one of cholera. It was, however, slight; and in a few days he recovered;—but not ere a woman residing under the same roof was similarly affected.

Lastly, on the 27th, his mother, Widow M'Millan, who had nursed her son during his attack, and occasionally slept in the same bed with him, was seized with the malady in a more malignant form, and fell a victim to it. Facts like these require no commentary.

Taking another illustration from the first six in the schedule of District First, I find a boy, W. B. aged seven, to whom I was called at six on the morning of the following day, Friday, the 20th. At eleven at night, a man lodging in the same house was also attacked; and on the following morning the father of the boy. Within a week after, his youngest child, aged only three years, became also violently affected. Eight or ten other cases almost immediately showed themselves, within a compass of thirty yards from this house; and this at a time when whole districts of the parish remained totally uninfected. Indeed, the whole history of the disease among us, from first to last, is nothing more than a consecutive series of examples, strikingly illustrative of the personal communicability of the contagion. That it can, besides, be conveyed by inanimate substances, is also very probable; but the testimony on this subject is more difficult of attainment, while it is less capable of proof*.

It is quite a mistake to suppose that the disease is either so erratic or whimsical, in its wanderings from district to district, as some would lead us to suppose; or that it will suddenly disappear, after having wreaked a desultory violence. This idea is founded on the erroneous supposition of its non-contagious nature, and a number of apparent illustrations have been brought forward in its support. The fact is, that cholera will nowhere abate, or pass away, while a victim susceptible of its influence remains, provided such be exposed to its contagion; and no doctrine more dangerous or destructive to the safety of society can be promulgated.

Cholera, a Disease of Debility.—The malignant cholera, as it has appeared in this climate, if not a disease of direct debility, is one that is dangerous only to debilitated constitutions; and, in not a single instance which has come under my personal observation or scrutiny, has it selected a victim from among the robust and vigorous. This is one of the chief reasons why its contagious nature has been so much a matter of dispute; for, that it is communicable in a most alarming degree, is, as has been shown, matter of hourly observation. That there are any constitutions unsusceptible of its influence, it would be hardy to maintain; but those more immediately exposed are unquestionably the weak and debilitated. Indeed, it strikes me, that there is almost as little chance of a man of sound health and temperate habits being subjected to this pestilence, as of his having the small-pox for the second time.

ANALYSES & NOTICES OF BOOKS.

“L'Auteur se tue à allonger ce que le lecteur se tue à abrégé.”—D'ALEMBERT.

Elements of Surgery. By ROBERT LISTON, F.R.C.S. Edinburgh and London; Surgeon to the Royal Infirmary, Senior Surgeon to the Royal Dispensary of Edinburgh, Lecturer on Surgery, &c. &c. Two Parts. 1831.

The Principles of Surgery. By JAMES SYME, F.R.S.E. F.R.C.S. Edinburgh and London; Surgeon to the Edinburgh Surgical Hospital, and Lecturer on Surgery. Edinburgh, 1831.

It is difficult to avoid the inference that to a generous spirit of rivalry or emulation we are indebted for the almost simultaneous production of these works. The authors are both eminent in their

* In the cases of Stewart and Boatwood, which fell under my observation, the disease was distinctly traceable to their having carried articles of furniture and apparel from the houses of the deceased relatives to their own.

profession—both distinguished teachers—and, we trust, will both be appreciated as contributors, by the books before us, to the advancement of surgical science. We notice their works together, simply as they treat of one subject, and not with any pretension to settle their rival claims to preference—*non nostrum tantas*, &c. Neither treatise, however, is complete: Mr. Liston promises a third and concluding part, and Mr. Syme a continuation of his labours.

The want of suitable text-books in the present advanced state of surgery, will not be much longer, we should imagine, matter of complaint. The systems of Benjamin Bell, Allan, and even Latta, have been hitherto almost the sole occupants of the field; and while British surgery has been diffusing itself through the press, in the form of monographs and able contributions to the periodical journals, nothing that gave any adequate notion of these extensions of science in a condensed shape made its appearance in this country, until the late Mr. Abernethy was in a manner compelled to publish a résumé of his Lectures; and what *he* has left incomplete, we may now, we presume, entertain a rational expectation of seeing accomplished by the rival lecturers of the Scottish metropolis. When the systems of Principles before us shall have been finished, we shall be able to speak more decidedly as to their merits; but, until then, it would be premature to give any thing more than a mere outline of the plan pursued by each.

Mr. Liston, after treating of inflammation generally, and as it occurs in particular tissues,—the cellular substance; the membranes, mucous, serous, and synovial; the bones, the arteries, and the veins (with which about one-half the first part is occupied)—then proceeds to the consideration of tumors in all their variety. After which, he treats of wounds and ulcers, scalds and burns. The second part is arranged according to the several organs of the body which may become the seat of injury or disease: injuries of the head, including wounds of the scalp and temporal artery, wounds of the brain, compression, and the several surgical resources available for their relief or cure. Then come the diseases of the eye, the nose, the lips, fauces and tongue, the teeth and gums, the face

and neck, the ear; after which, this part concludes with a quantity of miscellaneous particulars relative to the surgery of the upper extremities; leaving, of course, the entire of the surgery of the chest, belly, and lower extremities, for the portion of the work which is yet to come.

It strikes us, that, of the two works before us, Mr. Liston's is that which is a more immediate transcript from a note-book of lectures; there is a colloquial interest about it, which sometimes verges into eccentricity of phrase. For example: "*Venesection* at the bend of the arm, is too often resorted to by thoughtless or ill-educated practitioners, to the detriment of the patient; as after accidents, before reaction has occurred, in local pains not inflammatory, &c. It is had recourse to by those who have no correct ideas of the actions of the animal economy—who have not within their heads a peg to hang an idea upon; or if they have, they are too lazy to think and to combine their ideas, so as to come to a proper conclusion regarding what is the most proper and judicious course to be pursued in any one case. They follow a routine, and bleeding is too generally the commencement of it." But, on the whole, we like Mr. Liston's bold and vigorous method of expressing himself.

Mr. Syme is not so full as Mr. Liston, on the subject of inflammation and its consequences; but from what we can judge of the mode in which he manages the other topics which occupy the present portion of his work, he promises to be abundantly plain and practical. In his successive chapters, he treats of the blood-vessels, bones, joints, muscles, and tendons, with the treatment of external injuries; amputation is also included in the volume. From the syllabus which is prefixed, and which is intended to give us some idea of what remains to be completed, we find that the author has still to give his principles relative to the surgery of the thorax, abdomen, pelvis, nervous system, skin, eye, mouth, nose, and ear.

There is one characteristic of Mr. Syme's authorship which we cannot leave unnoticed, and for which we are inclined to give him much credit: he is extremely liberal in quoting authorities, and expends a fair proportion of criticism in noticing the inventions and improvements of his contemporaries. We

MEDICAL GAZETTE.

Saturday, March 10, 1832.

“Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso.”—CICERO.

CALUMNIES AGAINST THE MEDICAL PROFESSION.

IT would seem to us, and we have spent some little time in coming to an opinion upon it, that small, small indeed, is the debt of gratitude due by our profession to the public. We have but a very short and plain statement to make. When, in the course of last month, the cholera was officially announced as being in London, great was the outcry raised of a sudden against the supposed contrivances of medical men. We could not but mark with pain (though we disdained to notice them at the moment) the unworthy and disgraceful charges brought against the profession, by certain well-known public instructors and advisers: the disease—which was really “a nonentity,”—existed only in the “delusion” practised upon the public; there was “no cholera” in London; the city never was so healthy; it was a thing “got up” by the profession;—in short, it was “a professional humbug,” hit upon for the sake of “raising the wind.” Another step would have probably led to the denouncement of the supposed alarmists, and, as has been done in some parts of Europe, to the delivering them over to the popular fury. The cry, however, is now abated somewhat; the real existence of “something” in London is forced upon the credence of the public; but the notion that the report of it is much exaggerated, for interested motives, is, we believe, not yet totally abandoned. The profession, meantime, wherever the disease presented itself to them, performed their duty cheerfully, and are perform-

could produce numerous instances of this, but one must suffice. Speaking of the treatment of aneurism, he says, “The object of tying the artery being, not to prevent the blood from entering the aneurism, but merely to cause such stagnation of its current as may induce coagulation, it was proposed by M. Brasdor, of Paris, to obliterate the vessel beyond the tumor, when circumstances prevented the operation from being performed between it and the heart. Some unsuccessful attempts were made, on this principle, by Deschamps and Sir A. Cooper; but Mr. Wardrop has lately recorded several instances of its more fortunate application. It is evident that the operation cannot be performed with advantage, if a branch of any considerable size comes off between the aneurism and ligature, as this would allow the current to continue; it could not be of any use in cases where, the sac being small and regularly dilated, the contents remained fluid; and the only occasions where it promises any benefit, are those in which coagulation is already far advanced. But here the passage through the vessel beyond the tumor must be obstructed nearly, if not altogether, as much as it can be by the ligature; so that there consequently does not seem to be much probability of this operation being ever extensively introduced into practice.”

Atlas of Surgical Apparatus; being a Series of Delineations of the most important Auxiliaries of Surgery; with Descriptive Letterpress, explaining their several modes of application.
By HENRY T. CHAPMAN, M.R.C.S., &c. Highley.

PRESSED as we are for space, we cannot let another week pass without recommending this work very strongly to young surgeons and pupils. It gives an excellent account of the various kinds of bandages, apparatus for fractures and dislocations, as well as of all the important instruments employed in modern surgery; with illustrative drawings. The author, we perceive, gives practical demonstrations on the above subjects: if these be conducted in a manner corresponding to the lessons afforded by the “Atlas,” they must be a valuable addition to the practical education of our younger brethren.

ing it, without a murmur : to this hour, we believe, there has not been a single remonstrance against the unworthy treatment they have received.

Now it strikes us that it may serve some useful purpose to take a closer view of the parties who are immediately concerned in these calumnies. First, who are those who give vent to them?—and, secondly, are there any circumstances which may seem to have given room for even the shadow of suspicion touching the honesty and good faith of the medical profession.

The “no cholera” cry is easily traced to its source. It was raised in London by the commercial interests, and by that portion of the public press—indeed, including almost the entire of the non-medical portion of it—which has its great support in the former influential class of the community. And what was very remarkable in the conduct of these public organs, was, that they seemed unconscious of, or cared not how they appeared, pursuing the very same course for which they censured the good people of Sunderland. They not only maintained that there was no cholera, or any thing like it, in the metropolis, but directly charged all the medical men employed by government with making “a job” for themselves;—nor did they stop at any assertion, however glaring, relative to the exorbitant salaries of which those practitioners were supposed to be in the enjoyment. They threw open their columns to the pitiful jargon of a parcel of trading correspondents—dignified them with the epithets of “eminent practitioners,” — “distinguished physicians,” and so forth; and passed them off on the public as first-rate authority for *their* side of the question. But all this does not “stay the plague.” The disease goes on : the parties, however, have no notion of abandoning their course—though they have made many a tack. They now, pretty ge-

nerally, allow that there is a fatal complaint of some kind prevailing in London; that it slays its victims with a tremendous rate of mortality; but, though the cholera of the Asiatic type is acknowledged to have visited Petersburg, Warsaw, Vienna, Riga, Dantzic, and Hamburgh—aye, and Sunderland, and the North of England and Scotland—yet this cholera, or whatever it be, in London, can *scarcely* be the same! To be brief, these notable advocates are in a fair way of ending in something like a right conclusion after all. To this we saw that eventually it would come; and for that reason were careless of doing the champion’s office for the profession. The calumnies have been gradually subsiding—as they will, ere long, wholly subside; but this does not preclude the propriety of a remonstrance on our part with these same self-constituted public instructors, for the injurious charges to which they have given most disgraceful circulation.

If medical men were the contrivers of any part of the deadly diseases which occasionally visit the species, then might the silly reproach—that they live by the bodily sufferings of their fellow-creatures—which is frequently cast upon them, have some foundation. But being no more than the humble instruments by which those calamities that afflict mankind are alleviated or removed; and for this purpose devoting themselves, by a long course of preparation in the schools and in the world, and by the expenditure of much time and money, to a precarious and oftentimes perilous profession; are they ever and anon to have it cast in their teeth that their sole object is an *interested* one—that their chief aim is *money*? Are they alone, then, to be flouted, as seeking for their professional hire? While ample, nay, princely provision is made for the

church, and the highest honours and emoluments in the state are open to the bar, is that profession which confers the greatest of all earthly blessings on mankind to be sent empty away, or to remain contented with the chance of a scanty maintenance and a but too often premature grave? Why is that disinterestedness expected from medical men, which we never see exercised by any other class in the community—and least of all by those who call for it the loudest? It should be recollected, that the members of our profession are not wholly the slaves of the public. Some of them, to be sure, have made themselves cheap, and are cheap enough; like the man in the old epigram, who wished to seem poor, and *was* poor. There are worthless persons and pretenders in and about every profession; who, having managed to get connected with it, use the privileges of their order for unworthy purposes. But the great cause of the anomalous state of medicine to which we are advertising, seems to us to be, that the members of it have ever been too conspicuously animated with a spirit of generous competition in offering gratuitous service to the public: and of this, the said public have manifestly taken an ungenerous advantage. Nor is this all: from the common tendency which pervades most people to undervalue that which is readily obtained, the services of the profession to the public have come at last to be most unfairly estimated.

To come, however, to particulars. There are many silly people, we are aware, who think that, in the event of a great pestilence, like the cholera in its most malignant form, the fortune of every physician would be made. We would entreat those hasty reasoners to pause ere they adopt this most groundless notion. Most pestilences, like the cholera, rage among the poor; and medical attendance on this class is productive

of no emolument: nay, in the prevalence of an epidemic of a contagious character, like the one in question, the practice of the medical man among his patients who are in better circumstances, becomes straitened, if not altogether suspended, upon its being known that he holds intercourse with the infected. We could adduce instances of this, which have already occurred within our own cognizance, if we were only at liberty to mention names: gentlemen, whom we personally know, have been peremptorily tendered the alternative, strictly to abstain from cholera practice, or give up their accustomed attendance in particular private families. Here is blessed encouragement for those who would reap a harvest from an epidemic!—as well reap it “from the whirlwind!”

It is plain, then, we conceive, that the *poor man's disease* is likely to prove but a sorry speculation for those who have “interested motives,” and who are supposed capable of making “a job” of it. With the habitual attendants of the poor has the cholera practice in the metropolis begun, and with them is it likely to remain: and if it found those gentlemen money-loving upon its arrival, little the richer, we imagine, will it leave them upon its departure.

It is true enough that the occurrence of cholera has procured employment for a certain number of practitioners under the immediate control of government: but their number is extremely limited; and it may be more than questioned if some of them have not positively rather “gained a loss” than otherwise, by their new occupation. They are all, with we believe a very few exceptions, of the class of retired naval or military practitioners, who, in lieu of their unwelcome newly-imposed duties, receive the difference between their half and full pay, amounting to a few shillings *per diem* each

man; and for this pittance must they perform assiduous duties, and probably relinquish a portion of whatever practice they were in possession of, or perhaps be excluded from it, under the *taboo* of the above-mentioned private quarantine.

Such, then, are the few simple facts upon which have been grounded those ungrateful and foolish calumnies, some of which we have thought it not improper to notice: others of them were so gross and so unnatural, as, before they were a day or two old, to destroy themselves; and in some instances, we were glad to see indignant and overpowering contradictions put forth in the public prints by parties of unquestionable integrity.

Nothing, however, has given us more satisfaction in this regard, than the cool and praiseworthy endurance manifested by the profession itself under the circumstances: the very individuals who were most pointedly singled out—even by name—as, for a matter of lucre, supporting a public delusion, have not once deigned to come forward and retort, as with perfect truth they might, those foul imputations upon their insidious adversaries. What *we* have said will, we trust, in no way derogate from that modest bearing, which it is always so highly desirable to observe, when the dignity of the profession is in question: it was our duty, we conceived, to have said thus much; and thus much have we said honestly, fearlessly, and truly.

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PROGRESS OF CHOLERA IN THE METROPOLIS.

THE cholera, whether it be contagious or non-contagious, and whether a new disease or an old one, continues its course in the metropolis; but certainly not as yet in a manner to warrant the apprehensions which its very name was wont

to excite, when the enemy was still at a distance. There can, however, be no reasonable doubt that a malignant disease, corresponding in all its essential characters to that which has recently prevailed in the north of England, and over a large portion of Eastern Europe, has shown itself in London—a disease proving rapidly fatal to about one-half of those attacked by it, in its severe form, and unknown not only to our fashionable physicians, but also to the practitioners most familiar with the maladies which affect the poor of this city. On this point we have the testimony of numerous intelligent and candid persons; and if we may venture to offer our own contribution to the general stock, we will add that fifteen years' observation in an extensive field, embracing one of the localities where cholera now prevails, never presented us with one instance which could be confounded with that disease. If it be argued that it is either our native fever, or our English cholera rendered more severe, we can only answer, that the degree of intensity is such as to neutralize the resemblance. In the fevers of this country, we have never seen the patients die, either within so short a period, or in nearly so large a proportion; and in the autumnal cholera, to which we have been accustomed, the depression and danger have been in direct proportion to the evacuations and cramps;—which, however, is very far from being the case in the present epidemic. Impressed with this conviction, we think it our duty to caution our readers against being led away by the idea, that there is nothing either new or formidable in the disease, merely because it has hitherto been limited in its ravages, and chiefly confined to the destitute. It is at best but an equivocal source of satisfaction, that another is added to the evils of those on whom the burdens of humanity already fall most heavily; and at all

events, the most effectual method of turning the danger from our own doors, is to take every means of mitigating the visitation in the dwellings of the poor.

Much of the uncertainty and distrust which prevails, is to be attributed to the entire want of confidence in our public functionaries; and we regret to have to add another to the list of indiscretions which have been practised. We allude to the manner in which certain cases have been reported. It has been generally understood that those only were included in the returns which presented features of a malignant description; and the very high rate of mortality at once justified and was explained by this supposition. If all the cases, whether slight or severe, had been included in the reports, the alarm would have been much less; because the proportion of recoveries would have shewn very differently on the list from what they now do;—but it is quite clear, that the plan of stating the malignant cases only having been adopted, nothing could be more calculated to excite unnecessary panic than suddenly swelling the numbers by the addition of a batch of mild cases. Knowing how natural it was that the Central Board should be goaded on to make the most of the epidemic, we warned them that their returns would be sifted with the keenest scrutiny. We confess, however, that we were not prepared for such an act of rashness as the announcement of sixteen cases in the Marylebone Infirmary in one day, without the most absolute certainty of their existence. Of this large number, only one case had proved fatal:—surely this alone ought to have been sufficient to inspire caution in reporting them as the same with the malignant disease: yet was this done, and suffered to produce its effect upon the public mind, till a flat and unequivocal contradiction of its truth was officially addressed, by the Secretary of the

Local Board of Health, to the Editors of the various newspapers. Upon this hint the Central Board, without any explanation of their previous conduct, very coolly informed the public, in a note, that the cases were mild, not malignant cholera. Indiscretion can scarcely go farther. If it had been the design of the Central Board to favour the views of their opponents, and to take from their friends the power of defending them, they could not have adopted a more skilful proceeding. It may be that they were misled, but if so, it was their duty alike to themselves and to the public to declare by whom this blunder was committed. The public have only the Central Board to look to, and if explanation be withheld they must be content to bear the blame. The imprudence, meantime, is really lamentable, and we see in it the germs of most fruitful mischief; for it tends to weaken still farther the confidence—already reduced to a very slender portion—with which the public was disposed to receive the official statements. In fact, many finding these reports partly incorrect, have not unnaturally been led to reject them altogether. It is against this result that we are anxious to guard our readers; for however indiscreet it may be to swell the list with cases such as those alluded to, still this cannot alter the fact that cholera—modified it may be, to a certain limited extent by difference in climate and constitution, but still in all its essential characters *the cholera* which has proved so destructive in all the countries it has successively visited in its progress from the east—is *now present in this metropolis*. We must keep in mind, that though the boy in the fable cried, wolf! when there was no wolf, yet that he *did* come notwithstanding; and was suffered to worry the flock unmolested, in the confidence which the false alarm had produced. Let us take care that we do not fall into

a like error;—the circumstances warrant nothing like panic, for the devastation has not hitherto been nearly equal to what it frequently is even in this country from small-pox and various other diseases; but the re-action since the first alarm has been so great, that an overweening confidence, and a consequent disregard of the necessary precautions, are now much more to be dreaded than exaggerated apprehension.

Musselburgh, who is at present in town with the view of publishing his opinions of the present cholera, had, at the dinner of the Marine Society on Saturday, the honour of being introduced to his Royal Highness the Duke of Sussex, and of having a short conversation on the subject of cholera."!! Bless the silly man! does he suppose that any one cares if he had been introduced to all the Royal Dukes in Christendom?

EDINBURGH BOARD OF HEALTH.

GRATITUDE FOR MEDICAL SERVICES IN EGYPT.

M. CLOT, of Abouzahel, surgeon to the Pacha, in consequence of his zeal and skill during the late visitation of cholera in Egypt, has been presented by his patron with a superb embroidered pelisse, and a diamond chain, valued at 10,000 francs: he is, besides, raised to the dignity of a Bey,—an honour never hitherto, perhaps, conferred on any *christian*.

CAUTION TO FEMALE PRACTITIONERS.

M. DUPUYTREN lately took up the brachial artery of a man who had contracted a false aneurism from an awkward bleeding in the median basilic vein. The phlebotomist, it seems, was a female—a midwife—who ventured to try her hand at venesection. In consequence of the many accidents of this sort which occur, M. Dupuytren seriously advises that the median basilic be in no instance meddled with,—any other vein that is at all prominent in the fore-arm, he thinks, would be to be preferred.

PUFF SUPERLATIVE.

THE following (which had evidently been sent as a paragraph) appeared in the *Times* a few days ago; it is one of the best among the thousand and one modes of puffing to which the present occasion has given rise:—

"[*Advertisement.*]—Dr. Brown, of

THE following is an extract from a letter addressed to us by a correspondent, in Edinburgh, well acquainted with the circumstances to which it relates. It is in reference to the idea to which we lately alluded, and the improbability of which we shewed at the time, that the small number of cases in Edinburgh was owing to the practice of concealment:—

They (the Board) are, indeed, still apprehensive that some cases of cholera which have proved mild may have been concealed by the people themselves, on account of the prejudice which here, as well as elsewhere, has been excited in the minds of the lower orders by the great and rapid fatality of the first cases of the disease; but they think they are warranted in affirming that no fatal case has been thus concealed. It is also possible that a few mild cases of the true cholera, although seen by medical men, may have been considered as diarrhoea, and not returned to the Board, while a few cases of simple diarrhoea may have been returned as cholera. But it is scarcely necessary to say that the Board have taken every means in their power to ensure the early detection of every case of the disease; and that all the medical men acting in connexion with the Board, to the number of 120, are anxious to furnish, to the best of their judgment, the most accurate information as to the cases which may occur in the hospitals or the districts of the town to which they are severally attached.—District Boards of Health have been organized, by the most intelligent of the inhabitants, more or less completely in all parts of the town; and their activity, the vigilance of the medical men,

and the fears of neighbours, afford the best security which we think can be obtained against concealment.

ANATOMY BILL.

To the Editor of the London Medical Gazette.

SIR,

WITH reference to Mr. Travers's letter contained in your last number, I beg to say, that the following plan would have the effect of affording protection to the public—from the hand of the assassin, as well as from that of the uneducated surgical practitioner—as far as the metropolis is concerned:—

Let the unclaimed bodies of the dead of workhouses, &c. be conveyed to a dépôt, or office (of which there might be two, one for the east, the other for the west end), under the superintendence and direction of the inspectors. Let this be the only legal source whence schools, practitioners, or others, may obtain subjects for dissection. Teachers of anatomy should be obliged to send in a return of the number of students in their classes to the inspectors, and the supply of subjects to each school *should be proportioned* to the size of the class. This is, I believe, nearly the system adopted in Paris, and there acted on with so much advantage. In London it would obviously afford an ample supply of *matériel* to the anatomist; it would prevent all *competition between schools* on the score of *subjects*; and it would completely *annihilate* the race of resurrectionists, a new order of whom, I am disposed to fear, must spring up under the operation of the new bill. Those who have endured the pain of witnessing, even for a moment, the frightful depravity of these abandoned wretches, can well estimate the advantage which must result from their total annihilation.

My sole object in animadverting on Mr. Travers's letters, was to raise my voice, feeble as it is, against any restrictions on the practice of anatomy, which cannot fail to affect the professional acquirements of the rising generation of practitioners.

In my first letter I stated that I conceived a *savour* of *monopoly* emanated from Mr. Travers's observations; of course I now gladly embrace this op-

portunity of avowing the injustice of the application of the term. I trust that Mr. Travers will not attribute the use of it to any want on my part of that respect to which his high station and character so justly entitle him.

I remain, sir,

Your obedient servant,

R. B. TODD.

37, Great Russell-Street, Bloomsbury,
March 6, 1832.

SUPPOSED CHOLERA IN MARY-LE-BONE INFIRMARY.

To the Editor of the London Medical Gazette.

SIR,

IN compliance with your request, I beg leave to forward you a brief account of the twenty-nine children affected with mild, or, to use a more appropriate designation, inflammatory cholera, admitted into the St. Mary-le-Bone Infirmary, under my care, up to March the 4th.

On the fourth day anterior to the irruption of the disease, a penny had been given to each of the children (148 in number, and from 5 to 14 years of age), which was expended in fruit and cakes. As the whole continued in perfect health until the fourth morning subsequent, and as a number became affected four days later, it does not appear to me that the disease and the previous indulgence can, by legitimate induction, be connected as cause and effect. To the boys (250 in number) a *shilling* had been given on the same day, the whole of which was expended in a similar manner: yet they have hitherto remained perfectly exempt from the disease.—Nothing deleterious has been detected in the diet, water, or culinary utensils.

Opening at a remote part of the girl's yard, to which part, at this season of the year, they have not access, is a small drain, to carry off the water, and which, as there is little imbibition, in consequence of the yard being partly flagged and partly paved, is thoroughly washed with every shower. On examination, not the slightest offensive exhalation was found to proceed from it*. Other

* It may be remarked that the examination took place long before any persons unconnected with the Institution had any knowledge or suspicion of the existence of the drain, the information having been communicated to them at the Institution.

drains of the same kind exist in various parts of the establishment.

Viewing all these circumstances in connexion, and recollecting that the Clapham cholera, a disease identical in its nature, and not more severe than the present affection, required for its production the insufferable effluvia from an unknown and unsuspected sewer, containing the "accumulated filth of perhaps half a century," it becomes, at least, *improbable* that the drain in question was the source of the disease.

In mentioning these particulars, I would not be understood to combat the opinion that atmospheric miasm is capable of giving birth to the disease; but I wish merely to make a rigid statement of facts, in order that its due value may be assigned to every circumstance.

The symptoms were, in all, of the same nature—different only in degree. On admission, which generally took place within from ten minutes to two hours from the first uneasy sensations, the skin was in some partially, in others universally cold, feeling in the extremities like that of a corpse; its colour in the hands, arms, feet and face, was more or less deeply livid, and, where the natural complexion was high, it was of an intense livid purple. In a few only, whose complexions were *naturally* pale, did the aspect become cadaverous—eyes, in all, dim and glassy, sometimes lividity around them—features slightly shrunk—pulse either imperceptible or extremely small and feeble; in every instance slightly irregular: its range from 80 to 110; respiration accelerated and irregular, with sighings; tongue whitish, clean, moist—never cold; nausea in all—vomiting in most; rarely of ingesta; generally of a turbid watery mucus; bowels, in most, regular; in a few, two or three open evacuations had preceded the attack; urine in no instance wholly deficient; in every instance pain in the epigastrium, increased by pressure, sometimes extreme tenderness; occasionally the pain pervaded a greater or less extent of the abdomen.

Finding that dry heat, especially the hot-air bath, was sufficient to rally the circulation, and satisfied in a few minutes that the disease was of an inflammatory nature, I abstained from all internal stimuli; and when, in the course of from ten minutes to half an hour, the pulse had become distinct, though small,

weak, irregular, and from 110 to 136, I ordered venesection to the extent of from 8 to 14 ounces, according to the age; and 10 or 12 leeches to the epigastrium—to be followed by fomentations and the hot bran-poultice: 7 or 8 grains of calomel, and one of opium, were directed to be given after the leeching.

This plan, which was pursued in every case, was successful in direct proportion as it was promptly and effectually put in execution—the pulse becoming fuller, stronger, and more regular, and all the other symptoms greatly abating. On the following morning a slight recurrence of epigastric pain rendered a second application of leeches necessary; a second, and in some a third dose of calomel, (*gr. v.* without opium) was administered, and in the evening two or three drachms of castor oil. During the day there was generally one motion, either of retained fæces, or of a watery slime, remotely approximating to the character of gruel. On the following morning there were one, two, or three copious dark-green evacuations, and the patients were convalescent.

In the instances in which, from the press of business, the leeching was postponed for several hours, the cases became severe and protracted almost in the direct ratio of the time lost—three or four applications of leeches becoming necessary to subdue the abdominal pain, the evacuations both upwards and downwards possessing more of the rice-water character, in proportion to the severity of the case, the thirst being craving, the restlessness and anxiety great, and the pulse from 130 to 150, small, sharp, and irregular.

Amongst these there was a great tendency to a *sudden* secondary collapse, which occurred in several, the pulse becoming imperceptible, the surface cold, the eye sunk, glazed and semi-apart, the consciousness impaired, the voice puny, and the face shrunk and cadaverous.—This state was generally attended with a recurrence of pain in the epigastrium.

In the first instance of this collapse, (a patient in whom the first bleeding had been postponed longer than in any other) after rendering the pulse barely perceptible by the hot air-bath and a hot bran-poultice on the stomach, I bled the child from the arm, down which three ounces and a half of black blood

trickled slowly. In an hour and a half the pulse was distinct, and 145; and on the following morning there was a smart reaction, with intense epigastric pain, for which three subsequent applications of leeches during that and the following day became necessary.

Judging that the inflammation was too severe to be subdued by such depletions as the strength of the patient, aged only six, would allow, I gave mercury, by anticipation, to the extent of three grains every four hours, with inunction on the groins. On the third day she had perfectly characterized rice-water vomiting, and purging, and was still in imminent peril. On the fourth morning she had a dark bilious evacuation, and was safe, no material symptoms remaining but those of reaction from loss of blood. Several patients have been in similar circumstances—one only last night, (the sixth), and all have recovered under similar treatment.

When bilious dejections had appeared, and the epigastric pain had ceased, nothing further was requisite than to evacuate the bowels of the redundant bile by one or two grains of calomel, or an equivalent quantity of hydr. cum. creta, with six or eight of rhubarb, followed by two or three drachms of ol. ricin. both doses being repeated if necessary.

The diet was limited throughout to a small quantity of thin gruel thrice a day, and the only drink allowed was a table-spoonful of tepid barley-water, or toast-water, occasionally.

It may here be cursorily remarked that the pulse was in every instance slightly irregular, even during smart reaction, until the epigastric pain was finally subdued. The blood drawn from the arm trickled slowly, and was dark in the worst cases: in others it flowed in a small stream, and was more natural. In one it remained liquid, without separating into serum and crassamentum; in the others the separation took place, but the clot was unusually soft, and exhibited no buffy coat. There was a remarkable tendency to a sudden recurrence of the abdominal inflammation after it appeared to have been subdued; and unless its progress was promptly arrested by an application of leeches, the patient relapsed, in the course of two or three hours, into a dangerous state, sometimes attended with the secondary collapse already described. In

none of the above cases were there cramps.

The only fatal case that occurred was the first attacked, a girl, æt. 14. She had been affected with violent purging and vomiting for eleven hours before admission. The coldness was universal; extremities livid; lips and cheeks (naturally very florid) purplish; eyes slightly depressed in the orbits; palpebræ livid; great epigastric pain and tenderness; pulse imperceptible; great anxiety and jactitation; craving thirst; cramps in the arms and abdomen; hiccup.

She had vomited various internal stimuli given before I saw her. The heat was restored in twenty minutes by the hot air-bath, frictions, &c. She retained a calomel and opium pill; leeches were ordered, but did not fix. On the following day she was worse in all respects, and had perfectly characterized rice-gruel evacuations both above and below. She was bled, but without relief; became cold again; breath cool; eyes depressed; face shrunk; purging and vomiting of all injecta. Died at midnight.

Secio Cadaveris, twelve hours after Death.—"Omentum drawn down to the pelvis. Stomach contracted; mucous membrane much corrugated; the rugæ in many parts highly vascular. It contained much dark-coloured fluid.

The mucous membrane of the duodenum was turgid, and of a pink colour, from minute vascularity; and the mucous glands, both here and in the upper part of the jejunum, were more developed than natural, some being nearly as large as mustard seeds.

The lower part of the jejunum, and the whole of the ileum, had, externally, a pink colour, with an unusually fresh appearance, resulting from the uniform turgescence of the minute vessels in all the tissues of the bowels.

The upper part of the duodenum contained mucus in more than the usual quantity, which adhered rather closely to its surface. In the lower part of the duodenum, and a small adjoining part of the jejunum, the mucus was in large quantity, very tenacious, and of a dirty greenish colour. The villous surface of the remainder of the jejunum, and about three-fourths of the ileum, was thickly invested with a dryish, white, curdy matter, of an unctuous feel.

In the lower part of the ileum, the se-

cretion was more abundant and more fluid than hitherto: it was pearl-coloured, and not unlike the half-boiled white of an egg. The colon and rectum contained a large quantity of a fluid resembling thin gruel, but of a leaden or dark slate-colour, holding in suspension minute particles of a black matter. The contents of the stomach and intestines had a sweet and oppressive odour. No faecal scent could be perceived in any part of the canal.

The mammary and coronary veins were perhaps fuller than usual, and the right side of the heart contained a considerable quantity of fluid blood, with a small coagulum. The under part of lungs was more loaded with dark blood than is common. There were slight, but old, adhesions over a large extent of each lung. Bladder was contracted on half an ounce of urine, to a size somewhat less than that of an egg."

(Signed) J. SIMS, M.D.
H. MAYO.
J. PERRY.
J. HOPE, M.D.

Had the depletory measures been carried as successfully into effect in this as in the other cases, there is reason to believe that the girl might have recovered; since others were rescued from a stage of the disease perhaps as far advanced as was presented by her on admission.

On viewing the whole of these cases in connexion—on comparing the symptoms with the post-mortem appearances—and on placing the general history thus presented beside that of cholera, in its various grades and habitudes, as existing elsewhere, whether in Asia or Europe—the essential nature of the affection becomes, so far as I can judge, too manifest to require a comment*.

I am, Sir,

Your most obedient Servant,
J. HOPE, M.D.

13, Lower Seymour-Street,
March 7, 1832.

P.S.—Friday, March 9, 2 o'clock.—
Six of the children have slight pytalism:

* A rumour got abroad, that, in one child, a varioloid eruption existed. This was a callous chronic eruption, of about a dozen spots, on the hands and one foot, in every grade of progress, from a pimple to a blue cicatrix; as was carefully pointed out to all who inspected it. There was no other eruption but of chilblains on the hands and feet of one or two children.

with this exception, the whole are well. It may be remarked that, though the affection is designated "mild," in contra-distinction to "malignant" it is of a highly inflammatory nature, tending rapidly to the destruction of life, and requiring prompt treatment and careful watching.

MEDICAL MEETING AT THE FREEMASONS' TAVERN.

IN consequence of an advertisement in the daily papers, inviting the medical profession of the metropolis to meet at this place on Thursday, for the purpose of forming an Association, or Board of Inquiry, relative to the cholera, from a hundred, or a hundred and twenty, persons attended.

DR. STEWART IN THE CHAIR.

After the Chairman had opened the business,

MR. SCOTT proposed the first resolution, and prefaced it with some appropriate remarks; it was to the effect—That an Association be now formed, whose object shall be to inquire into, and investigate practically, the nature and treatment of the disease now prevailing in this metropolis.

MR. WHEB (York-Street, Marylebone) was surprised at the small attendance which he saw present, after the public invitation that had been issued to the whole profession. Such a meeting as this would by no means have the effect of tranquillizing the public mind. For his part, he thought it anything but creditable to the medical profession to have any doubts or disagreements on the subject of cholera, when there was such a mass of evidence, to elucidate its nature, before them. He had made up his mind about it. (Symptoms of impatience.) It was his wish to put an amendment to the preceding resolution, namely—That this meeting had the utmost confidence in the wisdom and patriotism of his Majesty's Government with regard to their measures for the protection of trade and the preservation of the health of the metropolis, but that the meeting was of opinion, that an addition of some more medical members to the Central Board of Health would be highly beneficial. He concluded with some censures on the public press, which were listened to with frequent interruptions.

MR. KING, *pro forma*, seconded the amendment, as nobody else seemed willing to do so.

DR. ELLIOT rose to express his dissent from the proposed objects of the meeting.—Instead of forming a new association, they ought rather to turn their thoughts to how they might best co-operate with the existing

Boards. The present step, he was of opinion, was calculated to aggravate the public apprehension; for which reason, he would support the amendment of Mr. Webb, for the better organizing the established Board of Health. Whatever resolutions should be now adopted, he advised them to transmit to the Central Board; and it would be full time for them to think of forming an independent association, when their communications should be either not attended to or ill-received by existing authorities.

Dr. FERGUSON said, that there was no courtesy to be expected from the Central Board: they should be met "front to front," and taught by a proper example what was their duty.

Dr. ELLIOT could give, from his own personal knowledge, a contradiction to any statement about want of courtesy in the Board. He was one of a recent deputation from St. Pancras, which had waited on the body in question, and received from them every courtesy.

The amendment was negatived, and the original resolution carried by a large majority.

Dr. J. JOHNSON, in rising to propose the second resolution, denied strongly the efficacy of the present Central Board, having, as it had, but three medical men in its composition: their time was totally taken up with correspondence, and it was impossible they could devote the necessary attention to sifting the returns. And now more than ever was it imperative on the profession to be practical in their proceedings. The disease was now in every parish in London, and the people should not be permitted to suppose that the accounts given of it by the profession were exaggerated. His motion was,—That the medical men of the metropolis generally, be invited to co-operate with the association.

Dr. SIGMOND was the seconder. He pronounced a high eulogium on the conduct of all the practitioners engaged in combating the disease, but particularly on Messrs. Evans and Hooper, of the Southwark Cholera Hospital, by whose unwearied exertions so much positive good was effected. It was not two guineas, nor twenty, a day, that would remunerate them for their trouble.

Mr. EVANS returned thanks to Dr. Sigmond for his complimentary observations, and took occasion to announce that he should be glad to receive the visits of his medical brethren at his hospital at all times; but with respect to the two guineas or twenty just alluded to, he (Mr. E.) did not expect to receive *one farthing* by way of remuneration.

Dr. DILL decidedly looked upon the disease, now in the metropolis, as wholly new to the profession: it was eminently calculated to exercise their best talents in exploring its nature and treatment. He would therefore advocate the utility of the associa-

tion now forming. He denied that they had any intention of "bearding" the Central Board, or of undervaluing those of the parishes. He begged to move—That a deputation be appointed to wait on, and apprise, the Board of Health of the result of the present meeting, or on the Privy Council for its sanction to their proceedings.

Mr. KING seconded the motion, and was proceeding to give his reasons for thinking the cholera non-contagious, when he was obliged to desist.

Mr. HOOPER considered such an association as the present as likely to be highly useful. He, in conjunction with his friend Mr. Evans, had now been for some weeks engaged in the treatment of the disease, and he protested he was now just as wise as when he began: he was never so much at a loss in all his practice. It was a most fatal complaint. He could safely say that nineteen out of every twenty sunk under it when once the patients reached the stage of collapse, as they generally did before they were carried to the hospitals. It was hopeless work for practitioners.

Mr. GREVILLE JONES advocated the new association on the ground of the utility which it promised in conducting experimental inquiries, and he thought that there was no necessity for making tenders of co-operation to the Board of Health.

Dr. J. JOHNSON feared that if they did not do so, the Board might shut their hospitals altogether against them, (No! No!)

Dr. GRANVILLE now rose, and in strong terms complained of the inefficiency of the Central Board. It was, he said, a Board of Execution, and not what it ought to be—a Board of Inquiry. He referred to the awkward predicament in which Mr. Poulett Thomson found himself, a few nights ago, in parliament, when he was asked for some account of the reputed cases of cholera at Marylebone; and he (Dr. G.) noticed the anomalous forms of the returns in which such and such cases are stated to be "reported as cases of mild cholera." In conclusion, he moved, that a committee of twenty-four, with power to add to their number, be now formed, in order to draw up the requisite rules, and to organize the new association.

This resolution was immediately carried. Among those who were the earliest mentioned as members of the committee we noticed Drs. J. Johnson, Stewart, Granville, Uwins, and Elliot; Messrs. King, Jones, Evans, Hooper, &c.

One more resolution was then carried for opening a subscription to provide for the purposes of the association. It is understood that it is not to go beyond a sovereign a-head, and to be perfectly voluntary. After this thanks were unanimously voted to Dr. Stewart for his very excellent conduct in the chair, and the meeting broke up.

ROYAL INSTITUTION.

Friday 24th Feb. 1832.

EDMUND S. HALSWELL, ESQ. VICE-PRESIDENT, IN THE CHAIR.

"On the Genius of Improvisation, by an Italian Improvisatore."

ACCORDING to the teaching of the Count, all nations and all times have had their improvisatori. Among the Hebrews, he observed, extemporaneous poets were common, and David was claimed by our lecturer as an improvisatore: the Greeks, the Phenicians, and the Romans furnished their quota; and the modern States of Europe were said not to be exempt from this *disease*, though none approach in excellence the Italians. Lord Byron was said to have been an improvisatore; but if the pamphlet, given as a specimen of the twelve lines extemporaneously composed, and on which this claim is founded, were unsorted samples, it is as well that the rest have been forgotten, as they would not have added greatly to his fame.

The anecdotes and illustrations interspersed through this long, but very far from tedious lecture, combined with the animated manner of the Count, excited general attention, and ensured him great applause from the fullest audience we ever recollect to have seen collected within the walls of this theatre. Many of these anecdotes depended more on the appositeness of their introduction, and on the manner of their recital, for their success, than on the matter they detailed: some two or three, however, may not wholly lose their point, and those we shall present to our readers.

The Count offered a poor improvisatore a ducat to tell him, in extemporaneous verse, the difference between a physician and an assassin, the answer to which conundrum, when forced from the shackles of verse, and which excited much laughter, and from none more than from several members of the College present, was shortly this, that "an assassin kills before he robs, and a physician robs before he kills." The two other learned professions came in also for a stroke of the improvisatore's; but the points would not tell in repetition.

Most of the improvisatori named, appear, from our lecturer's account, to have died apoplectic or mad, and the Count gave it as his opinion, that the power of improvisation is a symptom of mania. "Thirty years ago," continued he, "I was myself an improvisatore and a madman: when the fit was coming on, I felt a great internal commotion, trembled, seemed to be swelled, as if a large ball or barrel was within me; my sight failed me, my throat was irritated, I had singing in my ears, and subsequently fell to the ground convulsed." He was ill afterwards for three or four days, and his physi-

cian recommended a keeper, and pronounced him mad. In 1811, Gall examined the configuration of his skull, and declared that if he had not known him to be a soldier, he should have thought him a poet, and that he could detect many indications of madness, which, he quaintly observed, "was another consolation." The lecturer distinguished between true and false improvisatore, i. e. between those who are really under the influence of what he termed disease, and those who merely imitate some of its more notable symptoms, for the sake of gain. The proofs he gave of his own power, as an extemporaneous poet, were very satisfactory, and the several sonnets he composed were all to the same rhymes, which, although casually fixed on, were peculiarly difficult.

In the library were numerous specimens of natural and mechanical curiosities, sent from Mexico by Capt. Lyon; among which we noticed an oak leaf, upwards of 18 inches long by about 14 wide.

REPORT OF CHOLERA IN LONDON, UP TO THE AFTERNOON OF FRIDAY, MARCH 9, 1832.

New cases since our last report, 304
Deaths 150

	Cases from Commencement	Deaths from Commencement
Afloat in the River	26	17
Poplar.....	3	1
Shadwell	1	0
Limehouse	16	13
Ratcliffe	3	3
Rotherhithe	7	5
Southwark	135	88
Newington Butts...	41	18
Lambeth	30	23
Christchurch	18	11
Westminster	6	3
St. Marylebone ...	30	2
St. Pancras	4	4
St. Giles's	18	8
Whitechapel	12	7
St. George's East .	6	3
Bethnal Green.....	3	2
St. Luke's	6	4
Total.....	415	212
Cases reported } from other } places }	26	22
Grand Total.....	441	234

Total number of cases throughout }
England since the commence- } 6410
ment of the disease }
Deaths... 2040

THE LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MARCH 17, 1832.

LECTURES

ON

THE THEORY AND PRACTICE OF
MEDICINE;

Delivered at the London University,

By DR. ELLIOTSON.

PART I.—LECTURE XXIV.

Intermittent Fever—continued.

I MENTIONED, gentlemen, at the last lecture, that, to the production of those exhalations which arise from decayed vegetable matter, and which are the cause of ague, three things appear requisite: in the first place, of course, the existence of a certain quantity of vegetable matter; secondly, a certain degree only of moisture; and, thirdly, a certain degree of temperature: that if you take all these three into account, and the relative proportion of each, you will easily explain why the same circumstances which give rise to malaria at one time, may put a stop to it at another. I mentioned, that if a part be just sufficiently moist for putrefaction to proceed, and more water be added, putrefaction will then decline; whereas, if some be taken away, the effect may be precisely the same. So with regard to temperature. A greater or a less degree of temperature must necessarily have the same effect. You will therefore be able to conceive the explanation of the following circumstances.

Influence of Moisture in producing or destroying Malaria.—In Batavia, for example, the rainy season is comparatively healthy, because the rivers and canals are then plentifully supplied with water, which flows on rapidly, and all the marshes are inundated; but in the months of July, August, and September, the waters are nearly evaporated, and fever is then much more malignant than at any other period. On the western coast of Africa, and in some of the West India islands, and especially Barbadoes, fevers are rare in

the long continuance of dry weather. There is not much water there; and in dry weather, what there is, is evaporated; so that the vegetable matter becomes too dry for putrefaction. Fevers on the western coast of Africa are observed to be arrested in long drought; whereas, in Batavia they cease during the rainy season. At Charlestown, in South America, which is a swampy situation, bilious fevers prevail in hot summer weather; but in 1752 there was very intense heat, the thermometer was 98° even in the shade, the air was glowing, so that all vegetation was parched up, and universal health prevailed. Decomposition was put a stop to by the intense heat. In summer, fevers frequently prevail in certain parts, on account of the rivers diminishing to a great amount. All rivers, of course, diminish in summer, but some lose a very considerable portion of their depth, or rather, we should say, of their breadth. The lake Biviere, in Sicily, loses two-thirds of its dimensions in summer; the lake Cagliari, in Sardinia, loses much; and so does the Caspian Sea. Of course, the sides of a river are always more or less dry, but in summer a great portion becomes half-dry; there is just sufficient moisture for decomposition to take place, and therefore ague prevails when the rivers are shallow.

I mentioned that the retiring of the sea will frequently cause agues, by leaving a marsh only where the part was previously deluged; and I also stated that they may be produced by the sea breaking into a part which is not drained—the sea falling back again, but a great portion remaining for want of drainage. The Goodwin Sands are an instance of a marsh destroyed; the land is lost, but there is no longer a marsh where there was one; an immense deposit of sand has filled it up.

Marshes not necessary to the disengagement of Malaria.—I mentioned that it was not necessary that there should be an absolute marsh for the disengagement of malaria to take place; that any portion of water, how-

ever small the collection, containing decayed vegetable matter, might be sufficient for that purpose. Many places, therefore, produce disease which have only a small pond, or a small lake. Many places, no doubt, are suffered to give ague, and aguish diseases, from circumstances which are entirely within control. It is said that an inveterate ague was produced by the canal at Versailles, which was little larger than a fish-pond. Monfalcon quotes an instance of a fish-pond at Chantilly which was remarkable for its pestiferous character. Many country residences, there can be no question, have been made unhealthy from the custom of ornamenting them with woods and little lakes of stagnant water, *pièces d'eau d'agrément*, as the French call them—*lacs artificiels*. Edinburgh is now any thing but favourable to ague, but there was formerly a loch, called North Loch, which was famous for it. Agues prevailed in that district till the spot was drained, but now no agues are there at all. When I say ague, I mean fevers of an aguish character, whether remittent or intermittent; and many diseases of this description unquestionably are mistaken every day for real typhus. In order to the existence of ague, the situation need not be low, nor need there be a marsh. If water be hemmed in at any height, of course vegetable decay may take place just as though it were situated low.

I believe the fevers of ships are very often remittent fevers, but are mistaken for typhus, and often have arisen entirely from the bilge water. Clayey soils are the most favourable for these fevers, on account of the clay retaining the moisture, whereas gravelly soils let it escape. You might imagine, perhaps, that the Delta of the Nile would be a great source of ague. Ague, Sir James Macgrigor says, is not unknown in lower Egypt, but it is only seen sporadically; and the reason is, there is no stagnation of water, notwithstanding the existence of so much vegetable matter as is brought down to the Delta. In fact, there is agitation equal to too much water, so that putrefaction does not take place. Strabo remarks the healthiness of Lower Egypt, and ascribes it to this cause. On the other hand, ague will continually occur in spots where there is little more than a pool, or a puddle, but then the water is completely stationary. Even a ship may contain a source of malaria. Captain Smith says he never knew fever in any of his ships during a long service in the Mediterranean; and Captains Parry, Cook, and Sir Henry Baynton, all endeavoured successfully to preserve the health of their crews. They all had the holds of their vessels washed out daily, by means of plugs, till the water came out perfectly clear, so that no accumulation of filth could take place—no accumulation of vegetable matter. This

fact may be contrasted with one relative to the Powerful, a 74, whose crew, on their voyage from the East Indies, were nearly all disabled by fever, and the cause was found to be merely the ballast, which consisted of moist and putrid mud. Sugar ships have the greatest mortality, and sugar is a vegetable matter. The most unhealthy cargo that a ship can carry, is sugar. The castle of Flamandville, in Normandy, is situated high, but an endemic prevails around, in consequence of a mere moat, containing a quantity of vegetable matter, which gives forth pestiferous effluvia; and a family, resident at the place for a length of time, was at last exterminated; but before that, they underwent a loss of mental and corporeal power. The same thing has been observed in the neighbourhood of many castles. The besieged and the besieging have both suffered when the baronial castles were attacked; and the source of all the disease was the moat surrounding the castle; and the sentries were the first attacked. Down to the middle of the last century, one-third of the inhabitants at Bourgen Bresse were incapacitated, in consequence of the ditches around the fortifications containing a quantity of decayed vegetable matter. At Havre de Grace the soldiers were once known to be giddy, and experience violent headache five minutes after they had approached the ditches, and then had violent fever. I saw several cases of ague in London arising from the moat around the Tower. A stupid negligence in regard to the moat prevailed for a length of time; a great accumulation of filth took place, and I continually saw patients with ague from that part of London. The first case which I saw was that of a lady, a relative of a medical gentleman in the neighbourhood, and not having been in the country, she could not imagine how she had contracted the ague. At last she told me that she was in the habit of taking exercise on the ramparts of the Tower, and the circumstance of the moat, of which I had read so much in books, immediately occurred to my mind. I inquired whether it was in a filthy condition, and learned that it was, but that the authorities would not cleanse it out. One department thought it was not its business, and another not its, and some medical men laughingly said it could not be supposed that they should press the subject, because it brought grist to the mill. I believe, however, that it has at last been cleansed out, after the representations of the profession. Harbours, moats, and mill-dams, have all been sources of ague. In a mill-dam the stream is rapid, but the sides may be flat and wet. Flax-grounds have frequently been proved to have given origin to the same disease, in consequence of the flax lying upon the ground in a state of decay. In Italy and

Germany, fevers frequently prevail in the neighbourhood of flax-grounds, and these are instances of ague in dry, sandy, high places, from the hemp being dried there, and the fevers regularly cease when the drying season for hemp is over. Indigo manufactories have also been the sources of this disease. The inundation of a cellar has frequently caused these fevers to prevail in a family, and the passage of a drain under a house has frequently caused the inhabitants to continue the victims of fever—that is to say, the house has been rendered unhealthy, and all the people residing in it have been more or less attacked with fever from time to time. Nay, the removal of stores has frequently been productive of fever. The water under the stores has been impregnated with vegetable matter in a state of putrefaction; the removal of the stores has let loose the exhalations which were pent up, and the men employed in removing them have been known to suffer an attack of fever.

When we consider the peculiar susceptibilities of different individuals to the effects of morbid causes, we shall not be surprised that persons will occasionally catch an ague from the most trivial exposure to malaria. We know that some persons are singularly liable to be affected by particular causes. Many persons on approaching a hay-field will be seized with asthma, particularly if the grass be sweet-scented. I believe it is not the hay which produces the affection, but the flower of the grass, so that they are attacked if they approach fields at the time they are in flower, and which is at the period of hay-making. It is said that it is not at all uncommon for persons to be so affected from *ipecacuanha*. Some persons have been seized with asthma from *ipecacuanha* being powdered in the house; nay, it is said that some persons have been seized with it merely from a box of *ipecacuanha* lozenges being in the same room; at any rate, a trivial exposure to this drug frequently produces the disease. We must have seen persons thrown into a violent salivation from a grain of calomel; nay, I have seen it arise from the application of a little red precipitate ointment, or mercurial ointment, to a sore leg. From these various circumstances, we must suppose that some people exist who are peculiarly susceptible of the effects of malaria; and therefore I can imagine sporadic cases of ague to take place from a person merely passing through a market in which decayed cabbage leaves shall be lying. We all know that there is more or less putridity in these parts; they continually emit a noxious smell; and I can conceive, that even in a most healthy town, you may thus have a cause of ague. Ague is sometimes ascribable to very common and trifling causes, without at all invalidating our general conclusions. A very slight collection

of decayed vegetable matter may be supposed sufficient to produce ague in a person extraordinarily disposed to it; and, therefore, when we cannot account for the existence of the affection from a person having been near a swamp, still it is very possible that the case may be one of extreme susceptibility—that a minute quantity of putrid vegetable matter has been near the patient, or minute malarious exhalations have been wafted to him. And it is to be remembered that these miasmata will extend to a very great distance, and in very aguish seasons places which are usually healthy, and not situated near much stagnant water, frequently become unhealthy. It is the opinion of Dr. Macculloch, (whose works are the best on this subject, and which consist of two volumes on Marsh Fever and Neuralgia, and one volume on Malaria) it is his opinion, and also that of several Italian physicians and able military men, that a single inspiration of malaria may be quite sufficient to cause disease. If all this be the case, and there is no reason to doubt it, you see that we are not by any means to reject the belief that the disease always arises from such exhalations, because we can hardly tell how a particular individual, in certain circumstances, may have been exposed to them.

Period of Attack.—As to the time of attack, occasionally the exhalations have been so intense that labourers, in breaking up ground in France and in Italy, have died instantly; and others have been seized with giddiness, and some have even died, on looking into a ditch, or drain, around which aguish fevers prevailed. Many allow that fever may begin in half an hour after exposure.

Distance to which Malaria extends—Influence of the East Wind.—Then, as to the distance to which malaria will extend. It is said that the ill effects of stagnant water are known to extend three miles in Italy; that they have reached from the lake Agnano as far as the convent of Camaldoli, which is situated on a high hill. Lancisi, who first referred this disease to marsh miasmata, says, that, as thirty ladies and gentlemen were making an excursion of pleasure up the Tiber, the wind suddenly shifted to the south, coming over the Pontine marshes, and twenty-nine were instantly taken ill—one only escaping. They were all well before; but the wind coming over the Pontine marshes, was impregnated with malaria, and twenty-nine were instantly affected by it. Dr. Lind, of Haslar Hospital, who wrote on the Diseases of Hot Climates, says, that when Commodore Long's squadron lay off the mouth of the Tiber, two vessels that were close to the shore were affected, while the rest, lying a little farther off, escaped. The difference of half a cable's length from the shore has

caused vessels to suffer or escape; yet Sir Gilbert Blane says, that, in the West Indies, (and he heard the same respecting the channel leading to Calcutta), the malaria was observed to reach a distance of 3000 feet, and more. In Zealand the distance (he believes) is far less, owing to the comparative mildness of the malaria, and also of the temperature. It is the opinion of many authors, that the hotter the climate the farther does the malaria extend; that if the quantity of vegetable matter be great, and the heat intense, it produces its effects at a very considerable distance; but in cold climates, the vegetable matter and heat being less, the exhalations are milder in their nature, and less copious; and therefore it is supposed by Sir Gilbert Blane, who had great experience, that in Zealand the distance is far less than in many other parts. He says, that, at the time of the Walcheren fever, no persons on board ship were seized with the disease which was prevailing on shore, although the channel between Beveland and Flushing was only 6000 feet wide, and some of the ships lay nearer to one shore. It is the opinion of many that the malaria will not extend to any great distance. Sir Gilbert mentions, that when the ships watered at Rockford, he found that if they anchored so near the shore as to smell the land (the smell of land air is sometimes very perceptible at sea) they were affected; but if they remained only two cables' length from the spot where they smelt the malaria, no inconvenience was experienced. At Pensacola, it is said by Lind that a regiment lost 120 men, and eleven out of twelve officers who landed; but the whole crew of a man of war, which was lying one mile from the shore, remained perfectly healthy. However, Dr. Macculloch, who has investigated this subject more than any one else, but who has, perhaps, carried his views farther than most other persons will think justifiable, believes that malaria may be conveyed to an indefinite distance. He is convinced that it is conveyed from the banks of the Thames over the hills of Kent, even to a distance of some miles from the coast. He argues first, analogically, that dogs will smell the land long before it is visible; and, on the other hand, that the sea may be smelt very far inland; that whales or sharks may be smelt, both powerfully and offensively, as far as the spouting of the animal is visible from the mast-head; and therefore, he says, if these things can reach such a distance, and odours produce such an effect, so may malaria or vegetable exhalations. He mentions, that fogs and clouds of course go to an immense distance; that it is a fact that malaria easily unites with fogs and clouds; and therefore concludes, that as far as these will go, so far may the malaria extend.

He asserts, however, as a fact, that he has found records in ships' books, of malaria having produced fever five or six, or more miles from shore, and danger being instantly sensible by the smell; so that the ship's crew would go below, or weigh anchor and run to sea. He says it is well known that points in the coasts of Suffolk and Norfolk, and the eastern coast of Scotland, have ague where there is no local source of malaria for many miles, and that there are some points where there is no source of malaria for even a greater distance than that; and therefore he contends that it must be brought from Holland and the northern shores of the continent. The effect of the east wind, in causing and renewing aguish diseases, is well known; but it can no more be the simple air itself, than pure water, oxygen and hydrogen, can produce ague. Wind, if pure, can be poisonous no more than pure water; and therefore the noxious property must be, as in the case of water, contained by it. Now the east wind is perfectly innocent, perfectly innocuous, in the regions which it reaches after having passed over healthy countries; and, *vice versa*, other winds are as unhealthy in some parts of the world as the east wind is here; that is, where these winds come over a marshy district, exactly as our east winds do. This is another argument adopted by Dr. Macculloch. He says, still pursuing his argument, that land under the trade winds, and ships in the equatorial seas, escape; and that St. Helena, being so exposed to the east wind, ought never to be free from ague; whereas that is not the case. If the east wind blows over a frozen continent, or simply blows across the sea, though, of course, in its passage it must bring moisture, yet it is found to do no harm; to be injurious, it must blow over land where there is vegetable matter and moisture. For this reason, in one-half of the globe the east wind is exceedingly injurious from the month of March to the month of October. In French Flanders, the south and south-west winds do the same harm as the east wind in other countries. The east wind in summer is dry; hence it has less conducting power; and, thus, though we get ague from the Dutch ditches in spring, we do not get their remittent fevers of autumn. The east wind is so dry at that time, that it will not conduct the malaria; for a certain degree of moisture is as necessary for its conveyance as for its production.

To shew the healthiness of the east wind where it does not pass over a marshy district, I may mention that it is the salutary breeze of the burning islands of Western America, while many parts of France, Germany, Poland, and Russia, never feel its injurious effects. Dr. Macculloch says, that although "the east wind, after sweeping the burning sands of Africa, makes the side of

Sardinia which is exposed to it a desert, and in Minorca will not suffer an orange leaf to protrude beyond a garden wall, it is before the east wind that Mount Edgcomb roots its splendid trees even into the sea, not daring to shew a leaf to the western ones; while in Southampton river it is precisely the reverse." The east wind may be injurious in another way—namely, from its excessive heat and dryness, or its coldness and moisture. The Harmattan will not produce ague, but it is excessively hot and dry, and therefore an injurious wind. We all experience an unpleasant feeling from the east wind in spring; not by its producing ague, but by its being excessively cold and excessively moist.

Distant parts, however, suffer more if they be hilly; and frequently the immediate neighbourhood of a marsh will escape in some measure, while a village or a town at some distance, if it be situated on a hill, suffers exceedingly. This fact seems to be capable of explanation, from the circumstance of hills attracting the moisture, and every thing united with it, in the clouds. Malaria being conducted by moisture, and moving with the clouds, and the clouds being attracted to the sides of a hill, their moisture and every thing they contain is deposited; and thus you see that a hilly part may suffer much although it is situated at a great distance from a marsh. I mentioned that the convent of Camaldoli, which is situated three miles from the lake Agnano, suffers from ague, yet it stands upon a high hill. At Malta, the malaria produced under a cliff never affects the spot itself, but it produces direful consequences on a village situated above it. At Weymouth—in our own country—it is said that the back water rarely affects the immediate inhabitants, while those at a distance suffer. At St. Austle, in Cornwall, the disease prevails from some marshes at St. Blaizy. At Erith there is less ague in general than could be expected, but houses that are situated high above it often suffer very severely.

Sometimes a distant part will suffer, not from clouds conveying the moisture, but from currents of air. The locality of a part may be such that a current will proceed in some particular direction. If currents pass over a swamp, the part exposed to them will necessarily suffer; and if there be a peculiarity in a current, you will have a part affected which you could not, *à priori*, expect to suffer. Indeed, in Capt. Symth's Statistical Table of Sicily, he mentions seventy-six unhealthy towns and villages, and out of that number thirty-five are situated on hills or declivities, and are at a great distance from a marsh. It is supposed that the southern wind, blowing over a marsh, and tending upwards by its temperature, affects high towns (when the wind blows from the south, being a warm wind, it has a tendency to ascend), while the colder northern wind

does not affect those houses situated on the other side of the swamps, though placed equally high. We can easily conceive of a swamp, and towns on each side. When the wind blows south it will ascend, and high houses will suffer; whereas, if it blow north, it will not ascend to a house of the same elevation, but remain near the earth, and in that way the house will suffer very little, or not at all. Hence you are to take into account a number of circumstances, when you see ague prevail in districts where there is no marsh in the immediate neighbourhood.

There are, however, some singularities in the prevalence of this disease, which cannot be explained, or at least many, perhaps, will not be satisfied with the explanation that is given. It is said, that on the high road to Chatham and Feversham, comprising a distance of twenty miles, the effect of malaria is only noticed on one side of the road. The injurious lands which afford the malaria are situated about a mile off, but it is only detached houses on one side of a level road that suffer. It is, however, to be remembered that the dews will spread in a similar way—that they will spread to a particular spot, or stop at a certain bush. Every one may notice the same circumstance in the case of a hoar frost. Now, as far as these will extend, so may malaria; and they will explain the singularity of such an occurrence, in some instances. Dr. Bancroft was credibly informed that only one side of a particular street in Rome was affected by ague or aguish diseases; and Baglivi says the healthy spots of Rome are separated from the unhealthy by a wonderfully short distance. A similar circumstance is observed in Cadiz, and in many American states; in the latter, however, the reason is perfectly evident—some streets are exceedingly low, built near parts where there is a quantity of stagnant water.

Reason why low situations suffer most from Malaria.—It is in consequence either of the weight of these exhalations, or in consequence of their uniting with moisture and producing their effect when the moisture is deposited—and moisture is usually deposited on the ground—that the lowest spots are generally the most dangerous; and this also accounts for these exhalations being most dangerous at night. It may be laid down as a general rule, that malaria is most dangerous in a low situation and in the night. It is on ground and near the lowest parts of a house that moisture is deposited; and that being the case, the exhalations of malaria will be let loose there, and what is lowest will suffer most. Again, in the night the cold is greater than in the day; and from the cold being greater, moisture is deposited, and with the moisture malaria. Hence we have a sufficient explanation in general for the two facts I have just stated.

I may mention, that the power of moisture to dissolve these exhalations is rendered exceedingly probable, from the stench that arises from pools and privies in damp weather. Every thing that affords an offensive smell becomes worse in wet weather, and therefore the exhalations that I am now speaking of will also probably rise far more in wet than in dry weather. The effect of moisture is sufficiently seen in the increase of illness. I could give you plenty of instances of the difference in respect to elevation in causing a liability to ague. In the Spanish town barracks in Jamaica, there were three cases of fever in the lower story for one in the upper. Sir Gilbert Blane says that he was informed by a medical man, that in 1781, at St. Lucia, one regiment, situated on the top of a hill, lost 271 men from intermittent and remittent fever; another regiment, on the sides of the hill, lost 318; and a third, which stayed at the bottom of the hill, lost 486. The hill was called *Morne Fortuné*, and situated 872 feet above the level of the sea. At Walcheren the natives knew that those who slept in the upper stories were far less liable to disease than those in the lower; and if they caught ague it was much milder. Sir Gilbert Blane also mentions that he was informed by Dr. Ferguson, that at St. Domingo two-thirds more were taken ill on the ground than on the upper floor. In Italy the difference is so great, that an erect is known to be more safe than a sitting or a lying posture. Although, however, this is a general rule, yet there are exceptions to it. In some parts of Norfolk, danger is confined to the upper stories; but there, there must be a particular current which wafts the malaria in that direction. The great danger of night is always strikingly illustrated. Dr. Lind says, that 280 men of the *Phoenix* ship, in 1766, who went on shore at St. Thomas's suffered, and all but three died. The ship's company went on shore in parties of twenty or thirty, rambling about the island, but none of these, who did not remain at night, suffered. Dr. Lind mentions that, at Batavia, a boat belonging to the Medway was actually manned three times, every one having perished, and simply from the men having to attend duty on shore every night. It is found that a cold night after a hot day is much the worst. During the heat of the day there is more disengagement of malaria, and consequently when night comes on there is far more to be precipitated. Cold and wet are always most operative after heat. It is thought by some, in Italy, that evening is more dangerous than night, and that there is no hazard after nine or ten o'clock at night. If this be a fact, I presume it arises from the whole quantity of malaria being by that time deposited. The effect of night, however, must be ascribed in some measure to mere sleep. It is right to take every thing into consideration, and all

morbid causes act more powerfully in sleep than in the waking state. Sleeping on damp ground at night is injurious, not merely because the person has lain upon the ground, nor simply because he has lain there during the night, but because he has lain on the lowest spot when there is more malaria than in the day-time, and when the body is less capable of resisting it than in the day. Lind and Mungo Park were convinced that malaria was precipitated with the rain; and in this way they noticed frequently that rain was unhealthy. Park says, that the rain had not begun on one occasion more than three minutes before many soldiers fell asleep, and seemed drunk, while others vomited; and he mentions that he himself felt disposed to sleep in a storm, and could not help it, although he knew that he was on damp, and therefore dangerous ground. Dr. Lind says, that the most unhealthy rains in Guinea are those which occur first in the wet season (this looks as if malaria were precipitated in the rain); that in forty-eight hours the rain rotted their shoes; and when the clothes they had on were hung up to dry in the sun, they became full of maggots in a few hours, shewing, perhaps, that much injurious matter was contained in the moisture, and deposited with the rain. As the rain falls more and more, there is generally less and less unhealthiness: this may be ascribed to two circumstances—in the first place, to the quantity of malaria deposited by the first rain; and, secondly, to the rain falling upon the earth and producing so much moisture that vegetable decomposition will not so easily go on. Hence, again, disease increases when the moisture declines.

Salt Water productive of Malaria.—It has been thought by some that a mixture of salt water with fresh prevents this injurious decomposition; however, this is a mistake; for those salt marshes principally situated in the south will, in the summer, give rise to ague. For instance, around the Dol, in Normandy, where there is a salt marsh, scarcely an inhabitant escapes ague. Very often there is a severe intermittent fever in Holland after an inundation of the sea; and even when a place is well washed, as Herne Bay and around Reculvers, aguish fevers frequently prevail. On the other hand, it is contended that a certain portion of sea water augments the prevalence of this miasmata. This would be not at all to be wondered at, because it is a mere hypothesis that salt water prevents it. Sea weed, being vegetable matter, has actually been the source of fevers of this description.

Influence of certain Plants and Seasons in producing Malaria.—It is very probable that certain plants, and certain soils, are more favourable to the production of malaria than others; for sometimes there is malaria in spots where you would not suspect it, where the grass looks fresh and green, and you ob-

serve no signs of decomposition; whereas, in other parts, where there is a degree of decomposition, persons are perfectly safe. A friend of mine, who was travelling in Syria, saw a spot of grass fresh and green, though rather moist, and he there intended to pitch his tent for the night; but he was warned by some of the people of the country, who rode in haste to him, and implored him, as he valued his life and the lives of his retinue, not to pitch his tent there. He could not conceive why, and they told him that whoever remained on that spot at night was sure to be seized with severe fever. There was nothing in the appearance of the grass that could lead him to suspect any thing of the kind; but, of course, the lower portions were constantly dying and decomposing. It is, as I have just said, the opinion of many, that certain soils and certain varieties of vegetable matter are peculiarly favourable to the production of this malaria; but whether this is to be attributed to the nature of the soil, or to the existence of certain plants rather than others, I do not know; but such would appear to be the fact, and it is very possible. Some believe that the different kinds of ague arise from a difference in the vegetable matter. It is said in Germany, for instance, that tertians particularly prevail; that in Italy quotidians are most frequently seen; that in Hungary, fevers of this description are particularly attended with petechiæ; the exhalations of the Pontine marshes are said to produce fevers with short intermissions; Holland is remarkable for producing all sorts of fever—quotidians, tertians, quartans, and every thing else. Spain, Africa, and America, are remarkable for having these fevers, attended with black vomit, and with yellowness of the skin. In some parts of Italy, these fevers are particularly attended by apoplexy, and in many parts they are particularly followed or accompanied by neuralgia;—in India and Africa the liver chiefly suffers; at Walcheren the spleen. Even parts not distant from each other are sometimes thus distinguished. It thus appears that there are many peculiarities, according to situation; but whether this is to be explained by a variety in the malaria, I do not know.

Peat Bog.—There is one description of bog which does not produce malaria, namely, peat bog; but then there is no putrefaction; decomposition has taken place in a peculiar manner. The vegetable matter becomes carbonized, and there is not sufficient heat for putrefaction to occur; but it is said that when peat moss is in a certain latitude, and on a certain level, it can putrefy, and then, I believe, ague does prevail. That peat bog does not putrefy, is shewn by a fact stated on good authority, viz. that animal matter thrown into it will not putrefy; but it is said that where peat moss is placed in other situations where it is warmer, and

upon a proper level for moisture, it will putrefy and produce ague just as other kinds of vegetable matter will do.

Animal matter not productive of Ague.—Now that it is vegetable, and not animal matter, which produces ague, is proved by this circumstance, that no person has that affection from being exposed to the most intense animal exhalations. Thousands of carcasses are annually employed in many manufactories, as, for example, sal ammoniac manufactories, and yet no person is known to contract an ague from them; indeed, so far from it, that the most crowded places generally escape intermittent fevers. That part of Rome inhabited by the Jews, and called the *Judaicum*, is full of animal filth, but it escapes ague, while the elegant streets in the neighbourhood suffer from it very severely.

Fire and smoke, especially tobacco-smoke, appears to keep away ague. Malaria (not being the produce of dead animal matter, or effluvia arising from living bodies, but of vegetable exhalations, the more men and animals are crowded together, the less access is there for the malaria; and, indeed, it is generally supposed that the addition of smoke of every description has a great tendency to prevent it, and therefore those who live in the most smoky, crowded, dirty places, are situated in the most favourable manner with respect to ague. It was, however, once imagined that the smoke of coal was particularly injurious, and therefore it was made a capital offence to burn coal in this city, and forges were only allowed in the vicinity. Sir Gilbert Blane mentions, that in the Tower there is a document of a person who was executed in the time of Edward I. for burning coal in London; but it has since been fancied to be a great promoter of health. Napoleon, when he was with his army in Italy, was in the habit of employing fires to a great extent, to keep away malaria, and he did it with great success. But the heat must have been useful, by dissipating moisture and cold.

This curious, but interesting subject, not being finished, I shall resume it on Monday.

MEDICAL EVIDENCE

ON A

TRIAL FOR POISONING WITH ARSENIC;

From a Lecture on Medical Jurisprudence,

BY PROFESSOR AMOS.

THIS being the opening lecture of the legal part of the course, Mr. Amos thought it his duty to make a few introductory remarks on the nature of the instruction which he was about to afford. It was, he said, peculiar to the course of lectures on Medical Jurisprudence, delivered in the London University,

that a portion of them should be given by the professor of law. "I believe," continued the professor, "that lectures of this description are not given elsewhere, and have never been given before, by a practising barrister. It is, I conceive, a part of what you want to learn, to know the nature of those judicial inquiries with reference to which information may, in the course of your professional practice, be required from you. And who but a barrister can give you an adequate idea of those inquiries? Again, who else is competent to inform you properly respecting the powers and duties of the various judicial tribunals before which you may, in the exercise of your professional practice, be summoned? And lastly, but what is most pointedly interesting to yourselves, who but a practising barrister can competently put you in the very situation in which you will stand when you are called to take an important part in judicial discussions;—can tell you the kind of information you will be expected to supply, and the mode in which it will be elicited; can warn you, by living examples, of the faults and inadvertencies which may be injurious to your professional reputation, or, at the least, may lead to animadversions very unpleasant to your feelings?—But, not to waste time by expatiating on the utility of the information I have to communicate, instead of giving you that information and leaving you to judge for yourselves, I will take an example of a trial for murder by poisoning—not the most interesting I could select, by any means, but perhaps the most recent which has occurred: it took place at the last assizes for the county of Warwick.

Mary Anne Higgins and Edward Clarke were indicted for the murder of Mary Anne Higgins's uncle.

The woman's guilt was clearly proved, from her own confession, from her having bought arsenic, from its being clearly proved that her uncle died from the effects of arsenic, and a basin of soup was found with arsenic in it, of which she acknowledged that her uncle had drunk, and from a variety of other circumstances—as, amongst others, she said that she bought the arsenic to poison mice, and she pointed out a dead mouse, which she said had been killed by the poison, whereas it appeared upon examination that the mouse had not died from arsenic.

The woman, by her confession, satisfied every body not only of her own guilt, but also of Clarke's. Her confession, however, was not legal evidence against Clarke; she was to have been married to him. This man had been for some time past squandering the uncle's money, which he made the woman get from him, or steal, threatening not to marry her, if she did not.

Higgins gave the alarm that her uncle was dying about one o'clock in the morning, by calling up a neighbour, an indifferent

character, Ellen Green. Ellen Green deposed that she found the deceased dead. After seeing the deceased, she came down to the kitchen with the woman prisoner, to warm some water: when they were warming the water, the woman prisoner said—"O! I heard my uncle give a great groan, I think."—They went up stairs on this, and Ellen Green shook the deceased, but there was no appearance of life. While she was doing this, she heard a chair move in the room below, and afterwards, when she left the house, she found the street-door open, which she had shut on entering. This afforded evidence that some other person had been in the house, and the pretence of the groan was to enable this person to get away.

Another witness passed by the house a minute before Ellen Green came: this person heard a whispering in the house between two persons.

At nine o'clock the preceding night, a witness saw the deceased in the passage [*Here a small model of the house was exhibited.*] by which he used to go to the privy, and the man prisoner was watching him. The deceased appeared ill, and was leaning against the wall. As the deceased was returning up the passage, prisoner kept still watching, but screening his own body, and when the deceased came near the entry, the man prisoner retreated into the house.

At between seven and eight in the morning the man prisoner came to the manufactory where he used to work, and told the people there of the uncle's death. It was remarked that he gave very contradictory accounts of the circumstances—that he had left the deceased well the night before—again, that the deceased had been ill. He said that he had been with him till 10, and that he had had soup for supper.

The poison was purchased about one o'clock on the same day upon which the murder was committed; and the male and female prisoners were seen walking together in a direction towards the house of the deceased within a quarter of an hour after the poison was purchased.

The judge and jury thought the case suspicious, but that there was not enough proof to warrant a conviction against the male prisoner.

Having now given you the substance of the evidence on this trial—a trial which lasted upwards of twelve hours—I shall mention one or two points of the cross-examination of the witnesses, in order that, if you are placed in a similar situation, you may know what sort of questions are likely to be put to you.

The first question asked of Mr. Hume, a surgeon, was—Have you ever (independently of this case) examined the body of a person who met his death by arsenic? "Never." Now this may serve to shew you the importance of seizing opportunities of observing and

and taking accurate note of, with a view to forensic inquiries, the postmortem appearances of persons who have met with violent deaths. From the circumstance of Mr. Hume not having examined the body of any person who had died of arsenic, the effect of his evidence was somewhat impaired, although his evidence in general was characterised by great intelligence.

The next question was, whether witness would undertake to say that the stomach was inflamed. He answered that this was difficult to tell: its appearance was *highly vascular*, but he would not swear that the redness was *inflammation*. Here then is a new point of inquiry on such occasions; the speaking to the fact of inflammation as produced by taking arsenic into the stomach.

The next questions consisted of the suggestion of various natural causes which might have produced inflammation—*e.g.* if a labourer when he is hot were to drink cold water, would it not produce a redness? *Ans.* It might produce a redness. Hence you may perceive the importance not only of being able to speak to the effect of arsenic in producing inflammation, but of various other causes of inflammation, which the ingenuity of counsel, prompted by a medical assistant, may suggest.

One of the witnesses having spoken to some vomit which was found on the floor of the room where the deceased expired, and another, that the deceased was subject to the "*Water swamp*," Mr. Hume was asked very particularly as to the complaint known among poor people by the name of the "*water swamp*," and whether the vomit might not be attributed to an attack of this complaint. The vomit would have perhaps been explained away in this manner, had not Mr. Hume been positive with regard to the complaint in question, which he explained to be an affection of the stomach causing pain, and after some time a quantity of the gastric juice to be thrown up; it was (he said) very rare that the contents of the stomach came up with the juice, which generally came up perfectly clear:—generally from one to three table spoonfuls: and the witness said that he never heard of a person dying of this disorder:—that it was caused simply by an unhealthy and unnatural secretion of the gastric fluid.

Mr. Barton, a surgeon, describing the appearances after death, said, that the intestines were inflamed and gorged with blood; that the effect appeared to him to be produced by erosion or abrasion; by abrasion produced by some corrosive stimulant; that arsenic could produce those effects,—that the bowels were healthy in other respects. Mr. Barton also said that the appearance of the stomach was vascular. On cross-examination, he was much pressed for a definition of the word "*vascular*." He said, "*fleshy and red*." He was asked "*if every body's stomach was not fleshy*." He then said, that

in using the term "*vascular*" he meant an unusually red appearance: and that the vessels were distended more than naturally with blood. The former witness having said that the villous coat was not destroyed, and this being thought favourable to the prisoner, Mr. Barton was asked if the villous coat was not uninjured? The answer shews to the *legal* practitioner, that when he has once got a fact in his favour, he had best be content; and to the *medical* practitioner, the importance of minute observation upon all the appearances which may tend to throw light on the cause of death. Mr. Barton answered, that the villous coat was in part destroyed, abraded; there were points in which there was a loss of substance—some parts were not in continuance with others.

The next inquiry relates to the search after the poison. This, being a work of time, is frequently made at a different place from the house of the deceased; or, at all events, is frequently not made immediately. And here a wide field for the cross-examination of counsel is opened, as to the *custody* of the suspected matter from the moment it is discovered until the time when experiments are made upon it. The identity of the article experimented upon, and of the article found, must be clearly established, and therefore must be particularly attended to by the medical practitioner. In the present case, no doubt of the identity of the suspected liquid was left in any person's mind. But it is very necessary for a medical man to have it impressed upon him, that precautions of the nature I have been alluding to are essential with a view to forensic inquiries. And I would from experience, recommend a medical man to take a note at the time, of the precautions he has adopted; for in a scene where all is alarm, horror, and confusion, the parties often do not recollect what in fact has been done, and sometimes have an erroneous impression of what has been done. A remarkable example of this occurred at the very trial of which we have been speaking; a witness, Minstrel, said that he took the bottles, and Mr. Hume went along with him; that they went along a street together; and that when they came to a turning which led to Mr. Hume's surgery, he went on to the surgery, and Mr. Hume turned another way, and that when he came to the surgery, he (Minstrel) gave the bottles to the woman who opened the door. The woman not being called as a witness, for a moment some alarm existed amongst the counsel for the prosecution, lest the identity should be lost. But every body in court was immediately satisfied that this witness, apparently without any bad intention, was under a mistake, when Mr. Hume stated that he and Mr. Barton followed close on the witness, till they came to the turning of which the witness spoke, and then Mr. Barton and not he (Mr. Hume) turned the way described by

the witness, but that he followed the witness to the surgery, and when the door was opened, took the bottles out of the witness's hand himself.

The evidence to prove that the poison used was arsenic, was next adverted to by Mr. Amos.

Mr. Hume, the first surgeon examined, said, that the first test he tried was the nitrate of silver. He was asked if he did not know that this test was very fallacious. He said that it was allowed to be somewhat fallacious;—that it was not a test upon which he would depend entirely;—but that there were no other substances, which to his knowledge, would produce the same coloured precipitate. He was then asked whether phosphate of soda, or garlic, would not produce the same colour. No; he said they would produce a white precipitate, not a yellow. He was asked, if the two combined would not produce a yellow precipitate. He said, yes, but not the same bright lemon yellow that arsenic produces. He admitted that eminent men had been mistaken in applying this test. The nitrate of silver test was applied by Mr. Hume at his own surgery. Mr. Hume afterwards carried the suspected matter to Coleman's, a druggist, where the sulphate of copper test was applied. Before applying the sulphate of copper, a portion of carbonate of ammonia was mixed with a portion of the suspected fluid. On adding a solution of sulphate of copper, a grass green precipitate was yielded. Mr. Hume afterwards succeeded in metallizing a portion of the contents of the stomach. The metal was arsenic. No other metal volatilizes at the same degree of temperature. Antimony approaches to arsenic in this respect; but it will not volatilize at the same degree of temperature. It requires a higher degree of temperature. A spirit lamp will not volatilize antimony.

Mr. Hume admitted that he had never tried to volatilize antimony. On his re-examination he said that he had studied chemistry under Dr. Hope, of Edinburgh: that although the nitrate of silver or sulphate of copper might be fallacious, yet that the concurrence of both might be safely relied upon, and that the reproduction of the metal removed all doubt: that although the tests of arsenic had been mistaken by eminent men, yet that of late years the subject was better understood, especially since the publication of professor Christison's book.

Mr. Barton, the next surgeon, said that he did not profess to be a chemist; that all he knew upon the subject was derived from books.

Coleman, the druggist, was then called. He spoke of having filtered the fluid brought by Mr. Hume. That experiments were made by means of ammoniacal sulphate of copper, nitrate of silver, ammoniacal nitrate of silver, sulphuretted hydrogen gas;—and that the re-

sults were all the same;—they all denoted the presence of arsenic. That there was enough arsenic detected to have produced death. On being cross-examined, he said, that, besides being a druggist, he sold soap and candles,—that he had made many experiments. He was asked how many he would swear to; he said two. He was asked when he made them; he said in the last year. On being re-examined, he said that he had been brought up an apprentice to a chemist and druggist.

Dr. Arrowsmith was next examined. He said that he had made poisons the subject of study. That he saw three liquid tests applied, and performed himself the test, by reduction. Those tests had been all said to be fallacious, except the test by reduction; but in his judgment where the indications exhibited by the three tests agreed, no chemist could doubt of their conclusiveness, because the fallacies were different. He obtained crystals of white arsenic.

Now, gentlemen, here we are supplied with some hints which it may be useful to attend to. In the first place, you will contribute more effectually to the purposes of judicial inquiry, and appear to greater advantage in the witness-box, if you are able to state that you have on several occasions made experiments of all the tests of the poison which it is suspected has been administered to the deceased. But this is not all; you should be acquainted with the nature of the fallacies which have been supposed to be connected with those tests; for, in cross-examination, you will be sure to have it put to you—would not such and such a substance produce a similar result? and it would not be a very creditable answer to be obliged to say, "I don't know; I never tried." You observe Mr. Hume says it was not garlic or phosphate of soda—they would separately yield a white precipitate, and jointly they would not produce the same bright lemon colour. His evidence is not quite so satisfactory as it might have been in regard to the antimony, inasmuch as he had never tried to volatilize it; but he shews himself well acquainted with the result as a matter of scientific information. Again, you will observe that a medical man cannot get out of the witness-box after merely stating his opinion; his competency to form such opinion, and the grounds of it, will all be strictly scrutinized. Is your opinion grounded on books, or experiments you have yourself made? If the latter, how many have you made; when and where; what has been your medical education? how long have you practised? Again, you are liable to be asked, not only your own opinion, but also as to opinions expressed in medical works of reputation.

Lastly, gentlemen, you will collect that on the occasion of being called in where a person has died of poison, it is of the utmost

importance to husband the suspected matter; for it will not be satisfactory, unless several tests have been applied, and it will be all-ways better that the experiments should be made by more than one person, and it may sometimes happen that experiments may occur to you or to other medical men before the trial, which there is no opportunity of making, because all the suspected matter is expended. In the trial of Donnal, at Exeter, for poisoning with arsenic, the prisoner escaped, principally on the ground that the most material test, that of reduction, had not been applied, all the suspected matter having been spent in applying the other tests. In conclusion, the learned professor briefly noticed a case that occurred on the western circuit not long ago, in which the poison could not be traced in the stomach of the deceased: it was administered in milk, which was afterwards thrown out of the window, and some poultry that drank of it were killed. In such a case, Mr. A. remarked, it will most probably be required of the medical man, to shew, that he exhausted every means of tracing the poison, and he should be able to give satisfactory reasons how it may have escaped detection.

ON THE
USE OF ALKALINE SALTS.

To the Editor of the London Medical Gazette.

SIR,

I now advance to the more practical part of my subject; and I beg, ere the reader peruses this *second* portion of my paper, to direct his attention to that published in the Gazette of the 18th ult. p. 761-2, in which I cursorily developed my opinions as to the cause and seat of various most serious diseases incident to man: keeping in remembrance what I have there said, it cannot be matter of surprise if I regard the dissections of the ganglionic nerves in cholera asphyxia, by M. Delpech, reported in last week's Gazette, as happily timed, and strikingly corroborative of the accuracy of those views; and does not the remarkably successful treatment of Dr. Hope, of the *supposed* cases of cholera* in the Marylebone Infirmary, prove, as far as practice can be expected

to prove, the accuracy of M. Delpech's statement?

With respect to the doctrines that assign inflammation of the brain as the cause of typhus fever, I have briefly to observe that the evidence of such affection is very far from being always present: a late author tells us, that of 54 patients who had died of this fever, and whose heads were examined, only 37 shewed marks of cerebral inflammation having existed. So, of the doctrines that assign the cause of fever to inflammatory affections of the intestinal mucous membrane, of sixty patients who had died and were examined, only twenty-four had been afflicted with inflammation of the intestinal mucous coat, of whom fifteen had ulceration of the small intestines, and ten ulceration of the ilium and small intestines. Morbid anatomy, therefore, proves that neither inflammation of the brain nor of the intestinal mucous coat, are essential to fever. I question not that the brain is very frequently found to have suffered from inflammatory action in typhus; nay, I know it to be a fact;—and as little also do I question the existence of inflammation and ulceration of the intestinal mucous membrane, for this also I know to happen; but I do deny that either the one or the other can be regarded otherwise than as accidental; and were it a cause of the disease, it is incumbent on the advocates of this doctrine to shew why evidence of its existence is not always present—always to be found—since, according to their doctrines, the affection has been so severe as to cause death. If death be the result of phrenitis, pneumonia, gastritis, &c. &c. abundant evidence of the disease is always found; were it, therefore, essential that inflammation of the brain, or inflammation of the intestinal mucous membrane, should be the cause of fever, the evidence of this having existed would as surely be found as the evidence of the inflammatory actions in any other parts producing death; but if, on the contrary, these appearances are not found in one half of those who die, am I not entitled to assume that *gastric irritation*, which is always present in fever, is the cause of the phenomena in that disease—the accessions and remissions being independent, and referrible to the laws that govern our system in health as well as disease? and am I not equally borne out by the facts

* The title is our's, not Dr. Hope's: when gentlemen (as frequently happens) send us papers without any title, we prefix such an one as appears to us to be apposite.—E. G.

to assume that this cause is the effect of an acid of a peculiarly morbid kind, which is lodged in the stomach and intestinal canal—a cause fully equal to all the effects found either in the intestines or the brain, when the patient has been suffered to remain for weeks a martyr to its acrimony? It may be objected to this theory, that if a morbid acid in the stomach causes fever, how is it that bleeding in the arm relieves the patient, and generally is of great service in the early stages of typhus? To which I answer, that a morbid acid in the stomach has a direct tendency to cause blood to rush to the head, as will be abundantly manifested by the cases which follow, and that bleeding, by relieving this effect, gives the brain a temporary respite; the heart also may be placed in the same predicament, from the same cause. Thus we see, in *cholera asphyxia*, prostration of strength is extreme, and one of the most frightful effects is the communication of this loss of power to the heart. This explains the use of bleeding at this juncture; and do not these views satisfactorily explain how it is that this fatal disease is not attended with delirium, and that the brain remains so little affected? is not this to be ascribed to the sudden incapacity of the heart to force an undue quantity of blood into the brain, which is the reverse of what takes place in the first stage of typhus? The intellect, in cholera, remains peculiarly unclouded, whilst there is extreme prostration of strength; but if the patient survives this state, a severe typhus fever is generally the result—that is, if the nerves of the patient recover sufficient energy to be acted on by the morbid stimulus.

Not long since I took the liberty to call the attention of the medical profession to the use of alkaline salts in the cure of typhus fever—more particularly the carbonates of soda and potassa. It appears, that, two or three years after I had adopted this practice, Dr. Stevens, then practising in the West India islands, commenced the use of the muriate of soda in the yellow fever; and it seems that the Doctor claims to himself great merit and originality in this application of the neutral salts. However this may be, I was in the habit of using the carbonates of soda and potassa some years before Dr. Stevens thought of using the muriate of

soda; indeed, in 1827 I published a letter in one of the periodicals respecting the treatment of erysipelas, and mentioned a very bad case which was accompanied with typhus fever; wherein I gave the following formula, among others, as the purgative medicine I used:—

R Sodæ Carbonat. ʒj. ad ʒij.
Magnes. Sulph. ʒj.
Aquæ Menth. P. Oss. M. ft. mistura,
Capiat cochl. ampl. ii. 4tā q. q. horā.

Here, I may remark, is both an alkaline and a neutral salt. Now one curious coincidence which we learn from Dr. Stevens's Letter, on the action of the neutral salts on the blood, published in the Medical Gazette for January 14, 1832, is, that it was only in the year 1827, after a long residence within the tropics, that the Doctor 'commenced a new practice, on a new principle;' and not till 1828 did he go to Trinidad, to teach his practice there. It is another curious coincidence, too, that in the controversy, of which this letter I quote from is a part, much is said about the use of soda, &c. Now, coupling these circumstances together, an idea has crossed my mind that Dr. Stevens *may* have borrowed from me the practice he has adopted; for, in the quotation I have made from his letter of the 14th Jan. he does not say he had *discovered* a new practice (founded) on a new principle, but simply that he had "commenced a new practice on a new principle." The formula I gave, and which was published in 1827, contained both a neutral salt of soda and an alkaline salt of soda; and I stated that I had used it in a fever of a typhoid type, and one nearly as difficult to manage, too, as West India fevers of the typhoid type. Whether the muriate of soda be applicable to typhus fever in these climates I am not prepared to say, my success with the carbonate being too decided to allow me to deviate into experiment in so formidable a disease, unless I were warranted by previous success in minor cases, bearing some analogy.

It appears that both the carbonate and the muriate of soda possess alike the power of turning black blood to red; but this is to me a matter of indifference, because my practice is founded on an experience that has many years since assured me it was well founded.

In my work on *Insanity*, published six years ago, at page 58, I have mentioned that I had used the carbonate of soda as an alterative in the cure of the insane. At that time I had been several years watching its effects, and had reason to believe that it often exercised a peculiarly sedative power, by allaying the irritability of the gastric nerves; by which irritability the nervous twigs distributed to the carotids, amongst others, were, I conceived, excited to an undue action, and thus produced that violent throbbing of those arteries which is so common in insane persons. I allude to this notion in that work, but I was not so clear of the fact as to be induced to avow my opinions unequivocally, which I now do: the impression on my mind was, however, too strong to be forgotten, and appeared too important to be neglected; and in 1824, when I recommenced general practice in Derbyshire, I proceeded to ascertain its effects, as well as that of the *carbonate of potassa*, on headaches generally, as they occur in the sane. The results emboldened me to try it in states of mental irritation; and in this class of affections the soothing and curative powers of the medicine were equally gratifying; lastly, I ventured to try it in *typhus fever*, and the same results crowned my labours with success.

Now it will be seen by some cases which I shall notice of Indian cholera, or cholera asphyxia, that the practice which I ventured to recommend in this disease (reasoning from analogy, for I have never seen the malady) has been very successfully followed by various practitioners in distant places, and, in some instances, to the very letter of my suggestion. But first for my own cases.

No. I.—In my book of daily practice for the year 1825, I find accounts of three patients who had fevers of the typhoid type, and to whom I gave the carbonate of soda, and all of whom recovered; one a delicate servant maid, whose affection also partook of the character of phrenitis from a wound in the head that had some years before injured the brain; the other a lady, whose fever was also modified by an abscess in the neck; and the third was a farmer, whose case was unmixed.

1825, March 30th.—I was requested to visit Mr. John Bower, a hale old man, aged about 65 or 70, who resided at Whitfield, near Glossop, in Derby-

shire. I found him lying on his back; countenance much sunk; eyes dull and inexpressive; pulse quick and small; tongue rather moist and furred; teeth dry and dirty, from fever; temperature of the body low; great prostration of strength; much uneasiness on pressing the stomach; intellect confused. His bed was surrounded by many relatives, some of whom had turned their backs upon the patient to hide their tears, believing, as they afterwards insisted, that he would never recover.

R Submur. Hydrarg. gr. vij. statim sum-
mend. Appl. Empl. Lyttæ Scrobic.
cordis.

R Sodæ Carbonatis, 3j.

Sp. Æther. Sulph. C. 5iss.

Liq. Ammon. Acet. ʒij.

Aq. Menth. Pip. ʒviij. M. ft. Mist.
cujus capiat cochl. ampl. ij. 4tâ quâ-
que horâ.

31st March.—Felt himself very much better; had no pain; intellect clear.

Cont. Mist. Alk.

April 1st.—Is up, and down stairs; perfectly convalescent.

A little before this time, I had five other typhus cases in one family, in the same village; but as the first case was far advanced before I was called in, and proved fatal, and as the other cases promised to be of the most formidable kind, I shrunk from the responsibility of using the alkaline salts in their cases. Of these five cases, two died—the first attacked, being a child about two years old, and the father, who obstinately refused medicine.

No. II.—On the 21st April, 1829, I was called to see — Scholes' son, a boy who had worked in a cotton factory for some years, and who partook of the lax fibre of that unfortunate race of boys. He was twelve years old, had been ill eight days; he had been under the care of an apothecary, who had applied eight leeches to the temples, had kept his bowels open, and given him neutral saline medicines, but no alkaline saline. I found him lying on his back, and so weak that he could not raise himself in bed, though evidently roused to attempt it by some delirious vision. He was perfectly delirious; pulse 108, and thready; lips dry; teeth covered with sordes; tongue parched; eyes languid and dull, and inexpressive; countenance sunken; bowels relaxed; thirst troublesome, which was attempted

to be allayed by oranges, lemonade, &c. Ordered to take no other beverage than spring-water, toast and water, or common tea.

R Sodæ Carbonat. ʒj.
Sp. Æth. Nitrici, ʒij.
Aq. Ment. P. ʒiv. M. ft. mistura.
Capt. cochl. ampl. j. 4tâ quâque horâ.

23d.—Countenance intelligent, and he is perfectly collected. He complains of severe pain beneath the right scapula, and from the right elbow to the wrist; of this he had before made some mention on the 21st, on being repeatedly questioned (for my experience with the insane has taught me that valuable information may sometimes be extracted even from the very delirious); but yet, in this case, it was too vague to act on it; his bowels continue relaxed; he has taken some gruel and tea, with apparent enjoyment; tongue clean, pulse 120.

Cont. Mistur. Alkalin.
Applicetur parti affectæ Empl. Lyttæ amplum.

25th.—So much improved, that his friends seemed to think him well; they wished, in short, to load his stomach with solid food and stimulating beverages, which, of course, I peremptorily forbade. Pulse still 120.

26th, 27th, 28th, 29th. — Tongue clean. I now put him on a course of tonics. A relaxed and irritable state of the bowels, that continued up to the 28th or 29th, has been relieved by Pulv. Ipecac. Comp. and Pulv. Rhæi.; and on the 3d of May I left him well, though extremely weak. It will give, perhaps, a more correct idea of the tendency of the fever I have just related, if I notice another case that I was called to on the 27th of the same month, during my attendance on Scholes, and about half a mile distant. The boys had been play-fellows, and this latter, whose name I have forgotten, had been attacked precisely in the same manner. I saw this youth not until the twelfth or thirteenth day after the attack. His case terminated fatally.

No. III.—I was requested to see Harroff's son the 5th September, 1830. He had been suffering from severe typhus above three weeks, when the apothecary, who had attended him as well as the other boys, refused to continue his services. As the poor lad did not die, I was called in, two or

three days after this, which was about the twenty-sixth day of the disease. I found him in a state of the utmost exhaustion, scarcely having the power to pick his lips, which he seemed to attempt whilst I was examining him. He was supported by pillows on his left side, in consequence of numerous and large ulcers on his back; large drops of perspiration were standing on his face, and a clammy moisture was felt on his legs; his cheeks had a cadaverous appearance, with a purple hue; his lips were parched, and covered with a thick yellow crust; his teeth were loaded with sordes, his tongue was loaded with a thick, dry, yellow coat, and he had not the power to thrust it out. The blood-vessels of the tunica adnata were turgid; great insensibility both to sound and light; he seemed not to hear any thing that was passing about him, and at noon he thought it was "dark night." Had been much troubled with diarrhœa and very offensive stools, and he had been nearly constantly delirious, but by some exertion I got him to comprehend my questions. His pulse near 112, very compressible, and rather soft. He had been taking a solution of Nitras Potassæ, and Mistur. Cretacæ cum Tinc. Kino, and he had been allowed as much milk and butter-milk as he chose to take. I ordered him to have no milk, nor any acids in any form, but to take cold water, toast and water, or tea, or gruel, at his option.

R Carbonat. Sodæ, ʒij.
Liq. Ammon. Acet. ʒj.
Aq. Ment. Pip. ʒviij. M. ft. Mistur.
capiat cochl. ampl. j. om. horâ.
R Pulv. Ipecac. C. gr. ij. ft. pulv. post singulas sedes liquidas capiend.

7th. — Countenance natural; pulse 120; tongue nearly clean; takes a little food, and seems to relish it.

Cont. Mistura.

8th.—Tongue clean; pulse 126; eyes clear; takes food with a relish; has had a healthy stool; gets some sound sleep, but his many sores disturb him.

R Infus. Cascariillæ, ʒviiss.
Tinc. Calumbæ, ʒss. M. ft. Mistur.
capiat cochl. ampl. j. 4tâ q. q. horâ.
To have an ounce of good port, or sherry, two or three times a-day, and beef-tea or mutton-broth, and a little broiled mutton, &c.

10th.—Tongue continues perfectly clean; pulse 110; appetite for food

stronger. Henceforward he gradually, though very slowly, regained his health.

No. IV.—I was called to Mrs. W.—on the 20th April, 1829. She is a thin, little, delicate woman, a mother, and aged about 34 years. She complained of great debility, severe headache, giddiness, so that she could scarcely move about the house, had had no sound sleep the preceding night, and when she slumbered she was harassed by frightful dreams. She has extreme thirst; her countenance indicates distress; eyes heavy and glistening; tongue furred, of a dirty smoky hue, moist, and trembles as she puts it out. Pulse 104; skin dry, but not hot; makes but little water.

Apply six leeches behind the ears, and encourage the bleeding with warm bran poultices.

R Pulv. Ipecac. ʒj.
Antim. Tart. gr. j. M. ft. Emet. quam-
primum sumend.

R Submur. Hydrarg. gr. vj. ft. Bol. h. s. S.

R Carbonatis Sodæ, ʒij.

Sp. Æther Nitric. ʒss.

Aq. Ment. Pip. ʒviiss. M. ft. Mist.
capiat coch. ij. 4tā quaque horā.

21st.—Headache much less; only feels giddy when standing upright; does not feel so weak; is not thirsty; her manner, from being oppressed and melancholy, is cheerful; eyes still heavy; tongue furred; pulse 80, and more free; but she had a restless night.

Cont. Mistura.

22d.—Countenance natural; had some sound sleep last night; feels no pain in the head, except some slight pain at the back part, more like a soreness left by the previous severe pain than a regular headache; feels giddy on sitting upright; tongue moist; pulse 80; no particular thirst.

Cont. Mistur. Alkal.

25th.—She is convalescent.

No. V.—Hobson, a carter, aged about 40. I was requested by his master to visit him. I found the man had been ill about a fortnight, during which he had been under the care of an apothecary, who had bled him freely and given him saline purgatives; and his master, who, my patient at the time, requested me to see him in consequence of his extreme debility and exhaustion, not having strength, though a powerful man, to put his arm out of bed, &c.

I found his skin hot and dry; his tongue furred and of a smoky hue in the centre, red at the edges, and moist; pulse 96, thready, and rather sharp; countenance haggard; eyes very dull; and it appeared to cause him considerable exertion to attend to my questions, which he seemed but imperfectly to comprehend, though I spoke slowly, and put my questions in different phrases. He evinced some uneasiness from pressure on the stomach; bowels open; had been, and continued, extremely restless, having scarcely slept for many days and nights. Had been frequently delirious, but not constantly so.

Treatment.—To take water and slops, as I have ordered in similar cases.

R Sodæ Carbon. ʒij.

Aq. Ment. Pip. ʒviij. M. ft. Mist.
capiat coch. ij. 4tā q. q. horā.

29th.—Pulse 60; tongue clean; skin cool, and all his manner evinces a mind and body perfectly at ease, though much exhausted. In reply to my question, he said, "I felt quiet and composed soon after I took the medicine, and I have had more sleep since, than I had during the whole time I have been ill."

Cont. Mistur. Alkal.

October 1st.—I found him up and sitting in a chair by the fire. He was very cheerful.

No. VI.—Mrs. Hobson, the wife of the last patient.

October 3d.—On calling to ask how her husband got on, my attention was directly called to the wife. She exhibits great depression of countenance, and despondency; eyes rather prominent and suffused; tongue furred, but moist; skin hot and dry; pulse 100, and rather small and short; complains of great debility, that she can scarcely move about the house; has severe headache, pains in her limbs and back, nausea, loss of appetite, diminution of the usual quantity of urine;—her bowels are confined, her nights very restless, and when she dozes she constantly dreams of flying or swimming. "I think, sir," she concluded, "that I am going to have my husband's disorder."

R Pulv. Ipecac. ʒj.

Ant. Tart. gr. j. M. ft. Pulv. Emet.
vespere sumend.

R Submur. Hydr. gr. iv.

Pulv. Jalap. gr. vj.

Syr. Rhamni, q. s. ft. Bol. horā somni
sumend.

R Sodæ Carbonat, ʒiij. ft. Pulv. In Chartas, vj. divide, sumat j. 4tâ quâque horâ.

October 4th, 5 p.m.—She met me at the door, as I was about to enter, with an animated countenance, and in answer to my question she replied, “Oh, you have cured me.” I, however, found her pulse about 80, though natural; tongue clean; skin cool. She reported that the evacuations had respectively operated effectually; that she had no headache, and no pain; and that she slept better, though she had dreamt considerably.

Cont. Pulv. Sodæ, ter in die.

6th.—She is quite well.

I could relate ten or a dozen more cases of typhus, similar to those I have detailed, that occurred in this valley by the brook side; but I think I may now fairly close this part of my paper, and I may just observe that I have not lost a single case of typhus when I have relied on the use of alkaline salts.

There are one or two more cases, however, which I shall abridge into the smallest space, to shew the effect of the alkaline salts in allaying gastric irritation, and thereby removing delirium and restoring tranquility to the mind, and repose to the body, in fever; and in removing the unhealthy nervous stimulus when it is exerted on the arteries going to the head, threatening apoplexy.

M. J., a stout healthy young woman, aged 23, was attacked with hysteritis on the 24th January, 1828. The inflammatory action was subdued, by copious general and local bleeding, in five days. After, however, great restlessness and occasional delirium came on, with much mental irritation, though naturally of a most mild and enduring spirit. She took a scruple of the carbonas potassæ every two hours. The first powder procured ease—she speedily fell asleep, “and has slept mostly since,”—that is, eighteen hours after the first dose.

February 6th, she was quite well.

Paul Jones, engineer, aged about 30, March 29, 1829, in a state of furious delirium, requiring two men to hold him. It was evident he had been drinking spirits, and his friends and relatives, who were present, and who had spent the day with him, told me he had taken some rum and water, as they all had, but none of them to any excess. They were perfectly asto-

nished at his present state, as the man did not shew any symptoms of disorder till some time after drinking the rum and water, and all the other persons present were perfectly sober.

R Sodæ Carb. ʒss.

Julep. Camphoræ, ʒij. M. ft. haust. statim sumend.

The effect was immediate; he had no sooner swallowed the medicine than he ceased from struggling, and very soon after he spoke rationally. I advised him to take the medicine for the next twelve hours, a dose every four hours. The next morning he went to his usual work, and was quite well.

A highly-respectable gentleman, whose wealth enabled him to command what was considered by the public the best advice, visited the metropolis, and, for the advantage of medical aid, took up his abode there. He consulted a wealthy and highly-popular practitioner, and was alarmed by the assurance that apoplexy hung over him, and that *cupping* was his only remedy. The poor gentleman soon knew the nearest way to half the coppers about town. As soon as he felt the dreaded giddiness coming on, he hurried in alarm to the nearest cupper, lost a few ounces of blood, and was relieved. This could not last long; he grew languid, extremely timid, and mentally depressed; he felt, he said, that this state of things must kill him. He fled to the country, and became my patient, in consequence of an accident. In my investigations as to the state of his general health, I learned this melancholy tale. I put him in possession of the powers of the carbon. sodæ, and I have never understood that he has either been cupped or even affected by giddiness since, so as to cause him any alarm.

Of the Cholera Asphyxia, called the Indian Cholera.

Having stated in “The Times” of the 21st June last, that I had repeatedly detected acid to be predominant in the cholera morbus of these climates, I proceeded to infer that a peculiar morbid acid is the cause of all the frightful tortures and deaths that so frequently and speedily overtake those who are attacked by this terrible disease; and I also concluded, that as I had found ʒj. doses of the carbonas sodæ, given as often as the sensations of the presence of acid were experienced—that is,

two or three times an hour, or once in two or three hours—relieve the pains, and promote faecal evacuations, I ventured to recommence a trial of this medicine in the cholera asphyxia; having, moreover, reasoned myself into the conviction, from the information I have been able to obtain from various sources, that the same virus which produces the cholera asphyxia is only an augmented power of that which produces the typhus fever*, the various phenomena depending, for their variety, on the mental and bodily state of the attacked; especially on the state of the first passages of the alimentary canal, the *intrinsic* energy of the nervous power, which differs immensely in different individuals, and is not so readily ascertained as many seem to imagine; so that some shall have only one or two of the symptoms of unusual *gastric irritation*, which disappear with a dose of rhubarb and magnesia, others (as a patient of my own the other day) shall have pains of the bowels and cramp of the limbs, with great debility, and so on in all the various grades, till we come to the unfortunate who seems as if suddenly poisoned, suddenly struck with the hand of death; besides, the fact that the disease, if it does not speedily destroy, generally assumes the form of typhus fever, is another strong ground to regard the cause of the two diseases as identical, and the diseases themselves as partaking of this identity. I believe I may claim to be the first to have called the attention of the public to the curative powers of the carbonate of soda in this class of diseases. Since then it has been put to the test; and of its efficacy we have some information in the following cases, extracted from the *Medical Gazette* of January 7, 1832†.

In conclusion I will observe, that the alkaline salts, being carbonates, appear to exert a very peculiar power in allaying gastric irritation; more so in some cases than opium, or sulphate of morphine, or effervescing draughts, or nitric acid;—cases of which I should have adduced, had not my paper already

trespassed too much on your very valuable journal. Again: except with emetics, there is scarcely any medicine that the alkaline salts may not be combined with. Of the mode of treating cholera asphyxia in its various stages, the reader need not be informed that it is no part of my design. I hope I have said enough to make it desirable that those gentlemen who have tried the alkaline salts in the cholera asphyxia, would be so good as to give the particulars to the public. Have any patients afflicted with cholera asphyxia died where the carbonas sodæ has had a fair trial?

I am, sir,
Your most obedient servant,
PAUL SLADE KNIGHT.

DR. HOPE ON THE PATHOLOGY OF CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

In compliance with the wishes and expectations of many of the profession, I last week forwarded you an account of the twenty-nine cases of mild cholera recently under my care in the St. Mary-le-Bone Infirmary. Though it was not my wish or intention to have appeared in your pages, or elsewhere, in connexion with the subject of the present epidemic, I feel it my duty to place at your disposal the following corollary of cases, because, taken in connexion with those of the children already described, they form, so far as my reading and observation enable me to judge, the most complete commentary on the pathology of cholera that can be desired.

CASE I.—A girl, ten years old, had violent vomiting and purging, with *all* the other symptoms of malignant cholera, in their most characteristic form, and died in twelve hours.

Secitio, six hours post-mortem.—*Exterior.*—Eyes deeply sunk, encircled by a livid areola; features shrunk, as from extreme emaciation; complexion dusky; *hands and feet* livid, palms shrivelled; fingers drawn in; *mammary veins* full, *lungs* universally of a rather deep violet colour, especially below, but without

* See Mr. Searle's remarks on the connexion between Cholera and Fever, and also Dr. Holbrook's, in the *Medical Gazette*, January 7, 1832; besides many others, in other parts of the same work.

† We must content ourselves with referring to the cases; it is unnecessary to reprint what we published so lately.—ED. GAZ.

cadaveric engorgement; *pleura* of same colour; *heart*, coronary veins much gorged, right cavities distended, with liquid black blood, left ventricle and aorta contained the same; *liver* more gorged than natural; *gall-bladder* not quite full, muddy green colour, thick black blood, like treacle, oozed from the vena portæ and cava; *small intestines*, externally of a pink hue, traversed, especially in the jejunum, by transverse rugous streaks, of a deep-red colour, corresponding with the tracts of the valvulæ conniventes; substance of the intestines thickened, peritoneal coat viscid and sticky*, surface flattened from loss of resilience; *stomach*, a thick layer of greyish slime overspread its surface, which was reddish and soft; contents were a greyish liquid, with slime. The first three inches of the duodenum were palish, but, from this point to the end of the rectum, the mucous membrane was of a deep pink hue, and turgid, from minute vascularity, the colour amounting, in the jejunum, to light red, and over the last six inches of the ileum to deep red. Throughout the whole canal the isolated glands were remarkably developed, being, in the duodenum and jejunum, larger than mustard seeds, and thickly disseminated, rather smaller and more scattered in the ileum, and in the great intestines large and numerous again. Here also some of the congregated glands (patches of Peyer) were enlarged, and prominent. The whole villous surface of the canal was overspread with a thick layer of tenacious, adherent, slimy mucus, tinged yellow, with bile over two inches of the duodenum. The contents of the duodenum were, a little fluid, like gruel; the same in the upper third of the jejunum: in the two lower thirds was a fluid resembling half-boiled white of egg, which became thinner, more abundant, and more opaque, on descending, and this continued throughout the ileum, still becoming thinner and more abundant. Near the colon it was as thin as rice-gruel, and contained a floating intermixture of small opaque flakes. The same fluid existed in abundance in the colon, except at the sigmoid flexure, the calibre of which was obliterated by

contraction; odour peculiarly sweet and oppressive; no fecal scent or matter; *bladder* empty, size of a small flattened fig: no appreciable alteration of the abdominal ganglia, or nerves.

Signed by four physicians, including M. Auzoux, and three surgeons.

CASE II.—A female æt. 50, in the Mary-le-Bone Infirmary, had violent vomiting and purging, with all the other characteristic symptoms of malignant cholera in an aggravated form, and died in 10 hours.

Section, nineteen hours after death.—

Exterior.—Features extremely sunk, hands shrivelled—contracted, feet less shrivelled, extended by spasms; nails livid, *mammary veins* gorged, *lungs* emphysematous—prominent, bullæ as large as nuts, no unusual engorgement; *coronary veins* congested, *heart* hypertrophous, and fatty, three valves diseased; all the cavities, and also the *aorta*, contained thin black blood, the right side being gorged. In the right ventricle were two small amber-coloured coagula. Some black blood in the *pulmonary artery*, which was dilated to double; *liver* not materially altered, *gall bladder* only half full, *bile* natural.

Exterior of Intestines.—*Stomach* exhibited increased straggling vascularity, *colon* pale, in parts distended—in others contracted to the circumference of a finger. *Small intestines* universally of deep pink colour, with transverse red streaks, following the lines of the valvulæ conniventes, especially in the jejunum. Surface flattened.

Interior of Intestines.—*Stomach* contained water, with much slime, involving powder of mustard. A thick coat of adherent slime overspread the mucous membrane, which was universally mammellated, and red in several extensive patches; pylorus thickened, calibre of the first half-inch of the duodenum obliterated by contraction; mucous membrane of the duodenum and jejunum of deep pink hue, with turgidity, from minute vascular injection, rather paler pink throughout the ileum, and a blush throughout the colon; a few isolated glands slightly enlarged in the duodenum—not elsewhere; a thick layer of tenacious slime adhered to the whole mucous surface. Duodenum and jejunum contained much fluid, like thick gruel, tinged pink: the ileum contained a matter resembling half-boiled white of egg, with much thin, colourless, rice-

* This character existed equally in the girl, who died in the Mary-le-bone Infirmary, and in the boy in James Street. It is preliminary to the effusion of lymph on serous membranes.

gruel fluid. The latter fluid existed in abundance in the colon. Calibre of the sigmoid flexure almost obliterated by contraction. No faecal matter or scent, but a sweet oppressive odour. Bladder contracted to the size of a flattened fig.

Signed by four physicians and two surgeons.

On comparing these dissections with that of the child who died in Mary-le-bone Infirmary*, it will be perceived that they are essentially identical.

Thus the history of cholera, from its mildest to its most malignant form, is complete; all the intermediate links being filled up. Of the twenty-eight children who recovered, several, on admission, presented very slight symptoms; yet, from blood-letting being tardily or inadequately performed, they fell into a state equally perilous with that which existed on admission in the child who died. This child, who did not from the first present *all* the characteristic symptoms of malignant cholera, exhibited, on dissection, precisely the same morbid appearances as another child (Case I.) who *did* present all the characteristic symptoms; and the appearances in both were, with one exception, identical with those of the aged female (Case II.), who also was affected with the disease in its malignant form. The exception consisted in the minor degree of glandular development in the latter case; a circumstance probably connected with the age of the patient, as the development seems to be more marked in children.

Hence it follows that the disease, whether "mild" or "malignant," is the same in its intimate nature, and different only in degree. It follows, also, by an equally rigid deduction from facts, that the affection is primitively and essentially of an inflammatory kind; for in the children the symptoms were distinctly phlogistic, and they yielded to antiphlogistic treatment. In the one that died, and who had presented the phlogistic symptoms in question, the postmortem appearances corresponded with those exhibited in the malignant cases; whence it is to be inferred that in the latter the appearances originated

in the same cause. The character of the appearances themselves supplies a further argument, since they are such as congestion is wholly incapable of producing: inflammation alone can account for them. But it may be said that there are cases, especially those of very sudden death, in which no increased intestinal vascularity exists; in which nothing more is to be found than the characteristic *fluid* contents of the canal. Still, as these contents have, under more intelligible circumstances, been traced to an inflammatory cause, the evidence which they afford is scarcely less conclusive; they are vestiges, and therefore proofs of inflammation, in the same way that recent lymph effused on a serous membrane (though unaccompanied, as is not unfrequently the case, with the slightest discernible vascularity; though the membrane be even paler than natural, as we sometimes find it) affords a positive indication of antecedent inflammatory action.

Which, it may next be inquired, is the first in the series of morbid actions? Is the impression on the nervous system, or the affection of the bowels? The cases of death within a few hours, and even within a few minutes, appear to afford so strong evidence of the former, that to doubt it would at first sight seem rash.

Yet a close analysis of the circumstances leads to the opposite opinion. Supposing, for the sake of argument, that the primary impression is on the nervous system, its immediate consequence is, a retardation, verging on suspension, of the circulation, with a congestion of the great vessels and internal organs. But congestion is utterly insufficient to account for the intestinal changes; for the minute, homogeneous capillary injection, the thickening, the glandular development, and the extraordinary secretions. A mere straining, or infiltration, from gorged vessels, never yet produced changes of this description—a fact too familiar to every experienced morbid anatomist to require insisting upon. On this view, then, the disease could not be produced. Let us examine the other—that entertained by Dupuytren. An individual may experience a stitch in his ribs, and a shivering at the same moment. Thus the first evidence of an acute pleurisy is simultaneous with the complete establishment of the disease. Why may not an intes-

* Vide Med. Gaz. March 10, 1832, p. 855, where for "fresh," read "thick." The name of Mr. STAFFORD, Surgeon to the Infirmary, who was present at the dissection, was accidentally omitted.

tinal inflammation supervene with equal celerity? Supposing the attack to be smart, the patient may sink at once under the first nervous shock; for we must recollect that, in this disease, not a part, but the whole of the intestinal canal, is affected; and that the canal is in connexion with almost the whole of the ganglionic nervous system. When, then, we recall to mind the nervous depression connected with an ordinary gastro-enteritis, and still more the overpowering exhaustion and faintness familiar to the personal experience of most persons, as resulting from so slight a cause as a twinge of colic, or the action of a purgative, we can form an adequate conception of what that impression on the nervous system must be which proceeds from a violent inflammatory irritation of the whole tract of the alimentary canal. Supposing a patient to be carried off by this first shock, we should not expect to find, on dissection, well characterized vestiges of inflammation, for the process tending to produce them has been intercepted in limine. I recently attended a case of pericarditis, which appeared to have been fatal in 24 hours. The vestiges, though unequivocal from their nature, were almost inappreciably slight. Thus, then, would I venture to explain those cases in which death is so sudden as to breed the idea that the fatal impression is primarily on the nervous system.

In more protracted cases, violent vomiting and purging, with epigastric burning and pain, generally precede any appalling degree of nervous collapse. In such we should expect to find, and accordingly we do find, marked vestiges of inflammation, as, for instance, in the cases above detailed.

Admitting the nervous shock to originate in abdominal irritation, it becomes intelligible why the ganglionic system alone is affected; which is not so obvious on the doctrine that the disease commences with a primary impression on the nervous system.

If this view of the pathology of cholera be correct, its application to the treatment is too simple to require explanation. Free bleeding, already found so effectual in the early stages, should not be confined to the arm, but a large number of leeches also, with the usual fermentations, &c. should be applied to the epigastrium and abdomen. Internal stimuli should be avoided to the utmost.

Bilious dejections are rather the index than the cause of the solution of the disease. As the liver is one of the organs most susceptible of congestion, so it is one of the last to recover from it. When, therefore, a free excretion of bile shews that it has resumed its function, we may presume that the disease has come to a crisis. With the view of stimulating and disgoring the organ, calomel should be given, as being by far the most efficacious remedy for the purpose.

When the intestinal lesions are great, such, for instance, as those in the above cases, and the patient has sunk into decided collapse, he must, as it appears to me, inevitably die; for, as the cause is constantly in operation to regenerate the collapse, the nervous system, though temporarily roused, cannot be maintained in an active state for a sufficient period to allow of the necessary reparative process through the medium of sufficiently energetic means. Hence the paramount importance of grappling with the disease in the first moments of its existence.

The question whether the malady be imported or indigenous, appears to have created much superfluous disputation. What difficulty should there be in conceding to the advocates of the former opinion that it has already existed in this country, at Clapham, at Leeds, perhaps in the experience of several private practitioners, and that the present epidemic is nothing more than the extensive prevalence of what, in the above cases, existed only sporadically? This does not make the disease less cholera—less an extremely violent and fatal malady: it does not prove it to be an *ordinary* gastro-enteritic fever—an opinion which has refuted itself; nor does it decidedly prove any thing either for or against its contagious nature.

I am, Sir,
Your most obedient servant,
J. HOPE, M.D.

13, Lower Seymour-street,
Wednesday, March 14, 1832.

MELÆNA.

To the Editor of the London Medical Gazette.

SIR,
In a lecture on Melæna, by Dr. Elliotson, as reported in your journal of

February 18th, that distinguished physician is stated to have said, "I think that when these discharges take place from the alimentary canal, that, at any rate, they are *blood* which has undergone a certain change." He does not advance any reasons for entertaining this opinion, unless the following may be considered as such:—"It is admirably remedied by small and frequent doses of oil of turpentine, *just like hæmorrhage* from these parts;" and that "persons are exceedingly exhausted by this discharge."

Had this come from a person less deservedly eminent, I should have considered any remarks on it unnecessary; but an error, if such it be, in pathology, sanctioned by such an authority, cannot but lead to injurious mistakes in practice.

I will not occupy your pages with any speculative opinions of mine, but give the particulars of a severe case of Melæna which came under my notice about five years since.

John Edmonds, a labourer, in the prime of life, generally of robust health, had for a few days felt but little desire for food; still he followed his employment, until, rising in the morning, he was suddenly seized with a profuse vomiting and purging, of a tenacious semi-liquid substance of a reddish-black colour, which he and his attendants considered to be grumous blood. Of this matter he continued for several days and nights in succession to evacuate immense quantities; the first two days respectively not less, I should say, than two gallons. When poured from a *white* vessel it left a reddish tinge, but when seen in a mass, this tinge was scarcely perceptible.

There was a peculiarity in the evacuating this matter which was not unobserved by the patient—namely, that both the vomiting and purging were of a passive description, and called for no effort on the part of the patient; on experiencing a sensation of fulness, he had only, as it were, to allow its escape.

Whilst this matter continued to be passed, the symptoms were not at any time varied; with an almost overpowering feeling of lassitude, the pulse was full and soft, but unnaturally slow; the tongue was moist and not loaded, but of a dirty-white colour. A distressing *obtusè* pain in the head, which almost prevented his raising it from

the pillow; it seemed, he said, as though "a weight were on it." The abdomen full and soft; not in the least uneasy or painful on pressure. Respiration natural; the surface of the body of low temperature, and its colour not unlike that which attends a severe case of chlorosis.

Blisters were applied to the nape of the neck; calomel combined with opium administered, together with sulphate of magnesia in infusion of roses; but no *stimulants* or *tonics* were allowed.

At the end of a week these symptoms underwent a complete change; the system appeared to have been relieved by a sedative power which had influenced all its functions. The temperature was now above the standard of health; the pulse sharp and quick; the pain in the head very acute and lancinating, with intolerance of sound; and if he stepped incautiously, or struck his foot, the concussion obliged him to press his head with both hands.

These symptoms continued, more or less violent, for several weeks, and were eventually removed by a strict antiphlogistic regimen, assisted by frequent general bleedings, and other suitable remedies. As may be expected from the symptoms enumerated above, the blood was greatly buffed and cupped. The patient's recovery was perfect and permanent; he had never previously suffered from either biliary or dyspeptic symptoms.

Both the quantity of matter which was evacuated in this case and the treatment which was adopted, preclude my embracing the pathological opinion advanced by the learned Professor. Had the matter been *blood*, it is clear the patient must have died, and the treatment would have facilitated this fatal termination of the case. I considered then, and I still firmly entertain the belief, that the matter consisted of vitiated secretions, not only from the liver, but also from the whole secreting surface of the intestines, mixed with a sufficiency of *blood* to give it that red hue which was observed on the sides of the vessel, and which probably was thrown off by the liver and spleen, in attempting to remove a state of venous congestion consequent to their interrupted healthy functions.

Should this reach the eye of Dr. Elliotson, I know he will give me credit for having made these remarks with the

best intentions; and, I may add, that I shall feel grateful to him, or any of your readers, if it can be shewn that I am in error.

I have the honour to be, sir,

Your obedient servant,

JAMES HURD.

Cleeve Cottage, Yatton, Bristol,
February 23, 1832.

FRACTURE OF THE FORE-ARM.

To the Editor of the London Medical Gazette.

SIR,

HAVING had an opportunity during the last six months of seeing many cases of fracture of the forearm, I have been enabled to put into practice the mode of treatment usually recommended and adopted. Should the following observations upon the treatment of such fractures appear to you worthy of a place in your useful journal, you will oblige me by inserting them.

I remain,

Your obedient servant,

EDWARD LONSDALE,

2d House-Surgeon.

Middlesex-Hospital,
Feb. 29th, 1832.

The position generally adopted for placing a fractured forearm in, is that between pronation and supination, the palm of the hand being turned towards the chest. Now if the splints are applied to the arm while in this position, when the bone unites it will almost always be found that the hand cannot be perfectly supinated; for this reason—the lower portion of the radius being carried with the hand, when it is placed with the palm turned towards the chest, is in the position between pronation and supination; while the upper portion, not following it, remains in the state of perfect supination. It is obvious, that if the two portions of bone unite in this position, the hand can never hereafter be perfectly supinated, because the portion of bone upon which the motion of supination depends, is supinated to the utmost, and, consequently, cannot carry the hand further into the state of supination than it already is. Sir Charles Bell, in his "Operative Surgery," says, speaking of the hand being allowed to fall into pronation,

"it will be manifest, at the same time, that if the bones be allowed to remain in this position, a great irregular callus must be formed betwixt the ends of the bones, and that when they are thus fixed together, the hand will no longer be capable of supination." I conceive that the only way to insure the future motion of perfect supination, is so to fix the lower portion of the radius that its degree of supination correspond with that of the upper portion. Now this is certainly not done by placing the hand with the palm towards the chest, because, as I said, the lower portion of the bone is then between pronation and supination, while the upper is in perfect supination. By adopting the following position for the arm, this evil is prevented. Before applying the splints, let the hand be brought into the utmost state of supination; and while in this position, lay it on a splint about the width of the forearm, extending underneath from the back of the hand up to the elbow; then fix it to the splint in this position, with a bandage carried from the fingers upwards. Still keeping the hand supinated, let another splint be applied, extending from the palm of the hand or fingers up to the bend of the elbow, and broad enough to lie on the fleshy part of the thumb, as it will then prevent any tendency to pronation. Another bandage should then be firmly applied over both the splints, so keeping the forearm completely supinated between them. If the splints are well fixed at the hand and elbow, no pronation can now take place. The advantage of this position is, that when the bones unite, the hand will already be in perfect supination, and only have to regain the motion of pronating the hand, which will soon be got when the arm comes to be used. When the splints are applied, and the arm put in the sling, the palm of the hand, instead of being towards the chest, is turned upwards. The position gives the patient no inconvenience.

Of the advantages of placing the forearm in perfect supination before applying the splints, I am convinced; as in the cases in which I have tried it, the motion of supination and pronation have always been perfect afterwards; while in those cases where the ordinary position is adopted, there is generally more or less incapability to bring the hand into perfect supination.

THE "NO CHOLERA" CRY.

To the Editor of the London Medical Gazette,

SIR,

At a time like the present, when a man is held up to public observation and ridicule if he fearlessly advance his opinions—when the press is almost at its acmé of venality, and the mind of the public directed by such biassed and unprincipled rulers, I, as an humble individual, am induced to look to you for a corner in your valuable journal, open equally to all parties, and the organ of none.

The subject of cholera is bruted wherever we go, and almost daily a medical man's opinion is required as to the momentous question. I confess, like many of my brethren, I am at a loss to reply. If I say what I think, that cholera exists in London, I am denounced as an unprincipled monster—as one who is to profit by its existence—probably connected with those to whom such "enormous stipends" are granted; but if I say, no disease exists in the metropolis—that this is an idle rumour; then what will truth and conscience say? Doubtless the public journals are influenced by proper motives when they stigmatize us as a body, pointing us out as sordidly promulgating opinions in order to reap some extraordinary benefit—when they publish signatures to petitions afterwards altered in a few of their details, and when they will hear but one side of the question.

Now, sir, allow me to ask these worthies, who think themselves qualified to legislate upon reform, cholera, contagion, or, in fact, upon all subjects, however important, who should be the proper judges of these things? Is the physician questioned as to military laws, or the soldier upon those connected with physic? Or again, is a lawyer a proper person to consult in theatrical matters, or a tragedian as to the merits of a picture? But every one now-a-days thinks he understands all professions, unmindful of the good old precept, "*Nec sutor ultra crepidam.*"

Again, sir, I should like to be told how these "immense profits" are to accrue to medical men, not reconciling what is really the case with what is erroneously stated. I have heard of, and know, medical men, who are told they must not attend cholera patients, upon

pain of dismissal from this or that family; and when the disease gains ground, many families will leave the infected city. But, supposing the malady did rage extensively, would its victims be found among the rich, the middling, or even the respectable of the lower classes? Probably not; at all events, not so generally as to be of any importance to the medical man. There must, however, be some who receive salaries for their exertions in the public cause; and who are they? Why, it is those praiseworthy individuals who, to the neglect of other duties, are daily employing themselves in endeavouring to mitigate the disease—who superintend the exertions of those no less praiseworthy men whose occupation it is to visit the poor—who frequent the hovels and habitations of the (but for them) neglected poor; whose salaries are scarce sufficient to enable them to keep up even the outward appearance of their station in life. It is these men that the foul pen of slander attacks, as if it grudged the poor the consolation of medical aid, and denied our profession the credit due to our good intentions. Is it that they wish us to do every thing for fame, which, by the bye, goes very little way in these days? Alas! I blush for my country, when I call to mind that its prominent qualities are selfishness and ingratitude. In our sister country, are the authorities called to account for their *unnecessary* care in appointing a surgeon to every street, or is the profession held up to ridicule and derision?

But far be it from me to wish to intimidate the minds of men; this is no less a crime than to place them off their guard, as it were, in dangerous quiescence. I should like to know, from the authorities above alluded to, how it is that the English cholera is never prevalent at this season; and to remind them, that the symptoms of that now prevalent agree with those of the disease raging in Sunderland, which corresponded with that on the continent, which was nearly identical with the cholera of India.

When the disease was in India it was cholera, when in Russia and Poland it was the same, and the justice of quarantine laws allowed. It however arrived in Sunderland, and after a time was recognized as cholera; but now it has come to London, its name and its nature are to be changed.

I have not seen the disease in India, nor did I in Sunderland, therefore cannot judge for myself how far their symptoms agree with our *new disease*; but if I may judge from well-authenticated reports, the cholera now existing in London, and that which ravaged in India, are one and the same disease.

I beg to apologise for taking up so much of your space, and remain, sir,

Your obedient servant,

J. E.

MEDICAL GAZETTE.

Saturday, March 17, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Ar-tis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

PESTILENTIAL DISEASES OF ENGLAND.

HAS it never occurred to any of our sturdy advocates for the endemial origin of malignant cholera, to look into their authorities, and inquire whether we have any instance of a pestilence raging in this country, that has not been imported? If they have made the search, there can be no difficulty in guessing what they have found,—they can (we will venture to say) adduce *no* instance: but if they still persevere, and insist that this is no reason, why the country may not now originate a disease resembling the Asiatic cholera, we must request them to favour us with some shadow of a reason for the curious occurrence of the same phenomenon in various localities, all lying along a certain route, which the same disorder would seem to be continuously tracing for the last fifteen years. Can they assign any probability to shew that this complaint has sprung up of itself, independently, in all the places in which it has successively appeared? We apprehend they cannot; while, on the other hand, we have the most ample evidence to prove that never yet has malignant disease, contagious or epidemic, prevailed in this country (leaving, of course, those out of the question which

have been owing to a well-known local cause, and which may have spread to a certain limited extent,) that it could not be satisfactorily traced to a foreign origin.

The greatest visitations with which England has been afflicted, after the introduction of the obstinate and notoriously exotic and contagious eruptive fevers—measles and small-pox,—were, the pestilence which raged here, as well as almost over the whole earth, in Edward the Third's reign,—the sweating sickness, in the 15th and 16th centuries,—and the plague, properly so called. Of these, it may not be irrelevant to take a short historical survey; and perhaps it may even conduce to render us more tolerant of those ills we have, when we are led to reflect on those which our ancestors had to bear. A modern speculatist has ventured to suppose, "that, however much medical skill and energy may alter the form and pressure of disease, death will have its quota: that the stream of mortality is uniform, and that the totality will always prove pretty much the same." But, however much this may be applicable to the state of things during the last century or two, no thinking person will extend the proposition to the events of an earlier period: the mortality which ravaged England from time to time in the ages of which we have but meagre records,—and which wanted medical skill as well as medical historians, was tremendous. We shall proceed to notice a few of the visitations that are well authenticated, and to which we now more particularly allude.

It was in the autumn of 1348, that England was visited by that contagion, which, in its wide and destructive progress, does not fall short of any in the history of mankind. It began in Tartary in the year 1345, and continued to plague the earth for above seven years. After having spread through the various kingdoms of Asia, it passed

into Europe through the Levant, and devastated in turn every country and district into which this part of the world is divided. The kingdom of Naples lost above 350,000 of its inhabitants; Venice 100,000; Florence above 60,000; and Avignon, then the chief city of the papacy, was nearly depopulated. We have Guy de Cauliac's account of the disease as it existed in the latter place: he practised there during its prevalence; and his contemporaneous notes are of great value. It raged in Avignon, he says, for seven months, during the first two of which it presented an aspect considerably different from what it bore during the remainder of the time. It was attended, during its first period, with violent fever and profuse hæmorrhages, under which every one seized by it, perished within three days. In the second period, the chief symptoms were, continued fever, carbuncles, and abscesses in the axilla and groin, and with these, the disease was, until near its extinction, almost as fatal as during the first two months; the mortal period, however, was now prolonged to *five* days. Twenty-four millions of people, it is calculated, were cut off within the papal jurisdiction; and in short, it is supposed, that by this plague, *one-fourth* (the greater number of authorities say *one-third*) of the population of the world was destroyed.

But before we come to the description of the disease as it existed in England, we must notice the account which we have of it, as witnessed in the Eastern Empire by the imperial historian, John Catacuzenus. "It was," says he, "such an unconquerable evil, that neither any diet or strength of body could resist it; for it pulled down all bodies alike, as well the strong as the weak; and those who were most diligently looked after perished, as well as those who wanted all things. The knowledge of the physi-

cians was completely put to a stand; for some patients, enduring a little, died the same day, some the same hour; but those that held out to the second or third day were first taken with an acute fever, and the distemper affecting the head, were rendered speechless and insensible to all that was done; and so dropped off, as it were, in a profound sleep. Others were not taken in the head, but in the lungs: these had an inflammation in their inwards, which created acute pain about the stomach, so that they sent up blood and a cadaverous stench from within. Their jaws and tongues were dried up with heat, and black and tainted with gore. Some had imposthumous ulcers and black blisters upon their arms and under their arm-pits: some in their cheeks and other parts of the body; and in others there arose black spots all over the person—in some more superficial and visible, in others deeper and more obscure. But almost all died alike. The few who escaped, however, were no more touched with the mischief; for it never took any twice, so as to kill. There was no certain remedy; what was good for one, was to another in the same condition fatal." The royal writer then proceeds to give some account of the *moral* effects of the pestilence, between which and the account of the same effects given by Thucydides, in his inimitable history of the plague of Athens, we should be strongly tempted to draw a comparison, did our limits allow us to be so discursive. But to return to "this fiery scourge of God," and its invasion of England.

"About the 1st of August, 1348," says the learned and accurate Joshua Barnes, "it began in the sea-port towns on the coasts of Dorsetshire, Devonshire, and Somersetshire; whence it ran up to Bristol, so that the Gloucestershire men forbad all intercourse with the Bristolians. But this familiar fury

wanted no medium to introduce it; for as the scripture says of the pestilence, that it walketh in darkness, or invisibly, its progress not being to be found out; so unexpectedly and contrary to human precaution this plague also walked, or rather flew, among the Gloucestershire men: whence it went to Oxford, and about the 1st of November it reached London, and finally spread itself all over England, scattering every where such ruin and desolation, that, of all sorts, hardly the tenth person was left alive."

The same historian has left us an account of the disease, which, though somewhat prolix, yet, for its quaintness, may not be unacceptable to the reader.

"The method of this plague was to stay but two or three days with any person (for it surely killed in that or less time), and but five or six months in any place, and then to seek out for fresh quarters. And as if disdaining to be rivalled by any competitor, it solely reigned, either keeping back all other sorts of diseases, as being fully assured of its own sufficiency, or rather engrossing to itself all the armory of death and destruction; for it made more abundant conquests than ever all sorts of other diseases did at one time before. It took men generally in the head and stomach, appearing first in the groin, or under the arm-pits, by little knots or swellings called kernels, being biles, blains, blisters, pimples, wheals, or plague-sores, generally attended with pestilential fevers, which occasioned spitting and vomiting of blood, whence, for the most part, they died in a manner presently, or in half a day or two at the most. After the third day, though seldom any lived so long, there was hope, and yet then, many falling into a deep sleep, never waked more. The blisters and kernels aforesaid were certain symptoms of this malady; but the most deadly and deperate was, when many of these imposthumous swellings would rise over all the body, which being hard and dry, upon cutting sent forth little or no purulent matter; though by this means, after many cuttings and torments, not a few were healed in the end. But this remedy

was not found till very late, nor then in many places neither; so that generally it was an irresistible evil, every where tyrannizing without control. Nor did piety here avail: for those whose tender minds obliged them, in mere compassion, carefully to look to their friends or others, in this their deplorable condition, were for the most part surely seized on themselves; for, as we are told, the influence of this disease was so contagious, that it not only infected by a touch or breathing, but transfused its malignity into the very beams of light, and darted death from the eyes; and the very seats and garments of such proved fatal. Wherefore parents forsook their children and wives their husbands, nor would physicians here make their visits, for neither were they able to do good to others, and they were almost certain thereby to destroy themselves. Even the priests also, for the same horrid consideration, forbore either to administer the sacraments or absolve the dying patient. But yet neither priests nor physicians, nor any other who sought thus to escape, did find their caution of any advantage; for death not only raged without doors as well as in chambers, but, as if it took indignation that any mortal should think to fly from it, these kind of people died, both more speedily, and proportionably in greater numbers. * * * No physician could tell the cause or prescribe a cure; and even what was saving to one was no less than fatal to another."

The mortality in London, beyond a doubt, was very great. Sir Walter Manny, one of the most distinguished noblemen of the court of Edward the Third, purchased at this time a piece of ground, now the site of the Charterhouse, for the interment of such persons as the churches and church-yards of the city would not suffice to hold; and it appears, from an inscription upon a stone cross erected on the spot, and which was still standing in the time of Stow, that more than 50,000 bodies were buried in this ground in the space of one year. This evidently cannot be supposed to exceed half the number of the persons who died in that period:

so that we may safely conclude with Joshua Barnes (and Mr. Rickman, in his late returns), that fully *a hundred thousand* perished of this pestilence in London alone. Norwich also, which seems to have been then one of the most populous towns in the kingdom, lost 57,000; and in other places the terrible mortality was proportionate.

It is pretty certain that this pestilence, after its first dreadful visit, was no stranger in the country. We have the authority of Holinshed and other historians, that in 1378-9 it again ravaged the north of England, which it left almost unpeopled. In 1479 it so raged, according to Holinshed, for fourteen months, that it cut off "innumerable of people;" and Hume thinks it was the same scourge which visited England in 1563. "In the reign of Elizabeth, it was brought by the Earl of Warwick's army from Havre de Grace; there they caught the infection from the French soldiers; and, being much fatigued and ill-fed, sometimes 100 men died in a day. When it came to England, above 20,000 persons died of it, in the city of London, in one year.

We now come to notice a pestilence which was for a time supposed to be peculiar to this country—the *sweating sickness*; or, as it was called by professional foreigners, the *sudor Anglicus*. It was introduced into England along with the army of the Earl of Richmond (afterwards Henry VII.), one of the most wretchedly-appointed bodies of men ever enlisted under one banner. Some, however, have gone so far as to trace the disease through these troops to the Turks of the Levant, who contracted it at the siege of Rhodes. However this be, it is certain that though it was not attended in this country with glandular tumors and carbuncles, like the true plague, yet it had evidently only undergone some non-essential mo-

difications on its arrival amongst us. Dr. Mead instances its "excessive faintness and inquietudes, inward burnings, and critical sweat," as serving to identify it sufficiently with Levant plague; for these, he says, are "no where observed to have so much intensity as in true plague." Cains, who has particularly described the *sickness*, has fallen into some unaccountable exaggerations and errors, which lower him somewhat in our estimation; at all events, for the novelty's sake, as well as for the unquestionable eminence of his authority, we shall be pardoned if we quote the account of the disease with which Lord Bacon supplies us in his "Reign of Henry the Seventh."

"About this time (1485), in autumn, towards the end of September, there began and reigned in the city and other parts of the kingdom, a disease then new, which, of the accidents and manner thereof, they called the sweating sickness. This disease had a swift course, both in the sick body and in the time and period of the lasting thereof; for they that were taken with it, upon four and twenty hours escaping, were thought almost assured. And as to the time of the malice and reign of the disease ere it ceased: it began about the 21st of September, and cleared up before the end of October, insomuch as it was no hindrance to the King's coronation, which was the last of October, nor (which was more) to the holding of Parliament, which began but seven days after. It was a pestilent fever, but, as it seems, not seated in the veins or humours; for there followed no carbuncle, no purple or livid spots, or the like, the mass of the body being not tainted, only a malignant vapour flew to the heart and seized the vital spirits, which stirred nature to strive to send it forth by an extreme sweat. And it appeared, by experience, that this disease was rather a surprise of nature than obstinate to remedies, if it were in time looked unto; for if the patient were kept in an equal temper, both for clothes, fire, and drink, moderately warm, with temperate cordials, whereby nature's work were neither irritated by heat, nor turned back by

cold, he commonly recovered. But infinite persons died suddenly of it, before the manner of the cure and attendance was known. It was conceived not to be an epidemic disease, but to proceed from a malignity in the constitution of the air, gathered by the predispositions of seasons; and the speedy cessation declared as much."

His lordship is a little too curious and speculative touching the proximate cause of the disorder; but, upon the whole, we look upon his account of it as quite as good as any we possess. It is not true, maugre the authority of Caius, that the sweating sickness was a disease peculiar to the natives of England; not only did many foreigners die of it at different times in this country, but in 1529 we find that Germany and the Netherlands were visited by the true *sudor Anglicus*; it was this, too, that interrupted the celebrated conference, at Marpurg, betwixt Luther and Zuñglius, concerning the eucharist. Historians reckon four visits that it paid to England after its first introduction—in 1506, 1517, 1528, and in 1551; nor should we omit to mention, that no class of society seemed to be privileged to escape its ravages; it even invaded the court, and threatened royalty itself. In Lord Herbert of Cherbury's History of Henry VIII. we find the following curious passage:—

"Shortly after (July 1517), the sweating sickness (called for the propriety by which it seized on the English nation chiefly, *sudor Anglicus*) did much infect the kingdom, being of that malignity as within the space of three hours it killed. This caused the King to leave London, and adjourning three terms 1517, to remove Trinity term 1518 to Oxford, where yet it continued but one day, and was adjourned again to Westminster. Nevertheless divers knights, gentlemen, and officers in the King's court, died thereof; as the Lord Clinton, Lord Grey of Wilton, and others of quality; the vulgar sort so commonly perishing as in some towns to take away half the people; in others the third part."

And at a subsequent page:—

"As the plague destroyed the French in Italy, the sweating sickness consumed very many in England, it seeming to be but *the same contagion* of the air, *varied according to the climate*. It was first known in England in 1485, then 1506, then 1517, and now in 1528, when it so raged that it killed ordinarily in five or six hours space,—invading even the King's court, where not only Sir Francis Pointz, Sir W. Compton, and Mr. W. Cary (of the King's bedchamber) died of it, but the King himself was not without danger."

It had been generally supposed that the *sudor Anglicus* was extinct since the year 1551, but Dr. Mead was of a different opinion. The *Dunkirk fever* of September 1713 was a complaint apparently similar. It was brought over by our soldiers from that place, where it seems to have existed as a modification of the plague. Certain it is, that at Dantzic, not long before, the plague had been prevalent, and had spread thence into several cities of the north. In England the chief symptoms which marked it, were headache and feverishness, which went off in profuse sweats after a day's confinement; while in Dunkirk it was attended, in addition, with distressing discharges from both stomach and bowels.

Here we are obliged to break off for the present; in our next number we hope to resume the subject.

—

PROCEEDINGS OF THE CENTRAL BOARD.

It is a great calamity at a period like the present, and a very serious aggravation of the evils which must always attend the prevalence of a malignant epidemic disease, that the arrangements connected with the public health should be left to a Board certainly not possessing, and we fear not altogether deserving, the confidence of the profes-

sion. In Edinburgh, the whole city is under the observation, the guidance, and the moral influence of an association of intelligent men, who, having no personal purposes to serve, cordially co-operate in the accomplishment of public objects; estimating the disease at its true importance and no more—neither denying its existence nor exaggerating its prevalence; faithfully executing the trust reposed in them, and receiving in return the fullest confidence of their townsmen. If such be the enviable fortune of our northern neighbours, we need not go far to seek a contrast; for here, in our own Board in London—called “Central,” we presume, in apt allusion to the centrifugal force by which every thing within its sphere is repelled—we have a conspicuous illustration of the blessed effects of measures ill contrived and badly executed; where no common principle is acknowledged, and no general end obtained; where recommendations are addressed to practitioners which they do not follow, and statements given to the public which they do not believe. As to the former point, indeed, we cannot adduce it as matter of astonishment, for it is not to be supposed that the profession in England would be much influenced by the clinical instructions of a Board, only one member of which ever treated a single case of cholera asphyxia in his life, and not an individual of which is at all acquainted with the diseases of that class of the community among whom the present epidemic prevails. This, we say, is nothing wonderful; but we certainly do think it matter of wonder that the Board should not have organized some general plan of observation, by which the public might have profited by individual experience, and by which something like general deductions might have been drawn. Failing this, we are utterly at a loss to conjecture of what use the Central Board either actually is, or is

conceived to be. Mere numerical returns, keeping alive public excitement and leading to no definite results, are worse than useless. Yet such is the case at present; events being suffered to pass unnoticed, and information lost for want of being recorded. Not a victim who sinks under the disease but should add his quota to the general stock of information. A return, on a well-digested and simple plan, exhibiting the most important practical points, ought long ere this to have been in the hands of all practitioners; and one great object of the Central Board ought to have been to compare, study, and digest this mass of facts, and present them to the world, from time to time, in a perspicuous and intelligible form. Instead of this, the only evidence of their existence is the daily publication of the bald numerical return of their Secretary, and the appearance, at irregular periods, of a collection of desultory, ill-assorted, and often irrelevant, letters and papers, stamped with the royal arms and sanctioned by the “authority” of the Board, though denied their “responsibility.” So apparent is the necessity for some organization—so obvious the propriety of endeavouring to glean from the cases what instruction they can afford—that several of the Local Boards have proposed schedules for the record of cases calculated to elicit at least some useful information. One of those which has been adopted in Marylebone, St. James’s, and St. George’s, we have seen; and though it cannot, perhaps, be regarded as perfect, yet it is quite obvious that some such methodical plan of investigation, if generally acted upon, would be highly beneficial and instructive. The reader might naturally expect to learn that the Central Board, if they had not the genius to invent, had yet the sagacity to imitate, and had urged the general adoption of a similar step: will it be credited that they not only refuse

their sanction to any thing of this kind, but actually have so tied up their own hands as to render it impossible for them to require of practitioners any thing beyond a numerical return, even if they were disposed? The words of the Order in Council recently issued are, "a numerical account of all new cases of cholera," "in the manner or form required by the Board of Health, *in the city, town, or district.*"

Thus is every individual Board of the hundreds which exist, to make its own particular choice of the mode in which the return is to be made; and thus have the Central Board, by their own act and deed (for, of course, the order was drawn up at their suggestion), prevented all chance of any uniformity in the plan of observation and of record.

Nor is the case mended when we turn to the second question, and inquire how far the conduct of the Central Board has been marked by good faith and candour as regards the public. Inspectors have been appointed, for example, to the different districts of the metropolis, and one would naturally suppose that the cases would be given to the public as represented by these officers. Not so; for by their own shewing we have at least one instance in which these had undergone no inconsiderable modification in passing through the Board room. The use made of the Marylebone cases is the point to which we allude. It is notorious that such cases only had been reported as were of a malignant kind—that more than one-half of these had proved fatal—and that the list was consequently, and with justice, regarded as including only the most marked and pestilential forms of the disease. Such were the circumstances, when sixteen cases were suddenly announced in one day as having occurred in Marylebone. The consternation which followed in the parish was great; but, by and by, it was currently rumoured that the cases were of a very

mild and equivocal character; and, behold! on the third day, no reference to the subject having been made in the official reports, the Secretary to the Local Board published a direct contradiction of the announcement issued by the Board of Health. *They* had included the cases among the malignant, without a word of explanation; *he* having expressly stated that they were mild, not malignant. And how do the Board get over this dilemma? Why, by admitting in their next return, that the cases in question were reported to them as—"mild;" they actually acknowledge that they were reported to them as mild, and yet they placed them without comment among the malignant! This is to trifle with the feelings of the public in a manner quite intolerable. Nor is this all: seven cases were reported from Marylebone on Wednesday; they were all included in the general list, without a single comment. Do the Board mean to say that these were all malignant cases, and properly placed with those of which the average mortality is above one in two? We know not what answer might be given to this, if such inconvenient questions were answered at all; but we have made it our business to ascertain this point, and we can inform the public that all the cases except one were mild, and of a nature which could not have excited any alarm, except from the false importance given to them in the official return. It is as we feared it would be;—the Central Board have not had moral courage to act in a straight-forward, candid manner; they have too obviously looked upon any case bearing the name of Cholera as a God-send; and seem to have thought chiefly of the immediate effect which swelling the returns would have on their own interests, forgetful of the discredit and distrust which the exposure of such proceedings cannot fail to bring upon them.

COLLEGE OF PHYSICIANS.

Monday, Feb. 27, 1832.

SIR H. HALFORD, BART., PRESIDENT, IN THE
CHAIR.

—

Some Observations on the Plague of Athens; with a Comparison of that Malady with the Plague of the Levant. By DR. IRELAND, Dean of Westminster.

THUCYDIDES, in his admirable description of this calamity, says, that it began in Upper Ethiopia, then came to Egypt, from whence it was spread first into Persia, and afterwards into Greece. Its first approach to Athens was by the port Piræus, where its ravages commenced; it soon extended to the city, where the people were much pressed for room, by reason of the influx of the country people, who flocked into the town for protection from the incursions of the Peloponnesians. In the first perplexity occasioned by the plague, its effects were attributed to their enemies, the Lacedæmonians, who they thought had poisoned the wells; a circumstance frequently observed in similar cases—whenever the commencement of a malady cannot be assigned, it is attributed by the vulgar to poison. The account of the disease which Thucydides gives us, is wonderfully clear; at the same time he excuses himself from entering into the details scientifically, leaving this, as he modestly says, to *others*. It would be highly gratifying were we assured that he here alluded to Hippocrates, who is said, by Soranus, to have been present at Athens during the plague; but all evidence of the fact scarcely exceeds mere probability. The historian, however, has left us little to desire; he was well acquainted with the malady, having been attacked with it himself, and narrowly escaping. It is remarkable that he distinctly observes that those who escaped the disease once were never again dangerously seized by it. In the Athenian capital the plague raged with intense fury, and all animal life was equally subject to its baneful influence—the dogs died in great numbers, and the fowls soon altogether disappeared. Its infectious properties are beyond a

doubt; the language of Thucydides is clear on that point, and beautifully expressed. “The people took the infection (says he) by their attendance on each other—dying like flocks of sheep. And this effect of the disease was also a principal cause of its great mortality; for either the sick were left destitute—their friends fearing to approach them, by which means multitudes of families perished without assistance; or they infected those who relieved them, and especially such whom a sense of virtue and honour obliged most to their duty.” It is remarked, too, by the same illustrious historian, that the physicians suffered, most by reason of their greater intercourse with the sick.

The author then proceeded to compare this memorable calamity with what has been known in modern times by the more specific appellation of the plague of the Levant; and certainly the difference is sufficiently remarkable. The invariable characteristics of the modern plague seem most decidedly to have been absent in the Athenian malady; for it is not possible that if the glandular tumors and carbuncles which distinguish true plague were present, that they would have been omitted by our exact historian. He is particularly precise in his account of the external symptoms and appearances; the skin he describes as livid, or of a dull red hue, with “an eruption of small pustules and sores,” which would seem, by the way, rather to favour the notion that the disease was akin to small-pox: at all events, there is every reason to believe that it was more nearly allied to those pestilential fevers which devastated from time to time large tracts of the habitable globe than to the Levantine plague.

This latter visitation the very reverend author considered as most probably unknown to the ancients; and he particularly referred to the pestilence of Milan, in 1563, and again in 1630, as affording the strongest points of contrast between the ancient and modern malady.

In conclusion, the Dean took occasion to allude to the moral effects of these pestilential disorders. It is lamentable to think how all the springs of affection have been relaxed, and the most sacred ties of duty severed by the occurrence of these calamities. In

Athens, during the pestilence, the manners of the people became utterly impious, dissolute, and abandoned; and the historian tells us, that this indifference to all moral and religious duties arose from the circumstance that the fulfilment or neglect of them appeared to be equally unavailing, for all alike perished; and it was the universal impression that no one would survive a sufficient length of time to undergo trial and punishment. Every writer on plague has unfortunately had the same sad story of the combination of calamity and demoralization to record; but with whatever feelings we read of the conduct of the ancients under these scourges, we must confess that it is not without wonder that we find a similar bearing in those who are blessed with a more enlightened creed,—who have such sublime lessons before them in the words of the inspired writers. And the paper ended with an impressive quotation from the Psalms.

After thanks had been voted by acclamation to the very reverend the Dean for his learned paper, the President expressed a wish that some gentleman would, on a future evening, favour the College with a similar dissertation on some of the more recent pestilences which have prevailed in this country.

METEOROLOGICAL JOURNAL,

Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.

March 1832.	THERMOMETER.	BAROMETER.
Thursday . 1	from 30 to 42	30.10 to 30.20
Friday . . 2	34 45	30.22 30.24
Saturday . 3	32 44	30.25 30.13
Sunday . . 4	35 48	29.95 29.69
Monday . . 5	33 46	29.66 29.53
Tuesday . 6	32 46	29.49 29.34
Wednesday 7	29 43	29.34 29.35

Wind variable, S.E. and S.W. prevailing.
Except the 5th and 7th, cloudy; rain on the 1st, 4th, and 6th.
Rain fallen, .525 of an inch.

Thursday . 8	from 25 to 38	29.39 to 29.88
Friday . . 9	23 48	29.93 30.10
Saturday . 10	20 47	30.28 30.36
Sunday . . 11	30 45	30.34 30.16
Monday . . 12	30 47	29.99 29.90
Tuesday . 13	30 45	29.86 29.60
Wednesday 14	32 50	29.52 29.32

Wind variable, N.W. and S.E. prevailing.
Generally clear till the 13th; rain fell frequently during the 14th.

CHARLES HENRY ADAMS.

REPORT OF CHOLERA IN LONDON. UP TO THE AFTERNOON OF FRIDAY, MARCH 16, 1832.

New cases since our last report, 385

Deaths 204

	Cases from Commencement.	Deaths from Commencement.
City ...	9	3
Tower	1	1
Afloat in the River	41	20
Poplar	10	3
Shadwell	5	4
Limehouse	20	16
Ratcliffe	5	3
Rotherhithe	21	10
Bermondsey	49	22
Southwark	347	172
Newington Butts...	59	28
Lambeth	54	36
Christchurch	26	16
Westminster	16	6
Chelsea	8	6
St. Marylebone ...	50	8
St. Pancras	5	4
St. Giles's	27	14
Whitechapel	24	16
St. George's East .	7	4
St. Botolph, Aldgate	2	1
St. Luke's	10	7
Brentford	14	6
Total.....	810	406
Cases reported from other places}	16	14
Grand Total.....	826	420

Total number of cases throughout England since the commencement of the disease } 7049
Deaths..... } 2347

BOOKS RECEIVED FOR REVIEW.

History of Chronic Phlegmasiæ, or Inflammations, founded on Clinical Experience and Pathological Anatomy. By F. J. V. Broussais, M.D. Translated from the French of the Fourth Edition, by Isaac Hays, M.D. and R. Eglesfield Griffith, M.D. 2 vols.

A Treatise on Physiology applied to Pathology. By F. J. V. Broussais, M.D. &c. Translated from the French by John Bell, M.D. &c. and R. La Roche, M.D. &c. Third Edition.

An Essay on Cholera; founded on Observations on the Disease in various Parts of India, and in Sunderland, Newcastle, and Gateshead. By J. Adair Lawrie, M.D. &c.

W. WILSON, Printer, 57, Skinner-Street, London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF
Medicine and the Collateral Sciences.

SATURDAY, MARCH 24, 1832.

LECTURES

ON

THE THEORY AND PRACTICE OF MEDICINE;

Delivered at the London University,

By DR. ELLIOTSON.

PART I.—LECTURE XXV.

Remittent Fever, continued.

I WAS mentioning at the close of the lecture, gentlemen, on Thursday, that the exhalations which produce ague and aguish fevers—that is to say, intermittent and remittent fevers, are certainly not the product of animal matter; for where animal matter exists in the greatest abundance, and in the utmost state of putrefaction, these diseases are unknown, unless by chance there be in the neighbourhood a source of vegetable decomposition. I mentioned, indeed, that where people were much crowded together, and where there was the greatest filth, they were often the most free from ague; so that the *Judaicum* at Rome is said to be one of the healthiest spots in that city—the least subject to fevers of this description.

I also said that fire and smoke, whether of tobacco or whatever else, will keep away ague. The greater the crowd, the more narrow the streets, of course the less room is there for these exhalations to enter; and perhaps smoke, whether of coal, or wood, or of tobacco, may act by merely taking the place of these exhalations, and preventing them more or less from entering. It may, however, be, that they rather act by their warmth. Dr. Lind mentions, that when the *Edgar* ship of war, in 1765, was off the coast of Guinea, the crew were very sickly, and many of them

died from fevers of this description; but in the crew of a sloop of war which always accompanied her, very few were ill, and none died. In the latter there was a fire for cooking on the same deck where the men lay, and filled with smoke the spot where they slept; whereas in the *Edgar*, from the particular arrangement, there was no smoke whatever between the decks. Here were two ships off a country affording a vast quantity of malaria; the crew of the one remained healthy, and that of the other became diseased, in the way I have stated, the only difference being, that in the former the part where the men slept was constantly, or almost constantly, filled with smoke.

Aguish Diseases not contagious.—It appears quite certain that intermittent and remittent fevers are not contagious; but formerly ague was so considered. Dr. Fordyce, who was almost one of our own day, says, that both from his own knowledge, and a reference to the opinion of others, ague is contagious. Dr. Cleghorn, in his work upon the Diseases of Minorca, says the same. Dr. Wells, a colleague of Dr. Fordyce at St. Thomas's Hospital, accounts for Dr. Fordyce's opinions by remarking, that he (Dr. Fordyce) fancied that continued and intermittent fevers were mere varieties of the same disease; and as it appeared in those days that continued fever was contagious, so he was obliged to maintain that ague was contagious. Dr. Cleghorn's mistake is supposed to have arisen from his having observed that most of those who were about the sick in Minorca had the disease, forgetting that it did not arise in consequence of emanations from the sick, but from the situation in which they were placed. Of course there is no proof whatever that a disease is contagious, if you catch it from going to see a person labouring under it, for he may be in the spot where he contracted it from local circumstances; and in going to him you go to the spot which has given the disease. In order to prove that a disease is contagious, there should be a sufficient number of instances of persons going

from the place where they contracted the disease to a healthy part, and there giving it to others. There is no reason whatever to suppose that agues are contagious, though formerly not only were they supposed to be contagious, but even scurvy was also pronounced to be communicable in its nature.

Chemical Nature of Malaria.—The chemical nature of malaria, or marsh miasmata, is unknown; it is not carburetted hydrogen, and no known chemical substance has the same effect. The air of marshes is said not to differ chemically from the air of any other part with respect to its quantity of oxygen. There is no reason to suppose that it is a deficiency of oxygen, or an excess of carbon, or that it is carburetted hydrogen, that renders the air pestiferous in this respect. It is said by De Lisle that malaria has no smell. Thus much, however, is known—these exhalations may be suspended by aqueous vapour; indeed, it is said that they never rise but with some lighter body; that they require to become so suspended, in order to rise at all.

Being, however, suspended by aqueous vapour, they are even more deleterious than contagion; for, like that, a momentary application is sufficient in many cases to produce the disease; and, unlike it, they may be wafted, and produce their effect at a very great distance; whereas contagion spreads but a very short way, and after a certain distance is destroyed.

Malaria, like other gaseous bodies, too, very easily finds a barrier, so that a gauze veil is said (I can only repeat what I have read) to be efficacious in Italy, in preventing its effects upon the human body.

Period at which Malaria produces its Effects.

—If these exhalations be very strong, they may kill instantly. Persons, again, who have been scarcely exposed to them have been immediately seized with vertigo, vomiting, and syncope, and from that moment had regular intermittent or remittent fever. But usually their effect does not occur till a certain period has elapsed. In South Carolina it is said that eight or ten days is the shortest time which elapses between exposure and the appearance of the disease. The reason for so long an interval may be, that there is less heat there than in many other parts. Dr. Wells believes that autumnal agues of that part do not appear till spring—that is to say, the agues which we see in spring are the result of exposure in the preceding autumn. He founds his opinion on the fact, that strangers visit South Carolina in the spring with perfect impunity. Dr. Lind mentions having seen these fevers take place the very morning after exposure. Dr. Bancroft gives an instance of the disease occurring instantly, and then others which did not take place for many, even for nine months. Dr. Bancroft likewise ascribes the agues of spring

to the previous autumn, and for this reason: he says that he has seen persons seized with ague after they have returned to England from a warmer climate, where they had been exposed to miasmata, and that they have experienced the disease at too early a period in the year for it to have arisen from malaria at home. Probably ague occurs soonest in proportion to the intensity of the heat, and to the quantity of moist vegetable matter. It is certainly very common in England for harvesters who have worked in aguish districts, and have been exposed to the exhalations in autumn, not to be affected till the east wind blows the following spring. I have seen very many cases myself of persons who have left an aguish part in perfect health, and remained so for some time, even for months, when by chance getting wet through, or being exposed to a cold damp wind, they have been seized with ague. Dr. Macculloch doubts these long intervals, but I have seen them in too many instances to entertain the least doubt whatever.

Influence of Habit in lessening the tendency to Ague.—By habit persons become rather insusceptible of the effects of these exhalations;

by habit they cease to have ague when exposed to malaria, or if they do have it, it assumes a very mild character. This is very commonly observed in what are called the *pernicious* districts of Italy, France, and Sicily; and those persons chiefly are there susceptible of its operation in whom glandular disease has been induced. To shew the effects of habit, I may mention that General Monnet, a French General, found there was only one-half the sickness in a regiment in a malarious situation of Walcheren, the second year, that there had been the first, and that the third year his men remained at the place there was no sickness whatever. The inhabitants of one marshy district, when they go to another, suffer less than strangers—than persons who have gone to a marshy district from a place that was perfectly healthy. A physician in my parish used to relate a story from his own knowledge of a party going up the Thames, and one lady only caught an ague. She had always lived in Edinburgh, a high, dry, healthy spot, and all the rest of the party were inhabitants of a malarious place.

But it is said that one place does not by any means always give indemnity to an individual who goes to another, and that a place to season an individual perfectly for another, must be situated between the tropics, and not have a cold winter. It is found, for example, that Gibraltar will not season a person for the West Indies; but then Gibraltar is not situated between the tropics. Such a seasoning as would render a person altogether insusceptible of the disease, can only be obtained between the tropics where we can conceive malaria to be

produced in the most intense degree. It is also to be remembered that when a person has once had ague, he is very liable to a return of it from a slight cause—from such a circumstance as a cold wet east wind blowing, or getting wet through, or being in any way thrown out of health. He is rendered liable to have the disease renewed from slight causes, but altogether he is much less sensible to the influence of malaria.

Whether persons, however, have had ague or not, if they be constantly exposed to malaria, the constitution is impaired, they acquire a dull heavy look, and are bloated, or emaciated and shrivelled, pale, sallow, and weak. At Walcheren, Sir Gilbert Blane says that the people residing there were far less subject to aguish affections than strangers, but that they all looked sickly; and yet they would not allow their climate could be unhealthy; and these Dutchmen, when they saw one-half the English sick through their noxious climate, would not allow that it arose from the filthy ditches, but ascribed it to errors of diet, and the bad habits of the English. The liver and spleen are much disposed to become diseased, and dropsy to supervene. In the worst vallies of Switzerland poor persons are shrivelled, young persons look old, and middle-aged persons appear in the greatest state of decrepitude. In these vallies there is a great quantity of malaria always pent up. Indeed, so great is the injury produced by malaria, that whereas the average of human life in England is 50 years, in Holland it is only 25; in some parts of France which are very pestiferous through the malaria, you will find that the greatest average age is 22; in some it is only 20 and 18, and in others it is only 10. In some parts of Africa, and the East and West Indies, the average is as low. And yet many persons who have not been exposed to malaria are as free as the Dutch who have been accustomed to it. The Dutch living in the midst of it will not allow it to be unhealthy, exactly as a person of dirty filthy habits believes himself to be as clean as is necessary. Persons, however, who have been well-informed on these matters, will frequently, when they come to an unhealthy place, be obstinate, and act the part of mules, and many such have suffered for it. You will find it stated in Captain Clapperton's voyage, that the Captain himself, Mr. Pearce, Dr. Morrison, and Mr. Houlton, a surgeon, who went on an expedition to Africa, lay all night on the banks of a creek, when there was no occasion for it, and the next night they lay in a market-place, though there were houses enough for them to have entered. The following day Captain Clapperton was seized with fever; Dr. Morrison was seized in two days more; Captain Pearce the next day. Londer, their servant, was seized next; Mr.

Pearce and Dr. Morrison died soon; and Mr. Houlton and Captain Clapperton died ultimately. They were all warned of the dangers of thus exposing themselves. You will find instances of this awful obstinacy—this total disregard of human life—every day. Because the Dutch are obliged to have dykes and ditches in their own country, they have surrounded themselves with them when there was not the least occasion, in the East Indies; merely, I suppose, to be like their brother frogs at home.

Liability of Brutes to Ague.—It is said that in very unhealthy places even brutes are known to suffer. Dr. Macculloch says he heard that a dog at Guernsey had a tertian ague; but whether this is correct I will not pretend to say. He adds that epizootic diseases frequently prevail at the same time with marsh fevers, and that in St. Domingo the dogs are always the first affected.

Former prevalence of Ague in London.—Malaria is the constant scourge of the earth—not the occasional, but the constant scourge, producing not only these fevers, but very frequently dysentery, cholera, and sometimes neuralgia; and it causes an intermittent or remittent character to be given to other diseases, and undermines the health when it causes no disease. Malaria abounds to a frightful extent in Greece, Italy, France, America, Asia, and Africa: we know nothing of it here compared with the violence of its ravages in those parts. However, it was once far more injurious in this country than it is now; but from the energy and industry of the English character, such extraordinary improvements have been made in every thing to promote the well-being of man, that nothing abroad can be compared with our advantages. You know that Southwark was once an entire swamp, and at Westminster there is now a toll-gate, called the "Marsh-gate," from being situated in a place where formerly, and not so long ago, there was a marsh. From the improvements, however, that have been made, ague is now comparatively rare in London. It prevailed to a great extent before the fire of London in 1666, but from that time the place was better drained, so that the fire did good, as no doubt the cholera will do. Ague before the fire raged like a plague. Between 1667 and 1692 no less than 2000 persons died in London of dysentery, arising no doubt from the same cause as ague. So great, however, has been the reduction, that in 1723 there were only 44 deaths from ague; in 1730 only 16; and in the bills of mortality for the first ten years of the present century, there are only four deaths recorded in the bills from ague. Within the last five or six years agues have increased through the country and in London, and this may be easily explained. They have not increased to what they were formerly, but still there has been an increase. Sydenham says that they

disappeared in London from 1674 to 1678 ; and they disappeared in Plymouth, Huxham says, from 1755 to 1760. They were absent from Portsmouth for three or four years. They returned again in London epidemically in 1751, 1753, and 1754. Sydenham says that they returned again from 1677 to 1688, and they also prevailed at the beginning of the last century. These are all accidental occurrences. It has been said by Dr. McMichael, of the Middlesex Hospital, that during the few years that ague prevailed so much, the average heat was greater than when it disappeared ; the heat being greater, of course there was more vegetable decomposition. To shew the increase of late, I may mention that in 1823 I had but 8 cases of ague ; in 1824, I had 14 cases ; in 1825, 15 cases ; in 1826, 44 cases ; in 1827, 53 cases ; then it declined ; so that in 1828 I had but 27 cases ; and in 1829 I had only 3. If we had accurate accounts we should perhaps find that when ague increased in former times the average heat was higher than usual, just as it has been observed by Dr. McMichael during the last few years that ague is prevalent again.

Tendency of Malaria to give an intermittent character to other diseases.—Persons who have once had ague, or have it upon them, are much disposed to exhibit a more or less aguish form of any other disease under which they may labour—that is to say, when a person is the subject of ague, it is very common for diseases to become more or less intermittent, and this is especially noticed in aguish countries. Rheumatism, ophthalmia, vomiting, and purging, may all be intermittent, and even periodical. However, it is quite certain, that independent of malaria many diseases will become intermittent. Insanity is sometimes intermittent. I have seen a person mad once a-year, or once in three years. Palsy is likewise occasionally intermittent, and in one case I saw it periodical ; it came at a particular hour in the day, but the man to whom I refer had had ague many years before. Dr. Macculloch, however, ascribes a vast number of diseases to malaria, but there cannot be a doubt, I think, in any one's mind but his own, that he has carried his ideas to an extravagant pitch. He has done great good by attracting the attention of persons to malaria more than it was before, and his writings are very powerful and learned ; but I think no one can read his publications without perceiving that he had one idea constantly before him, which was malaria ; and he ascribes infinitely more to it than reason can justify. However, notwithstanding I am satisfied of the extravagant length to which he pursues his ideas, yet I am equally satisfied that too often we do not ascribe diseases to malaria when they arise from that source. We frequently meet with sporadic cases even of ague, which cannot be easily traced to

malaria ; but I have no doubt that if we could ascertain all the circumstances we should find that the individual has a striking tendency to it, and has been exposed to malaria by passing through a market, or some other place, in which there was vegetable matter in a state of decay. It is also to be remembered, that though the influence of malaria is so great, yet cold and wet, and other causes of debility, will induce ague without the reapplication of malaria when a person has once had it. Sometimes, too, when malaria has been applied, the disease does not occur till such circumstances as these have taken place. I mentioned in the introductory lectures that the predisposing will sometimes become the exciting cause. The exciting cause of ague is unquestionably malaria, but sometimes it will only act as a predisposing cause ; it will lie dormant in the system, and it is the circumstance of getting wet through that really is the exciting cause. From all the circumstances, I am convinced that many cases are aguish, where there is no suspicion of their true nature.

Proximate cause of Ague.—The proximate cause of ague—the real essence of that state which is always present when the symptoms occur—is unknown : we can say certainly it is a peculiar state ; for a person, in the intermission of ague, is perfectly well. There is frequently no disease to be found, whether inflammatory or any thing else, of any particular organ. Again, ague frequently cannot be removed by antiphlogistic measures, though it certainly can occasionally, or by any thing, in fact, which makes an impression on the system. Again, inflammatory diseases are not cured by sulphate of quinine, and remedies of that description. The disease may be inflammatory, and often is so, and requires antiphlogistic measures ; but its essence is not inflammatory—at least I know no proof of it. It may be inflammatory, but there must be something more than inflammation.

It is impossible to say how the measures cure it which are specifics in the disease—as, for instance, bark. It cannot be by curing inflammation ; neither can it be by irritating the stomach. Some have affirmed that bark only acts by irritating the stomach ; but if that were the case, brandy and cayenne pepper would do just as well, and so would corrosive sublimate. Nor can I conceive that specifics operate by strengthening the patient. We might give a person full diet, and take care that he has regularly exercise, and improve his strength in every way we can, yet we should not cure his ague ; whereas a few grains of quinine—a quantity too small to strengthen the body directly—continually puts a stop to it. I cannot pretend to say how it is that remedies act in curing the disease, but of this I

am satisfied, that is not what people pretend it to be—this, that, and the other thing. The remedies do not cure it, as people say, by strengthening, or stimulating, or any similar operation. We can only say there is a peculiar state, and remedies cure it by a peculiar operation. This is, I acknowledge, saying nothing; but, at any rate, it is not advancing unfounded hypotheses, and fancying oneself wise, when one is ignorant.

Arsenic, which cures ague very well, is not a tonic; it will debilitate the body, cause it to tremble, and depress the powers of the system, and yet a small quantity will stop the disease. Why this sort of remedies is called tonics, I do not know.

Diagnosis.—As to the diagnosis, we sometimes have a little difficulty in distinguishing ague from hectic. Hectic is a disease which continues for a length of time, and so may ague. It is called *hectic*, from being placed in the habit—being a continued disease—that is to say, lasting for a length of time. It also resembles ague in this respect,—it is more or less intermittent. In hectic, however, the rigors are irregular; all the stages, too, are irregular—each occurs in the most irregular manner; one comes before the other, without any sort of order. After a time, in hectic there are no rigors—nothing but sweating; constant feverishness; the pulse constantly quick; and this feverishness is excited by taking food, and by the slightest quantity of nourishment. In hectic there is generally profuse sweating on dropping asleep; the moment the patient drops asleep, some part of the body is in a sweat. In ague, when the sweating is over the person is comparatively well; but in hectic, so far from being better, the patient feels weaker. In hectic there is a red patch on the face, the palms of the hands, and the soles of the feet; the tongue is moist, and generally red; in the urine there is not the lateritious, red, brick-dust sediment of fever, but a pink deposit. In hectic there is not the exposure to the cause of ague, and there is not the aguish face, that peculiar cast of countenance which is so often seen in ague. In hectic there is frequently a local disease, suppuration in some part of the body; and there is an exacerbation at night, and frequently at noon.

Now no one of these symptoms alone will enable you to distinguish the disease, because ague is sometimes irregular, recurs at no certain time, and is, perhaps, even almost remittent; so that you may have to make the distinction between intermittent and remittent fever. In ague you may have sometimes profuse sweating and little else; it is therefore necessary, in making a diagnosis, to take all these circumstances into consideration; and if you do, you will, in ninety-nine cases out of a hundred, be correct. Now and then, however, there may

be a little difficulty. Sometimes you have ague and hectic together. I have seen hectic, of course, in a person labouring under phthisis, where there were rigors, increased heat, and sweating, from ague, and increased heat and sweating from the suppuration—so that really the case was one of difficulty; and there was the aguish face at the same time, so that it would have been difficult to make the distinction between the diseases, only that I knew there was suppuration in the lungs. The aguish face, and the circumstance of having been exposed to the cause of ague, shewed that ague was probably present, but the suppuration also shewed that hectic fever was present. If you take pains with your cases, you rarely can be deceived; now and then there will be a difficulty, but that will occur in instances where the hectic fever is attended by great rigors, and where, on the other hand, ague is assuming a remittent, rather than an intermittent character, and the patient has a local affection. You must also remember that rigors will occur without ague or hectic. Many persons, from mere nervous debility, will experience rigors; many persons will not be able to do their business, and their teeth will chatter, from real depression of mind. Again, persons with a stricture are subject to rigors; some persons always have rigors when a bougie is passed, and many without that, simply from a stricture in the urethra, will experience the same. If you find the rigors come on suddenly, and there is a stricture in the urethra, you ought to doubt whether there is ague. When rigors occur from stricture, or other disease of the urethra, there is generally no heat, no sweating; neither is there the aguish countenance, nor can you ascertain that the individual has been exposed to malaria. If, however, you still have doubt, after all your inquiries, it is best in all cases to give the remedies for ague; they can do no harm, if they be properly managed; at any rate the sulphate of quinine can do no harm.

The diagnosis of intermittent from remittent fever, is an easy matter—it is a mere circumstance of degree. If there be a perfect intermission, we call it "ague;" if it be imperfect, we call it "remittent fever."

Prognosis.—The prognosis in ague, in this country, is almost always favourable; you may almost always promise a speedy cure. If there be conjoined with the ague a local structural disease, of course that part of the prognosis must stand on its own foundation; but the ague itself you may generally cure. If its type be quartan, you cannot promise so speedy a cure as if it be tertian or quotidian; and I believe you are placed under the same restrictions if the disease be autumnal and not vernal. An autumnal ague does not yield so readily as a vernal; and, indeed, the lat-

ter usually yields so easily, that it often ceases spontaneously, after a few paroxysms; some think, after seven. It generally ceases when the warm weather comes in, and will frequently do so on a person changing his residence. You will recollect, that it is the ague of spring, and not of autumn, that is fit for a king. There is a contrary proverb for autumnal ague:—

“*Febris autumnalis
Est longa aut lethalis.*”

In hot climates, and even here, many intermittent fevers become remittent; and from being remittent they will become continued, without vigorous measures are adopted, and they may rapidly prove fatal by congestion of the head, thorax, or abdomen. It is in hot countries, and in hot weather in this country, that we have to give a guarded prognosis. In hot countries it is common for patients to die early in the attack, in consequence of the enormous congestion occurring in the head, chest, or abdomen. Now and then this may be the case here, from some peculiarity in an epidemic; we have always, therefore, to consider, in giving a prognosis, whether there is any peculiar form of ague present; and if that be the case, and the disease is sometimes fatal, our prognosis must be more or less guarded, although, without that, we should give the usual prognosis. We have no idea in this country of what aguish fever, remittent or intermittent, is in hot climates. In Italy, these are called pernicious fevers; for as soon as a person is seized he may fall into a comatose state, from which he never recovers; and on inspection after death, the greatest degree of congestion is found in the lungs and head, and also in the abdominal viscera. In this country, however, we may almost always promise, not only a cure, but a speedy cure; and that by means not at all severe, *Tuto, cito, et jucunde.*

At the next lecture, I shall commence with the treatment of this disease.

CLINICAL LECTURE

ON

HYDROCELE AND HÆMATOCELE,

Delivered at St. George's Hospital,

By B. C. BRODIE, Esq.

GENTLEMEN,

I INTENDED to have given a lecture on a case of aneurism now in the hospital, in which the external iliac artery has been tied, but I shall postpone its consideration till the next week. At present I shall offer you some remarks on another case of great

interest, and probably to you of more importance, as it is one which you may be frequently called upon to treat in practice.

John Neighbour, *ætat.* 46, a farmer's labourer at Uxbridge, was admitted into the hospital on the 13th of February, 1832. The report of the case at that time is as follows:—

“The scrotum is considerably distended, forming a tumor capable of containing about a pint of fluid. It is oval in form, and fluctuates distinctly. The tunica vaginalis appears thickened, but the testicle can be felt at the back part. The skin of the scrotum is rather tense and shining, and two or three large veins are seen ramifying over its surface. The tumor, when held before a strong light, is perfectly opaque. Some swelling exists about the cord, dependent on an inguinal hernia. The patient is of an unhealthy aspect, and exceedingly irritable temperament. Pulse and tongue natural; bowels open.”

The appearances here were exactly the same as in common hydrocele, except that the transparency was deficient.

In common hydrocele the tumor is always more or less transparent when placed between you and a candle.

When I was a student, some of my teachers used to deprecate the mode of investigating hydrocele by placing a candle behind it, but from it, nevertheless, you obtain much important information. You learn the situation of the testicle, whether it be on the fore or back part of the tumor, and you ascertain the course of any large vessel. You may detect in some cases a fungus growing from the testicle into the tunica vaginalis. I have known cases in which fungoid disease of the testicle has been mistaken and punctured by persons who ridicule this mode of examination, and who might have avoided such an error, if they had resorted to it. In general a hydrocele occurs without inflammation, but sometimes inflammation is the precursor of the disease, in which case the tunica vaginalis becomes thickened. The transparency of a hydrocele is thus often materially diminished, but not so as to render it completely opaque. This circumstance may, however, occur from the admixture of blood with the fluid contained. Let us now proceed to the history of the case before us.

Two years ago, the patient first noticed that there was some enlargement about the right testicle. Its occurrence he could not account for, either by strain or injury. The swelling increased, and in about ten months he was tapped, when a pint of clear fluid was drawn off. Nine months ago the operation was repeated, and again on the first of January last. On this occasion some considerable bleeding took place from the puncture made by the trocar, which was stopped by applying a compress of linen or lint. Since

that time the tumor has regained its original size, but it produces little or no inconvenience except from its bulk.

February 7.—It was determined that the fluid should be drawn off, and the cyst injected, if the case proved a proper one for that mode of treatment. The trochar and canula were introduced in the usual manner; and on withdrawing the former, a stream of dark-coloured fluid, apparently serum, mixed with blood and coagula, followed to the extent of a pint. Some warm water was now injected, which was retained a few minutes, and then drawn off: this was discoloured by the solution of grumous blood. The patient complained of some pain in the back during the operation, and was immediately put to bed."

The fluid drawn off on former occasions was clear, but bleeding had taken place from a vessel externally after the last operation, and it is probable that the same thing happened internally into the tunica vaginalis. I had thoughts of injecting the cavity with port wine and water, but when I saw the nature of the fluid which escaped, I was fearful of producing too great a degree of inflammation. Here, you perceive, was a case of hydrocele complicated with hæmatocele. I shall in the remaining part of the lecture confine my observations to the circumstances under which hæmatocele occurs, and the treatment necessary for its cure.

Hæmatocele most frequently occurs in combination with hydrocele, the blood being mixed with the fluid contained in the tunica vaginalis, and partially dissolved in it. If the quantity of blood be small, which is most frequently the case, the solution is complete; but when otherwise, as in the case before us, coagula are formed, which remain undissolved. The causes of the disease are as follow:—

1st. A vessel maybe wounded by the introduction of a trochar, as in the present case.

2d. The testicle in general escapes mechanical violence, by its mobility; but when hydrocele is present, there is a tumor, which is much exposed to injury, and a blow often produces rupture of a vessel which bleeds into the tunica vaginalis, and in this case the effusion is much greater than when it occurs from puncture with a trochar. I recollect a case where about a pint of blood was thus extravasated within two or three hours after the receipt of a blow.

3d. But rupture of a vessel may arise from a diseased condition of the arteries. This is analogous to that which occurs in the brain under the form of sanguineous apoplexy. There is a natural change which takes place in the arteries of old persons; they become ossified in patches, and ulcerate, and I have known this to lay the foundation of a hæmatocele. In these persons

also you have extravasations of blood into the subcutaneous cellular membrane, and this more especially in the lower extremities. After a time, the blood thus poured out becomes, in appearance, very much like coffee-grounds.

But hæmatocele may occur independently of hydrocele, by a direct blow or injury inflicted on the testicle; the tunica vaginalis being distended with blood alone. A gentleman was riding on horseback, and struck his testicle violently against the pommel of the saddle. The testicle became immediately swollen to more than double its natural size, the tunica vaginalis being distended with blood.

A negro died in this hospital some years ago, who had an enlargement of one testicle, but it was not known during life, as it afforded no inconvenience. I examined the part, however, and found a very large quantity of grumous blood in the tunica vaginalis, and at the back part a soft pulpy mass was seen, not at all resembling in structure the testicle, and only recognizable as such by its connexion with the epididymis and vas deferens. There is a similar specimen in the museum of the hospital.

I conceive that the absorption of the testicle took place, in this instance, from the pressure produced by the sudden effusion of a large quantity of blood; the tunica vaginalis not having been previously dilated by the existence of a hydrocele.

I saw another case, where the extravasation of blood was so great that the tunica vaginalis was ruptured, and the blood escaped in large quantity into the cellular membrane of the scrotum.

Let me here allude to a circumstance which various surgical writers have described as hæmatocele: I mean the rupture of a vessel in the cellular membrane of the scrotum.

The tissue here is exceedingly loose, as in the eyelids, and slight injury will rupture vessels and produce *ecchymosis*; but the name of hæmatocele is here improperly applied.

I have occasionally seen some very serious symptoms follow the sudden occurrence of a hæmatocele. The following case is an example. A man (a painter) who was in the habit of drinking to excess, while climbing a ladder was seized with pain in the testicle, and an enormous hæmatocele was the consequence. He came into the hospital with symptoms more nearly allied to mania than delirium; he was treated accordingly, and in a few days they subsided.

The treatment of hæmatocele must depend, in a great measure, on the circumstances of the case. I shall first describe to you the means to be employed when hæmatocele alone occurs, and subsequently that which is proper when it is combined with hydrocele.

If the extravasation of blood into the tu-

nica vaginalis be small, I should recommend you to keep the patient quiet for a time, applying cold lotion, but nothing more; and the blood will eventually become absorbed. I have already mentioned a case where a gentleman received a blow while riding. There was some blood in the tunica vaginalis, and the testicle could be felt at the back part; I advised him to allow the disease to run its own course; in about six months absorption had taken place to some extent, but the tumor, after this, remained stationary for a considerable time. He begged of me to perform an operation for his cure, and a day was fixed, but business called him from town. I saw him in about a year from the appointed time for the operation, and there was but a very small portion of the coagulum remaining, from which he experienced no inconvenience. In this case, had the extravasation of blood been great, I should not have waited for the natural cure, as the testicle might have become absorbed. You may infer from these observations that when the blood effused is small in quantity, no interference is necessary; but if, on the contrary, it be large, you had better lay open the tunica vaginalis, as I shall presently describe.

Should both hydrocele and hæmatocele exist, from whatever cause, if the quantity of blood be ever so small, it is sufficient to deter me from injecting the cavity with stimulating fluids, as in common hydrocele. If, on drawing off the contents of the sac, I find that the fluid is tinged with blood, I allow the fluid again to collect, and repeat this operation at certain intervals until it is voided perfectly clear. You may then perform the common operation for hydrocele by injection, and with success. Should the quantity of blood effused be large, you may wait in vain for its absorption; and the best plan to be adopted, whether such effusion takes place combined or not with hydrocele, is to perform an operation similar to the old operation for hydrocele by incision. You make a puncture with a lancet through the skin into the tunica vaginalis, introduce a director into the opening, and enlarge it by means of a scalpel; cutting, however, on the director, or from within outwards, and allow the contents of the tunica vaginalis to escape. It used formerly to be the practice to dust the surface of the tunica vaginalis with flour, by way of ensuring a sufficient degree of inflammation. The danger, however, is in producing too much, rather than too little inflammatory action. The exposure of the internal surface of the sac appears to be quite stimulating enough to produce all the inflammation that is wanted.

A piece of lint should be placed between the edges of the wound, to prevent union by the first intention.

I have known some rather severe symp-

toms follow this operation of exposing the cavity of the tunica vaginalis by incision; and I have understood that formerly, when it was performed more frequently, being the common method of curing a hydrocele before that by injection was introduced, it occasionally proved fatal. Nor is this surprising, when you consider how large is the surface which is sometimes exposed. Considerable inflammation may take place, even though the sac be but of small size. You have a full, bounding, frequent pulse, hot skin, and loaded tongue, together with suffusion about the eyes, and great pain in the head, with other marks of determination of blood to the brain. Under antiphlogistic treatment these symptoms generally, in the course of a few days, subside.

In the case now under consideration I made an experiment. On finding the fluid drawn off contained a quantity of blood, I injected the tunica vaginalis with tepid water, and repeated the operation twice, with a view to dissolve the coagula contained in the sac. By these means I imagined that the serum which would be subsequently effused might remain clear, and that I should gain time, by being able to resort to the use of a stimulating injection sooner than if the coagula had been removed by solution in the serum effused from the cavity of the tunica vaginalis.

On the 20th of February, three days after the operation, the report is as follows:—

“Scrotum generally red, much swollen,—swelling being chiefly produced by effusion into the tunica vaginalis; there is much pain in the part on pressure; no headache or sickness; countenance rather anxious; pulse frequent; tongue loaded and white; skin rather warm. An incision being made into the anterior part of the tunica vaginalis, a considerable quantity of dark, turbid, bloody serum, with sulphuretted hydrogen, producing an offensive smell, escaped. The interior of the cavity being exposed, shewed lymph deposited on its surface, with great vascularity.

Lint between the edges of the wound; poultice over the whole.”

The symptoms were referrible to a collection of putrid fluid in the sac; this produces the same symptoms in cases of urinary abscess, and under whatever other circumstances it may occur; and you should always endeavour to evacuate it, so as to rid the system of the offensive matter.

Sulphuretted hydrogen is in this instance generated, and it is no wonder that it should produce so much constitutional derangement, when it is known that one part of this gas, diluted with 500 parts of common air, is sufficient, if respired, to destroy life.

The patient's symptoms were relieved im-

mediately after the operation, and the wound is now suppurating and granulating, being the usual method of cure after the operation of incision.

TETANUS AND HYSTERIA.

To the Editor of the London Medical Gazette.

Cambridge, February 27, 1832.

SIR,

I TAKE the liberty of transmitting to you the reports of two cases which occurred last year at Addenbrooke's Hospital; and which, I think, are not devoid of interest. Should you not be overburthened by contributions, and should you deem them deserving of being recorded in the Medical Gazette, you would oblige me by inserting them.

I am, sir,

Yours most obediently,

HENRY J. H. BOND, M.D.

Physician to Addenrooke's Hospital.

CASE I.—*Tetanus, occurring after an Injury to the Spine, and continuing to recur at each successive period of Menstruation—Recovery.*

Leah Wheatley, æt. 25, unmarried, of a florid complexion, moderately robust, possessed of considerable strength of mind, and of a patient, cheerful disposition, came under my care on the 6th of February 1831. The following was the account she gave of herself.

She had enjoyed habitually good health till July, 1830, when, her foot having slipped on going down stairs, she fell with violence on the sacrum. On being raised she felt giddy, and had pain in the occiput. Some hours afterwards, she was seized with violent pain and spasms affecting the trunk, which, she assured me, drew her backwards in the form of a bow; having, at the same time, spasms of the muscles of the jaw. This attack lasted about a quarter of an hour, and on the following day she had two or three similar seizures. Four days from the occurrence of the accident she became insensible to her state, and remained so for a fortnight, during which time she had frequent returns of the tetanic affection, accompanied

with hæmoptysis. Blisters were applied repeatedly to her head. For about ten days after this, she was so much recovered as to be able to walk, but continued to experience a tightness across the sacrum, with some impairment of the power of motion, and some numbness in the lower extremities, especially in the left. In about a month, however, from the occurrence of the accident, she was again seized with several tetanic attacks in succession, similar in most respects to the former. These attacks had continued to recur monthly, with tolerable precision, up to the time she came under my notice—viz. from July 1830, to February 1831. They always began a day or two previous to the menstrual period, and continued throughout its duration. The menstruation, prior to the accident, had been normal, both in point of time and quantity, but subsequently had always been imperfect. In the intervals between the menstrual periods, she enjoyed comparative ease; suffering, however, occasionally from slighter spasms in the back and face, aching pains in the parts so affected, and having numbness of the lower extremities: this latter symptom was always more marked for some days after the tetanic period, but gradually decreased till its next occurrence.

The treatment up to this time (February 1831) had been various; the whole class of tonics and antispasmodics had been exhausted; cupping had latterly been several times had recourse to in the neighbourhood of the spine; but the attacks had not abated in violence. The following is the report of the case from the time of her being under my care.

February 6th, Noon.—Is in great apprehension of the recurrence of her spasms. Complains of acute pain along the vertebral column, with occasional twitchings between the shoulders, and slight retractions of the head; great tenderness on pressure over the spinous processes, from about the third dorsal vertebra to the extremity of the sacrum. Pain also in the hypogastrium, and a sensation as if a broad band were tied round the loins. Pulse quick; face flushed, and anxious; tongue quite natural. It is just a month since her last attack.

Applicentur Hirud. xii. labiis externis et sumat. Ant. Tart. gr. ʒ ex Aq. Dist. om. semihorio. Baln. coxend.

8 P.M.—The twitchings of the muscles are less frequent, and the pains much abated. Pulse 84. Two dark evacuations from the bowels. (This, she says, has always been the case during her attacks.) The medicine has caused retching.

Contin. Med.

February 7th, Noon.—Continues much as on last report; several hard and red lumps in the back, apparently partial contractions of the muscles. A slight colourless discharge has appeared from the vagina, with forcing pains.

7 P.M.—Has had three or four complete tetanic attacks. The following is the train of symptoms they presented. At first, pain in the muscles of the back and neck, with an indescribable sensation at the extremities of the fingers; the breath then became snorting, the arms semiflexed and stiff, and the hands reversed. The head was presently retracted, and next the trunk gradually in some instances, suddenly, by a single jerk, in others) rose into the form of a bow; the toes and ankles bent in the same direction, the occiput and heels alone resting on the bed. The lower jaw, by this time, was firmly closed, the eyes affected with strabismus, the face intensely flushed, and covered with profuse sweat. This state continued for three or four minutes, the patient remaining sensible, but in evident agony, though silent, from the spasm of the muscles of the neck. To this state succeeded a tremulousness of the whole body; the muscles then gradually relaxed, but continued affected with intense pain, which lasted for some time longer, but was at length followed by a short but complete repose, resembling that in the intervals of labour-pains; four fits returned within the hour; one, less violent, came on quietly and almost imperceptibly, not preceded, as were the others, by a sense of suffocation. The nurse called this "one of her still fits." During these attacks, or rather in the intervals, she had taken forty drops of laudanum, and a tobacco injection had been administered. In the course of two or three hours, she had become quite free from any spasmodic affection, and expressed herself as being quite comfortable. She had vomited several times.

Tinc. Opii, ℥xx. were again given her.

8th.—Some symptoms premonitory of a return of tetanus having appeared in the morning, an ounce of Ol. Ric. was given, and the following bolus ordered to be taken soon after its operation.

Moschi, gr. xv. Camph. gr. v. Conf.
Rosæ, q. s.

In the afternoon had a tetanic attack assuming a different form, which lasted an hour and a half, affecting principally the diaphragm and the abdominal muscles. A soap clyster was given during the spasm. On the subsidence of the attack, there was continued sickness and vomiting.

9th.—Another fit in the night; none during to-day. Menstruation is established, but scantily.

10th.—Severe vomiting, but no tetanus. Bowels freely open; has had an opiate. Menstruation more abundant.

11th.—Return of the spasms, affecting chiefly the head and arms; delirium; head hot, and face flushed; stools dark and lumpy.

Abrad. Capillit. et applic. lot. frig. Cucurb. cruent. temporibus. Calom. gr. v. Pulv. Jalapæ, ℥j. stat. sum.

12th.—Delirium continued throughout the night, but she is now sensible. Menstruation has ceased.

13th.—The present menstrual and tetanic period having now apparently elapsed, a plan of treatment was adopted with a view to prevent, if possible, the recurrence of the attack, or abate its severity. A strict regimen was enforced, and, between the 13th and 20th of the month, blood was repeatedly drawn by cupping from the neighbourhood of the spine; the bowels were kept freely open by a senna draught, and on the 20th two issues were made in the loins, on each side of the vertebral column. Up to the 28th she continued much in the same state as she usually had been in the intervals of the attacks, complaining generally of heat and pain along the course of the spine; and the tenderness being greatest over the spinous processes of the second and third lumbar, and fourth and fifth dorsal vertebrae.

On the 28th of February the tetanic attacks returned, and continued more or less till the 5th of March, but were less severe than on their previous occurrence. On this occasion the spasms

affected principally the muscles in the front of the trunk, and were attended with an enormously tympanitic state of the abdomen, and vomiting. The stools were, as usual, dark and lumpy. There was likewise again delirium. The menstruation was very scanty, there being scarcely any sanguineous discharge. The remedies employed during this attack, were purging by calomel and ol. ricini, terebinthinate clysters, and an embrocation frequently applied to the lower extremities, consisting of equal parts of ol. olive and ol. terebinth. These latter remedies appeared to have considerable control over the violence of the spasms.

The attack having entirely subsided by the 6th of March, the same preventive treatment was persisted in as previously. The issues now discharged freely. Cupping was repeated when she suffered most from pain and heat in the back. The terebinthinate liniment was applied when the numbness and twitchings threatened to increase, and the patient herself thought that these were controlled by the application. The horizontal posture was mostly maintained, and the diet vegetable.

There being some symptoms, by the 15th and 16th of March, of a return of menstruation, leeches were twice applied to the external labia. She suffered from spasms on the 17th, with nausea and vomiting, but had no regular attack of tetanus. By the 18th the menstrual discharge was established in a larger quantity than on former occasions; a spontaneous purging occurred, which seemed to cause great relief.

The same remedies were applied as at the last period, with the addition of the hip-bath. The setons dried up completely during the prevalence of the spasms, but discharged again spontaneously on their subsidence. During the remainder of March, and to about the middle of April, cupping, the issues, the terebinthinate liniment, and mild purgatives, were continued, and with apparent benefit. Any violation of regimen was always followed by increased pain and heat in the back, and threatened spasms, which were reduced by cupping.

In April, the menstruation returned with its usual accompaniment of spasms, but in a still more mitigated degree. Leeches had been applied previously, to the labia, as on former occa-

sions, and the same treatment employed during the attack.

Fresh issues were made in the back in the beginning of May. The two next menstrual periods were passed over with still less suffering, and in the meantime the pain and tenderness in the course of the spine, the numbness of the lower extremities, with the impairment of their power of motion, and the twitchings of the muscles, had all greatly abated, though liable from time to time (especially by any imprudence) to become aggravated. Strict regimen and diet was still enforced, with some abatement of rigour. By the end of June she was so far recovered as to be able to walk in the ward without assistance, and had passed over a menstrual period with but very trifling indisposition. In the beginning of July she was removed to her home, some distance in the country, with strict injunctions to pursue the same plan, and to maintain a moderate discharge from the issues.

I saw her in the autumn, when she had had no return of the spasms, and complained only of some weakness in the lower extremities; and the menstrual function had been entirely re-established without any morbid accompaniment. She presented herself at the hospital in December last, in the enjoyment of perfect health.

CASE II.—*Hysteria complicated with Paraplegia—Recovery.*

Mary Dockley, æt. 19, of a florid complexion, was admitted under my care March 1st, 1831. No very precise account of the previous history of her illness could be ascertained, but the following appeared to be its outline:—

She had been ill about two years, having been confined to bed the greater part of that time. During the first year of her illness, she had suffered chiefly from hysterical fits, attended frequently with hæmoptysis. The latter symptom had recurred at irregular intervals of a month, six, eight, or sixteen weeks. Twelve months previous to her admission she menstruated for the first time, which was coincident with a recurrence of hæmoptysis, but she had never menstruated since.

During the last twelve months, in addition to the above symptoms, she had been paraplegic. No very distinct ac-

count was given of the mode in which this came on; nor whether it was gradual or sudden. She had taken a large quantity of calomel.

On admission into the hospital her state was as follows:—A total inability of moving the feet, which were extended by gravitation, as she lay in bed, nearly in the same line with the legs. She could with difficulty produce a semiflexion of the knees. Total insensibility of the left leg, and to a considerable extent above the knee; the right leg nearly as insensible; both extremities chilly and damp, while the rest of the body was warm, and covered with perspiration. Nothing morbid could be detected on an examination of the spine. Respiration very quick and short, with cough, and scanty mucous expectoration; the respiratory sound in left lung obscure, and accompanied by a slight crepitation, especially behind. In the right lung respiratory sound loud and clear; pulse generally above 100; heart's impulse in excess; tongue rather white; a total disrelish for all food; fullness of the abdomen, with rather a confined habit of the bowels; extreme dejection; almost total inability, or indisposition to speak; intractableness and perverseness; every evening a violent attack of hysterical convulsion, producing great distortions of the upper half of the body, whilst the lower remained in its habitually motionless state, attended apparently with temporary blindness; the paroxysm was accompanied not unfrequently with delirium, and lasted the greater part of the night. She seldom appeared to get any sleep.

During the whole of March she continued much in the same condition. The paralysis of the lower extremities remained as complete; the temperature, however, was rather more equally distributed; the abdominal muscles seemed almost inactive, and occasionally the urine was passed involuntarily. She would frequently abstain a whole day from taking a morsel of food, and during one fortnight she could be prevailed on to take no more than was barely sufficient to prevent inanition. At the end of March, however, the pulmonary symptoms had become mitigated. She had never expectorated blood from the time of her admission. The dyspnoea was now urgent only during the hysterical paroxysms; these began slightly

to abate, and her mind was somewhat more composed.

The external remedies employed during this period, either simultaneously, or in succession, were—

Cucurb. Cruent. sacro, nuchæ, et inter scapulas.

Hirud. Capiti. urgente delirio.

Liniment. Camph. comp. vel Linim.

Terebinth. cruribus e: dorso. Em-

plast. Lyttæ lumbis. And afterwards, Setaceum collo.

The internal remedies were—

Pil. Aloes c. Hydrarg. Mist. Menthæ

Sulphuric. c. Magnes. Sulphat. p. r. n.

Mist. Ammon. fœtid. Haust. ex Træ.

Valer. Træ. Castor. Ext. Hyosci.

comp. or Haust. ex Træ. Opii. urgente

hysteriâ. And, latterly, Decoct. Cin-

chon. et Ferri Subcarb.

During the first half of April there was but little alteration, and the treatment was confined principally to the seton, cupping, and the use of the stimulating liniment; and to a trial of hydrocyanic acid, in gradually increased doses. But from the middle of April to the middle of May, her general health greatly improved; her "moral" was quite changed; from being morose and intractable, she became cheerful and patient; her appetite was good, and her *enbonpoint* increased; the hysterical paroxysms were much abated, and she rested tolerably; but the paraplegia remained unchanged. The pulmonary symptoms had likewise nearly disappeared. During this period the treatment consisted in giving the decoct. cinchonæ, or quin. sulph. and an opium pill at night, and in the use of the shower-bath. The seton was allowed to heal up.

She was now ordered to be drawn in a chair daily in the garden of the hospital. Her general health being so much improved, the remedies were more exclusively addressed to the paraplegia, the bark and shower-bath, and stimulating liniments, being at the same time continued. Strychnine pills were given her, in gradually increased doses, till she took about a grain daily. This was persisted in to the second or third week in June, but the paraplegia scarcely, if in any degree, abated. Electricity was next tried, in addition to the other remedies. The shock was felt vividly above the paralytic parts, and excited

contractions, but no sensation in the lower extremities, although it was repeatedly applied with considerable intensity; but in a day or two from its first application she menstruated for the first time since she became paralytic.

No further improvement, worthy of notice, took place up to the middle of July, during the continuance of the same remedies. But from this time a diminution of the paraplegic state commenced, and continued progressive, and this improvement was simultaneous with the successive application of small blisters to different parts of the lower extremities, and the continuance of the administration of bark and the use of the shower-bath. The first return of sensation was experienced in her feeling the action of the blisters; soon after she began to have pains in the right leg, and various anomalous sensations in the same extremity. Early in August she was able to move the toes of the left foot, which was now sensible when pinched. In the course of the same month the right leg recovered still further its sensibility and power of motion; and by the middle of September she was sent into the country, having at that time a tolerable use of the right leg, the left still remaining powerless and insensible. On presenting herself in October at the hospital, the right leg was perfectly restored, and the left had in some measure recovered. In December last she again presented herself at the hospital nearly free from any paralysis, there remaining only a slight dragging of the left foot, and otherwise enjoying perfect health, without any remains of hysteria. The menstrual function had long since been quite established. She had continued the occasional use of small blisters to the extremities to the time of her recovery.

up persons of both sexes got it also, some of whom were above forty years of age, and in them it was often very severe: *infants*, however, seemed generally to enjoy the privilege of exemption from its attack, as if their systems were insusceptible of the morbid effect of the contagion. Of two families who fell under my particular observation, the youngest of each (both babies in arms) entirely escaped, although all the other children were attacked; and I know a great many other instances of similar exemptions. I myself did not see any child under a year old with the disease during the whole epidemic of 1830; no such case occurred amongst the military, and I have not been able to hear of more than three or four instances, in civil practice, where infants at the breast became affected:—they were all upwards of eight months old, and the disease in them was not of a severe nature.

This greater exemption of infants is a circumstance which I think has not been particularly noticed by writers on Scarlatina; but it is an important fact, which I have to point out for further observation. Neither Huxam nor Fothergill advert to it. Dr. Withering, in his observations on Scarlatina, as it appeared at Birmingham in 1778, states, that it seldom occurred in children under two years of age: and Dr. Sims, in his full account of this disease in London in 1786, mentions, that he saw but one child at the breast who had it, and that but slightly. I am inclined the more, however, to believe in the insusceptibility of infants to the Angina Exanthematica, as the same has been found to obtain in other contagious diseases; for example, Dr. Hennen mentions, on the authority of the hospital surgeon, Mr. Naudi, that in the plague at Malta, in 1813, several instances occurred in which children sucked their dying mothers without contracting the disorder from them; and Mr. Kite, of Gravesend, in his paper on small-pox, in the "Transactions of the Medical Society," states, that "he had occasion frequently to observe that very young children had been repeatedly inoculated, and for several weeks constantly exposed to the worst kind of small-pox, without any effect; that afterwards the measles became unusually rife, of a putrid nature, and much more contagious than he ever before or since observed it; that he at-

ON THE EXEMPTION OF INFANTS
FROM CONTAGION,
PARTICULARLY WITH REFERENCE TO
VACCINATION.

By — MURRAY, M.D.

Cape of Good Hope, August, 1831.

CHILDREN were chiefly the subjects of its attack (Scarlatina), but many grown-

tended in several families where the young infants (particularly when under two months) were the only part of the family that escaped the disease, although exposed a considerable time to the infectious air, and lying all the night close to other children passing through every stage of the complaint, and consequently inhaling into their lungs the very essence of infection; nay, he had been informed of more than one instance, where, in addition, the mother had the disease, and the child, although constantly in her arms, breathing the air reeking with putrid particles, and sucking the milk impregnated with the disease, strongly, as we should think, has for months withstood the infection."

From reflecting on this degree of insusceptibility of infants to the influence of contagion, I have been led to consider that it may perhaps be owing to the circumstance of persons being vaccinated in infancy, while their constitutions are but imperfectly susceptible of the action of the vaccine virus, that vaccination has been found not to prove a perfect preservative against the influence of small-pox contagion.

It is most usual to vaccinate children within three or four months after birth, with a view to protect them from small-pox during the rest of life; but the instances in which it has failed to afford perfect protection have of late years been so numerous in different countries, as to have caused considerable distrust in the minds of the public relative to the utility of the operation; and I believe there are few intelligent parents who do not now entertain more or less anxiety and apprehension about their children on this score, when they send them out into the world.

These failures of vaccination have been attempted to be accounted for by medical men in various ways, such as that the operation might have been improperly performed, or performed when the infant was labouring under some other eruptive disease, or that the course of the vaccine disease might have been interrupted by the pustules having been punctured or broken, or that the virus might have been of a spurious description, or that time had diminished the protecting virtue of vaccine, or that in certain cases of failure it might have been owing to some idiosyncrasy or peculiarity of constitution in the patient himself; but it must be

confessed that all these reasons are unsatisfactory and untenable in the majority of cases.

I do not know if the idea be new or not which I have here broached, that the failures may probably be attributable to the operation being performed at an age when the constitution is insusceptible of undergoing the regular process necessary to preserve it from the influence of variolous contagion, but from the circumstances I have just mentioned, I think it is likely to be as correct as any other I have yet known brought forward.

I have not an opportunity of ascertaining whether in the late variolous epidemics in Europe any difference was found in the anti-variolous protection afforded in the cases vaccinated at the age of a few months, and in those in whom the operation was not performed till after three or four years of age; but I feel anxious that this subject should be investigated, as it may be found to elucidate many of the obscure points connected with the subject of the protection in some, and the complete failures in other cases that have hitherto eluded explanation, and I the more hope that it may prove correct, as we should then be enabled to prevent failures hereafter. As connected with this point, I have further to add that I have known a strong effect to be excited in the constitution of adults from re-vaccination; and reasoning on this circumstance from analogy, we should suppose it could not happen if their system had undergone the proper vaccine action from the primary operation; and if it be granted, as I believe it now is, that there is no just ground for the opinion that the prophylactic virtue is altered or lost in the constitution after a certain time from the date of vaccination, then I think that my supposition is greatly strengthened by another circumstance, which is, that when revaccination has been instituted in adults, it has been found to preserve them more effectually from small-pox and its modifications; of this a good instance came within my own knowledge of a family in Scotland which escaped infection through this means when a variolous and varioloid disease was raging epidemically all around it.

Were my children to leave this colony for England, or indeed for any place where there was a chance of their being

exposed to small-pox, I should certainly have them all revaccinated, by which means I should consider them to be rendered more secure against the influence of its contagion.

ASPHYXIA.

To the Editor of the London Medical Gazette.

SIR,

IF you consider the following cases worth recording in your valuable journal, I shall feel obliged by their insertion.

I am, sir,
Your obedient servant,
HENRY EWEN.

Long Sutton, Lincolnshire,
March 13, 1832.

About ten o'clock in the morning of Monday, January 30th, I was called to visit two men at Sutton Bridge, who were supposed to be suffocated on board the brig *Economy*. I found them supported on chairs in the kitchen of the Bridge Hotel. They were completely insensible; their countenances were a livid purple; the extremities and surface generally cold; pulse weak, irregular, and frequent—scarcely to be distinguished; breathing stertorous; jaws firmly clenched.

I directed them to be placed in the recumbent posture on a mattress, before a good fire, the head and shoulders being raised, and supported by pillows. Bottles of hot water were placed on each side of their trunks and between the thighs; diligent frictions were applied to the extremities, and sinapisms to the soles of their feet. I next introduced an elastic tube into the stomach, and forcing open the jaws with the handle of a common table-fork, injected some hot brandy and water, the beneficial effect of which was obvious to all. After an hour this was repeated, when the pulse became stronger and regular, but was very frequent (120 in a minute). The face became of a natural colour, the lips being red; the breathing also was less laborious. One of the patients vomited freely after injecting the warm fluid into his stomach; the other had done so on being first brought out of the cabin on deck. The food ejected consisted of

meat, &c. *quite unchanged*, which had been taken for supper the preceding evening. Friction and the application of warmth were continued till the circulation of the blood was fully restored. Blood was abstracted from the arm *after* the hot brandy and water was exhibited, with a view of relieving the congestion of the brain and lungs. The patients were then placed in warm beds. One had an involuntary evacuation of fæces, the other retention of urine; the former shewed signs of returning sensibility towards evening, and took a pint of boiled sago. He was still in a state of stupor, but capable of being roused by speaking loud and sharply to him. The next morning this man was quite sensible, and only complained of extreme weakness, (being incapable of raising his head from the pillow), and of "great soreness" all over him. He progressively recovered; his pulse, in the course of twenty-four hours, returning to the natural standard. The other man recovered much more slowly; sensibility returning the following evening, not until thirty-six hours after he was removed from his perilous situation. It was necessary to introduce the catheter several times. I injected a camphor enema into the rectum, applied twelve leeches to his temples, and a blister to the nape of the neck. This man also was very weak for two or three days, and complained of "great soreness all over" him. They expressed themselves, on becoming sensible, utterly unconscious of any thing which had occurred from the time of getting into their hammocks on Sunday evening.

It may be right here to state, that the men had had a fire in the hold on the preceding day, which was left burning with the hatches fastened down.

SOME ADDITIONAL REMARKS

ON

PERMANENT SPASMODIC STRICTURE OF THE GREAT INTESTINE;

Its Appearances and Treatment.

By JOHN HOWSHIP,

Surgeon to the St. George's Infirmary, and
Teacher of Surgery.

IN a little essay upon spasmodic stricture in the colon, lately published, it

was my object to point out the occasional occurrence of spasmodic contraction, or stricture, in the great intestine; to shew that this stricture frequently gives rise to all the inconveniences induced by habitual confinement of the bowels; and to demonstrate that there is a mode of treatment by which this complaint may be ascertained, and by which it may also be removed.

I there ventured to state (what I believe to be true) that the operation of injecting a fluid into the great intestine, for the specific purpose of first determining the existence of spasmodic stricture, and then relieving and removing the contraction, is an entirely new proposition in surgery; and that although from what I have seen of its effects, I have the utmost confidence in its powers as a successful means, that confidence is as yet rather the effect of close, than the result of very extensive observation. I still continue, therefore, carefully to collect every new fact upon this subject; and hope that by this course, aided by the kind assistance of some of my professional friends, I may in a few years be enabled to bring the more important points regarding the symptoms, diagnosis, and treatment, into a comparatively distinct and satisfactory form.

That the bowels are occasionally subject to painful spasm, has been long known; but as the pain in such cases has made the strongest appeal to the feelings of the patient, and the attention of the practitioner, it has been generally, if not invariably believed, that the pain and the spasm were inseparable companions; a conclusion which I have found to be entirely at variance with the truth.

As to pain, presumably in the bowels, it has usually been considered impossible to come to any precise or clear conclusion with regard to its exact situation; but in the essay mentioned, it was shewn that where certain affections productive of uneasiness, pain, or costiveness, are situated in any part of the large intestine, the intelligent practitioner may always determine the affected part with precision; and that in the great majority of such cases, were these complaints attended to (as they ought to be) in their commencement, we should much less frequently see mere disorder induce disease.

I have there demonstrated, among other points, the astonishing extent to

which acute febrile excitement, with severe local pain, may go in hurrying the case on to a fatal conclusion, which no medical means can avert; when the simple expedient of distending and washing out the painfully contracted and irritable colon relieved, and at once removed all the symptoms in a manner scarcely to be credited. In short, the most complicated train of evils, connected as so many links of the same chain, are sometimes induced by complaints originating in the great intestine, exciting in the first instance either local pain, or permanent spasm, or perhaps both.

Without adverting further to what has been before advanced, I shall now mention some particulars of a case in which the seat of spasm (by the adoption of the appropriate treatment) was first deprived of its painful character, and then relaxed; although it is curious that it did not recover its susceptibility to the usual griping uneasiness excited in other parts of the bowel under the influence of distention. I shall, then, in conclusion, describe the peculiar characters of a preparation I lately made, demonstrative of the precise appearances of this form of stricture, as also its induced consequences during life.

CASE.—Aug. 3, 1830.—Mary Cape, æt. 33, had been a month under my care, in the St. George's Infirmary, for pain in the left side, with a sense of soreness. I applied a blister twice, and with benefit, while they remained open. She said that four years since she had been confined for seven weeks with nervous fever; and that the bowels, always regular before, had never since acted at all, except when she took medicines for that purpose. It appeared, however, from her account, that as long as nine years since she was kicked by her husband while pregnant. This injury was in the left side, between the lower ribs and margin of the ileum. A premature still-birth followed, and from that time she had occasionally felt an aching uneasy pain in the apparent seat of the sigmoid flexure of the colon. Since the nervous fever (she said) this pain, in the same part, became more constant, and more considerable, and she thought induced the habitual confinement of bowels which then followed.

It was, as she expressed it, an irksome aching pain, always increased by taking

a full breath; moving about also excited it, and exercise aggravated its severity, until at length she was obliged to stop.

7th.—To-day, for the first time, I injected the great intestine. About ten days since the pain in the left side very acute. I applied the second blister: it was kept open, but was now healed: it entirely relieved the pain. The woman was to-day flurried and frightened at the idea of an operation, and gave an indistinct account of her feelings. I visited her, however, in the evening, and then clearly ascertained that as the warm water flowed into the bowel, the first sensation felt was in the seat of the old pain, but it was not pain (she said) but a comfortable feeling of warmth, or “working,” in the part. As the quantity increased to fulness, the usual gripings came on in the other parts of the bowel, to a very painful degree, but not the least in the seat of the old pain. I inquired if it occurred to her at this time that the spot in which she formerly felt the pain was the same that now felt warm and comfortable. She said it was, and that at one moment she was about to mention it to me. Four pints of warm water were slowly injected, and retained half an hour; and the operation was repeated in the same manner every subsequent morning.

17th.—The operation was repeated daily, with precisely the same feelings as at first. On making the experiment, she now found herself able to take exercise more freely than before without increasing the pain in the side.

24th.—The great intestine was daily injected in this case for a fortnight, and the operation then discontinued. The bowels, left entirely to themselves, were now relieved daily without medicine, having recovered the power of spontaneous action; neither was there any return of pain in the side under exercise. Having thus lost her complaint, she was allowed to go out, and has since remained well.

July 22, 1830.—I examined the body of J. B. æt. 56, who had died in the Infirmary from chronic diarrhœa, supposed to have arisen from ulcerated bowels. He had been five months complaining, but only of irritation, and a constant tendency to looseness. At first his bowels were disturbed almost every hour, but by the assistance of medicines, with leeches and blisters to the

abdomen, he was relieved; so that latterly his bowels were moved only two or three times in the day.

There was no abdominal tenderness till a few days before his death, when seized with rigors. He next day complained of pain in the lower part of the right side, with shortness of breath, and febrile heat, &c. He was bled, leeches, and blistered, and ordered medicines, without relief, and in three or four days died.

On opening the body, the cause of death proved to be acute inflammation in the lower part of the chest and lungs, upon the right side. Extensive adhesion, with a copious effusion of serum and fibrinous matter, were found between the lungs, the diaphragm, and ribs. I found no ulceration, or other disease of the intestines. The whole of the colon, however, was considerably enlarged, down to a part of the rectum, about seven inches above the sphincter, where, for the extent of one and a half inches, it was so closely contracted as to be nearly impervious. It just allowed fluid feces, as I afterwards found, to flow through it, in a stream equal in diameter to a small goose-quill.

Very desirous to preserve this appearance, I took great care not to pull or disturb it. I therefore tied the rectum low down as possible, dividing it below the ligature. I placed a second ligature on the middle of the colon, dividing it above, and separating its attachments to the spine, lifted out the detached part of the intestine upon both my hands into a large basin of cold water.

I next opened the lower end, washed out its contents, filled it with a strong solution of alum, in water, and tied it up again. Without lifting it out of the water, the upper end was now opened, washed out, filled with proof spirit, and tied up; and then, having brought the glass, filled with rectified spirit, with its top sloping to the edge of the basin, I gently raised the preparation on my hands, allowing the lower part to pass first into the glass with as little disturbance as possible.

Having allowed it to remain ten days in spirits, it was again placed in water, the ends cut off, the contents washed clean out, and the preparation again suspended for a month in fresh spirit, which was afterwards changed several times.

This preparation eventually exhibited

a very clear and exact demonstration of what happens in permanent spasmodic contraction of the great intestine, the appearances in the parts remaining precisely as they were during life.

The internal opening of the stricture is seen brought into view by cutting away a part of the side of the intestine, admitting the passage of fluid matters only. The small aperture, exhibiting the mucous membrane puckered up into folds, by the contraction of the muscular fibres, surrounding it externally. The superior part of the bowel excessively distended, thin, and weak, from habitual and constant accumulation; the inferior portion only moderately and occasionally occupied, and of natural appearance and thickness.

Externally, the two longitudinal bands seen spread and dispersed over the enlarged part of the intestine above; moderately expanded over the part below, but so contracted as to be both drawn together into one very narrow band at the strictured part. This preparation, which is now in my collection, I shall be extremely happy to shew any gentleman who may feel desirous of seeing it.

21, Saville-Row, March 12, 1832.

CROTON OIL IN CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

IN answer to an inquiry of Dr. Bartlet, contained in your last (March 10) number of the Gazette, regarding the use of croton oil (*Tiglii Oleum Ph. Lond.*) in cholera morbus, I beg to state, that it has been employed by me in numberless cases of that appalling malady.

The disease, as it presented itself to my observation at the General Hospital in Calcutta, was one of such a rapid and severe character, as never to allow me to place dependence solely upon the effects of one remedy; the croton oil was, therefore, ordered by me as an adjunct to other active remedial agents.

As the application of any medicine to the cure of a disease by a medical practitioner, must depend upon the opinion entertained by him of the nature of such disease, I think it right to acquaint you with my reasons for the employment of

croton oil in cholera morbus. I consider, then, this disease one of a *purely inflammatory* character, however equivocal and revolting many of the symptoms may be which present themselves in its progress, and one in which all the secretions, but more particularly those of the liver, stomach, and intestines, are materially affected. Under this impression my endeavours are *invariably* directed to overcome such inflammatory action; and I have observed, that one of the *earliest and most certain* signs of safety, is the restoration of the secretion of these several organs to their natural state. I have *thus* used the croton oil, being a medicine active in its effects, easily administered, and capable of being taken when other remedies could not be swallowed, or would be rejected. I have given it in doses of from two to four drops, usually combined, in the form of pill, with calomel and compound extract of colocynth, or with the former medicine and opium, repeating the quantity in *one* or two hours, according to the *effect produced*; it very rarely augments the vomiting already present, but produces an abundant increase of the alvine evacuations.

I have much confidence in its powerful and remedial effects in this frightful and fatal complaint, and recommend it to the attention of any practitioner who may entertain the same ideas as myself of the inflammatory nature of cholera morbus.

I remain, sir,
Your obedient servant,
W. W. HEWETT,
Surgeon.

8, Pavilion Parade, Brighton,
March 15, 1832.

ON THE USE OF MURIATE OF SODA IN CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

PRESUMING that it is of consequence to make known any thing striking that comes under our observation concerning the terrible disease that is at present prevailing in the north of England, I have taken the liberty of sending an account of a case, shewing the efficacy of a remedy which I believe has not as yet been used in it to any extent—I

mean a *saturated solution of muriate of soda*.

I remain your obedient servant,
T. STEPHENS.

CASE.—J.—T—, aged 38, a pitman, a man of weak constitution, subject to dyspepsia, sometimes attended with diarrhoea. I was called to him on the 12th instant, about two A. M. found him vomiting and purging, with severe cramps in the extremities; the skin was very cold, and covered with a clammy perspiration—countenance sunk—lips and margins of eyes of a blue colour—eyes deeply drawn into the sockets—cheeks hollow—extreme prostration—tongue moist, of a bluish white—hands soddened and wrinkled, of leaden colour—pulse small, weak, and at intervals imperceptible; the evacuation from the bowels had the appearance of rice-water; that from the stomach consisted of transparent fluid; there was a constant craving for cold water; the voice a weak, husky whisper; great restlessness and jactitation: along with these symptoms, there were certain marks in the whole appearance of the patient which shewed the case to be a very unfavourable one.

Treatment.—Above ten ounces of blood was drawn from the arm, of a thick black appearance, without producing any effect on the pulse; after this, an emetic of mustard was given, which produced full vomiting; this raised the pulse a little, but it soon sunk again. The *muriate of soda* having been suggested to me as a remedy, I determined to try it in this case. I made a saturated solution of *common salt*, and gave a large table-spoonful every half hour. In addition to this, I took a slip of flannel, long enough to reach from the sternum to the pubes, dipped in a strong solution of camphor, and over this a flannel dipped in hot water.

Half-past seven, A. M.—On revisiting him, I found the effect of the salt was really surprising; reaction was now established; the pulse distinct, about 80; cramps much relieved; slight vomiting at intervals; only one or two watery evacuations passed; no urine as yet passed.

The solution directed to be continued, and pill to be given every hour, containing gr. v. of extract of colocynth, and gr. iss of calomel.

Two P. M.—Found him in a tranquil sleep, skin warm, vomiting and purging ceased, pulse strong, about 90; the hands had become smooth, and resumed their natural appearance; the countenance had also undergone a considerable change for the better.

Two P. M.—Still continuing better; passed some evacuations, containing faecal matter; urine passed pretty freely.

13th instant.—He is now so much better as to be considered convalescent.

North Shields, March 13, 1832.

ANALYSES & NOTICES OF BOOKS.

“L'Auteur se tue à allonger ce que le lecteur se tue à abrégé.”—D'ALEMBERT.

Reports of Medical Cases. By DR. BRIGHT. Price 9l. 9s.

[Concluded from page 480.]

THE following case will form the best possible transition to the interesting topic which succeeds it. Dr. Bright says that the resemblance in the case to what occurs in paralysis from lead, was so strong that he could not help questioning the correctness of the patient's account of himself.

Paralysis of the Hands, after frequent attacks of Obstruction of the Bowels.

“Mary Jessop, a woman of spare habit, aged 31, was admitted under my care into Guy's Hospital, June 9th, 1830. She is the wife of a soldier, and lay-in seven months ago with a fine child, which she now nurses. She says that during the last two years, before her confinement, she was subject to remarkable constipation, so that she was in that short period nine times under medical treatment for severe attacks of the kind: after her confinement, she first began to find an increasing weakness in both her hands, and they are now both precisely in the condition of those of a person affected with the paralysis from lead: she has no other ailments of which to complain, and she still continues to hold her child by means of her arms and in the bendings of her elbows.

“I blistered the nape of the neck, paid great attention to the bowels, and gave the Peruvian balsam, and after a few days had recourse to tepid bathing twice in the week. This was continued, with no good effect to the fifth of July, when she was ordered to take an ounce of the *mistura guaiaci* every

six hours, and to continue the bath. Upon the whole, she seemed rather to improve on this remedy; but as I discovered towards the end of the month that she had leucorrhœa, I ordered, besides the mixture, that she should take the extract of gentian with a grain of the sulphate of zinc three times a-day, increasing the zinc from time to time; and the compound alum wash was used locally. Under this treatment she decidedly improved, and her convalescence was assisted by bathing her hands in warm water in which mustard had been diffused; and when she left the hospital, in the beginning of October, she had very tolerable power in her hands, though they were still far from being completely restored."

Paralysis from Lead.

Any preface from us we conceive to be needless, before the following very judicious remarks. "We have abundant opportunity of observing the injurious effects produced by lead amongst the work-people in the white lead manufactories, and the house-painters of the metropolis; besides the occasional cases which arise from accidental exposure to this exciting cause of disease. There are two sets of morbid phenomena connected with this poisonous agency—the one, a state of constipation, probably arising from an approach to paralysis in the muscular fibres of the intestines, which is soon followed by the accumulation of scybala and flatus, and hence violent colic pains, with furred tongue, sickness of stomach, distention of the abdomen, and sometimes even inflammatory symptoms; the other, a paralytic affection, which seldom extends beyond the upper extremities, and is chiefly confined to the extensor muscles of the fore-arm, in consequence of which the power of raising the hand is in a greater or less degree destroyed. This is often ushered in, or accompanied by, headache, vertigo, and a sense of weight and pain between the shoulders; and it is generally a sequel of the constipation and colic, often not taking place till after a second or third attack of that affection; but this is by no means constantly the case. Thus, I once had a waterman under my care who had lost the power of his hand, without any previous ailment, in consequence of painting his boat; and in a case I shall presently relate, the same fact will appear. I should be inclined to think, from what I have seen, that the paralytic affection is more often the re-

sult of the exposure to which painters are subject, than of that to which the manufacturer of lead is liable; though the latter has the abdominal affection very frequently. There are certain processes in the operation of painting, which are more particularly apt to bring on paralytic attacks; as, for instance, the flattening process, in which the white lead is mixed with a large proportion of the oil of turpentine, which, though its action is not obvious, may very probably prepare the system to receive the impression, or may make that impression more intense. Very plethoric persons, lately come from the country, are very liable to be affected with colic in the white-lead manufactories; and the habit of drinking seems to predispose to the paralytic affection. It is also observed by those who are employed in painting, that a neglect of personal cleanliness often leads to both forms of the disease, and that exposure to cold and wet appears often to be the immediate cause of the paralytic attack. * * *

The diseased condition of the mucous membrane of the colon, in cases of this kind, deserves well to be borne in mind: the frequent irritation to which the viscus is exposed, appears gradually to thicken the cellular substance beneath its mucous membrane, so that the surface becomes thrown up into ridges, thick, hard, and unyielding; and the membrane itself seems to lose part of its power of resisting injury; it becomes abraded, and is discoloured by the fæces, and covered with shreds, probably in part derived from the separating and sloughing of the membrane itself, and partly from the effusion of lymph. It is not only in colic from lead that this condition of the colon exists, but I have seen it take place in other cases of obstinate or habitual constipation; particularly after the employment of much purgative medicine."

Five or six illustrative cases are given, well deserving of, if we could but spare them, the space which they would occupy; but we shall make no apology for presenting the reader with the views of treatment which our author sets forth.

Treatment.—The warm bath, with fomentations and poultices to the abdomen, and the combination of calomel and opium, followed by castor oil, our author thinks, will be generally found sufficient to overcome the constipation and colic;

but the calomel and other purgatives should be used with caution, as the large intestines are not well able to endure severe irritation; extensive destruction of the mucous membrane of the colon might be the consequence. Leeches applied to the abdomen, and sometimes to the anus, afford relief, by subduing the tendency to inflammation, while they facilitate the action of the purgatives. The bowels should engage our principal attention in paralysis from lead, as well as in colic; but this alone will not restore the injured powers. In the recent attack, Dr. Bright's practice is to take blood by cupping from between the shoulders, under the impression that, in all probability, some inordinate action or congestion exists in the brain, or the upper part of the spine. Blisters also are extensively applied, in the early part of the treatment, near the supposed seat of the disease; and afterwards, with a view of stimulating the nerves of the paralytic limbs: and all those tonic and stimulating remedies which are found useful in other sorts of chronic paralysis, may be employed in the advanced stages of this. Splints, too, to the weakened wrists, in the manner recommended by Dr. Pemberton, will be attended with decided advantage.

Concussion and Pressure from Accidents.

This is a subject which would seem properly to belong to the province of the surgeon; yet injuries of this sort (as Dr. B. observes), as they frequently lay the foundation for diseases which the physician is called upon to alleviate, should not be passed over in a collection of medical cases.

Falls and Blows.—Upon the occurrence of a severe fall, the fracture may take place either at the part of the cranium which is struck, or at any distant part, but more particularly at a part nearly opposite to the blow, or at the basis, in various directions; and the fragments of bone may either be displaced or not, producing by their points and edges more or less injury to the membranes, or to the brain itself. The dura mater may be detached or lacerated; blood may be effused between it and the bone; or blood may be effused internally, between it and the arachnoid. The arachnoid and pia mater may be lacerated, or may be detached from the brain; blood may be effused between

the arachnoid and the brain. The cineritious portion of the brain may be lacerated either by the pressure of the bone or by the tearing away of the pia mater; the medullary substance may likewise be lacerated, or vessels more or less numerous may be ruptured in its substance, and blood may even be effused into the ventricles. Now, to which of these, or whether to any of them, the symptoms of concussion are to be ascribed, may be made matter of question.

The symptoms of this sort of injury may be very various. Frequently there is an immediate abolition of all consciousness for a longer or shorter time,—sometimes for many hours; the stomach may eject its contents; a state of stupor may be induced, from which the patient may be occasionally roused, so far as to give incoherent answers, and he may remain in a state of idiocy for several weeks. There, however, may be, in some cases, recovery in the course of a few hours, or a few days, but without any recollection of what has passed. On other occasions, intolerance of light, screaming, convulsions, or paralysis, and not unfrequently apoplectic coma, and death.

These latter symptoms belong to the existence of inflammatory action, and there is every reason to believe, from what has been in most cases traceable after death, that their immediate cause is laceration of the brain and vessels, in greater or less degree. In this the great difficulty of the treatment consists. As the injuries may be of a very varied description, they require a very varied sort of management. The fractured cranium, for instance, though it may be restored without the least inequality of surface that would produce irritation, in favourable cases, may in others, where a thick mass of bony deposit is required for the repair, be a source of pressure as long as life continues. And again, from the injured dura mater, fungous growths may spring out; on the arachnoid, lymph may be deposited; and, with regard to the lacerated portions of brain, although there is much mystery as to the stages by which repair is brought about, Dr. Bright thinks it is analogous to the repair of apoplectic injuries, requiring much time and great caution in the management of the invalid for a long period after the first symptoms have subsided.

Two cases are given by the author in illustration, both terminating favourably, though without complete removal of the effects of the accidents—falls in both instances; one of the cases, resembling in some degree the effects of paralysis from lead.

Pressure on the Spinal Cord.

This may be either from disease or accident. In scrofulous subjects more particularly, inflammation, suppuration, and absorption, are very apt to take place in the complicated structure of the spinal column; and the body of the vertebræ being in part destroyed, the regular continuity of its canal is interrupted, and pressure on the spinal cord is the necessary result; sometimes the same effect is produced by some external cause—as an aneurismal tumor, producing ulceration and absorption. One of the most frequent situations for disease is the articulating surfaces of the first two cervical vertebræ; for though the early part of the process, and the ulceration of the softer structures, produce no pressure and no paralysis, yet, after a time, the mischief either becomes so great as to allow of displacement and pressure, or the bony matter is irregularly deposited, and thus pressure is produced.

The accidents which produce pressure on the spinal cord are chiefly occasioned by falls and blows, or by weights passing over the body.

In disease of the upper cervical vertebræ, the pain and stiffness with which it comes on are often mistaken for the effects of cold and rheumatism; and the thickening of the surrounding parts, the confined and painful motion of the head, referred to deep-seated structures: the partial pressure in the progress of the disease, giving rise to symptoms of local nervous affection and partial paralysis. Death ensues from exhaustion, or the sudden displacement of the vertebræ. Three short cases in illustration are supplied by Dr. Bright; they were communicated to him by Mr. Key.

Where accidents produce displacement and pressure in other parts of the column, there is much variety in the symptoms, owing to the different kinds and extent of the injuries inflicted. Cases of this description generally come under the care of the surgeon; but they are not altogether passed over by

Dr. Bright. Two very interesting examples are given in the Reports, but we are obliged to limit ourselves to one of them, and the remark by which it is followed.

“ In the month of August 1827, two patients died, nearly about the same time, with paraplegia;—the one from a cart going over his back and displacing some of the dorsal vertebræ, which were reduced; he lived about three weeks;—the other from a chronic disease of a fungoid nature, brought on in the spine by the circumstance of carrying too great a load on the head, which after death appeared actually to have crushed the body of one vertebra, and around the part fungoid disease had been established.

“ Both these patients died from the irritation of ulcerated bladder. In the former, sinuses were formed by ulceration into the cellular tissue surrounding the bladder; and two days after the accident, several bullæ arose on the feet and on the part of the inside of the knees where pressure took place; these made a very slow progress, continuing almost unaltered for nearly a fortnight; they then dried away, being apparently first ruptured.

“ The two most remarkable points to be incidentally noticed in the foregoing cases are, first, the diseased state of the bladder, resulting from its diminished power to resist injury, and from the changes taking place in the condition of the urine, detained in its most depending part, which becomes one of the most frequent causes of fatal irritation in paraplegia;—and, secondly, we observe the occurrence of bullæ on the paralyzed limbs, to which circumstance I have already referred in some remarks made at page 383; the general inability to resist injury is likewise marked by the extensive sloughing of all the paralyzed parts on which pressure is made.”

Nor is that minor sort of paralysis passed over so familiar to the physician—loss of power in the muscles of one side of the face, arising generally from exposure to cold. Dr. B. has known it most frequently to be the result of inflammatory disease of the covering of the portio dura. Attacks of this kind often yield to leeches, antiphlogistic remedies, and blisters.

Chronic Hydrocephalus.

After a brief but valuable detail of the principal facts connected with this distressing complaint, Dr. Bright presents us with some observations on the

treatment. The case recorded by Dr. Traill, of Liverpool, in "Monro's Morbid Anatomy of the Brain," is one of the few in which remedies have proved at all successful. It was the case of a child of eighteen months, with an enormous head and distended sutures; and in whom strabismus, frequent convulsive twitchings, and an approach to coma, were present. Dr. Traill and Mr. Reay rubbed mercurial liniment upon the head daily, and purged the bowels briskly with calomel and other cathartics; and this system was pursued steadily for a month. At the end of ten months, the child was seen in perfect health of mind and body, though the head continued rather large.

With regard to tapping the head in this complaint, there are some passages here, regarding the history of the operation, which we must beg leave to extract:—

"The first case in which this operation was performed, to the record of which I can immediately refer, is to be found in the sixth volume of the Medical Communications, where an account is given of its being performed in the year 1778, by Dr. Remmett, of Plymouth, on a child two months old. In the course of two months the head was punctured with a lancet five times, and no less than eighty ounces of fluid drawn off. Seventeen days after the last operation the child died, and the whole cavity of the cranium was found full of a pellucid fluid, of which above two quarts were collected. This fluid seemed to be external to the brain, and immediately under the dura mater, which with its processes were entire; and after the most minute examination, nothing could be traced but the medulla oblongata descending into the spine, and behind the orbits of the eyes a very small quantity of medullary substance indistinctly resembling brain, but quite entire, and covered with the pia mater.

"In the year 1817 a case occurred to Dr. Vose, of Liverpool, in which he punctured a hydrocephalic head four times, and drew off thirty-two ounces of fluid, with great relief to the patient, who several months afterwards, at the time the case is recorded, was doing perfectly well: but we unfortunately find it stated by Dr. Traill, in Dr. Monro's work, that the child died of the disease some time after. Subsequently to this, Mr. Callaway, of Guy's

Hospital, performed the operation, but with only partial success; the fluid accumulating again rapidly after each puncture, and the children sinking under excessive exhaustion and emaciation. Within the last two years, Dr. Conquest has performed it on three or four hydrocephalic children, and with such favourable results as to give him sanguine hopes of its ultimately proving a most valuable operation. In a case operated upon last year, not the slightest return of the disease has appeared, and the child is stated to be in perfect health. And the last case he has publicly mentioned, is in the Medical Gazette for November 6th of this year (1830), in which, at the first operation, twelve ounces were taken away, and at the second, eighteen ounces. The child is stated to be apparently well, the bones having nearly closed, and the patient free from any evidence of disease, although before the operation it had fits almost incessantly.

"Soon after Dr. Conquest's cases were brought before the public, one was stated in the Medical Gazette, where the operation, having failed in the hands of some other practitioners, gave an opportunity of examining the state of the brain, which was so completely softened down as to suggest the idea that the operation could not well have succeeded. However, I am inclined to believe that this softened condition of the brain might have been the result of some recent slow inflammatory action; and, at all events, the apparent success which has attended one or two cases, holds out a slight encouragement to a more extensive trial of this most doubtful remedy. There is no doubt that many cases will fail; for in some the tendency to pour out fluid continues unabated, and between each successive operation the head rapidly increases: but if, fortunately, as sometimes in the operation of paracentesis of the abdomen, the tendency to accumulation should have ceased, either from the effects of remedies or from some local change depending on the abstraction of the fluid, and if the cerebral disorganization should not be totally irreparable, a cure may be effected: still, however, it remains to be proved to what extent such cures will be satisfactory as to the future mental condition of the child; and probably a very small proportion will even apparently succeed."

Four cases of chronic hydrocephalus

are given; the last of them, the remarkable one of James Cardinal, who died in Guy's Hospital, in the 30th year of his age, on the 24th of February, 1825. The circumference of the head at its largest diameter, as measured after death, was $32\frac{1}{2}$ inches; the distance from one articulation of the jaw to the other, over the vertex, was 21 inches; and from the insertion of the ossa nasi, in the os frontis, to the posterior margin of the foramen magnum, was $23\frac{3}{8}$ inches. About a pint of fluid was found in the ventricles, and six or seven externally; the cavity of the skull, when cleared of its contents, would hold ten pints of water. The ossification of the cranium might be considered complete. At his birth his head was scarcely larger than natural; but in about a fortnight it began to increase rapidly, until at five years of age, his mother was of opinion, it attained as great a size as ever it did after. He was weak and tottering on his limbs, near-sighted, but heard with remarkable acuteness; his taste was perfect, and his digestion good. His mental faculties were very fair; he read and wrote pretty well; his memory was tolerable; and he is said never to have dreamt, nor to have had sexual desires. He was childish and irritable in his manner, and was easily provoked. Very affectionate to his mother; voice feeble, and somewhat hoarse; bowels generally costive. He was subject to fits, which came on during sleep. He caught cold, and died with febrile symptoms and diarrhœa.

Upon examination of the head after death, the ventricles were found to be so much disfigured by the fluid within, as well as by the peculiar position the brain had assumed, that it was with difficulty the various parts were discovered. The cerebellum was flattened and vascular.

Some remarks on spina bifida, congenital hernia, and serous cysts in the arachnoid, follow, and are accompanied by plates of beautiful execution. The present part of the Reports concludes with some general observations on pressure, and on the deranged action of the kidneys as it affects the cerebral functions. This latter subject is one of great interest, and would deserve to be laid before our readers at full length; but we must be content, seeing that we have devoted so much space already to

extracts from this great work, to close for the present our agreeable labour—to resume it, however, we hope, ere long, with abstracts from the remaining volume.

MEDICAL GAZETTE.

Saturday, March 24, 1832.

“Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso.”—CICERO.

PESTILENTIAL DISEASES OF ENGLAND.

It happens, unfortunately, that we have no contemporary medical records of the first outbreak of the *Sweating Sickness* (of which, among other pestilences, we spoke last week); we only know that it was imported with the new King (Henry VII.) and his ill-appointed troops, and that it raged for a time with the greatest fury. If we possessed such records, we should probably find that the faculty were, as usual, not a little divided in opinion on the subject—the better-informed, and the more unprejudiced, looking upon the disease as it really was, new, contagious, and formidable—the crowd of quacks and medicasters, to shew their superior intelligence, and that they were ignorant of nothing, maintaining the complaint to be by no means new, nor infectious, nor dangerous, in *their* estimation. But this was peculiarly the dark period in the medical history of Britain, preceding the dawn of a day of more enlightenment; the wars and political troubles had, for more than a century, repressed, if they did not totally extinguish, every thing like ardour in the pursuit of professional knowledge. The clergy had abandoned nearly all connexion with medical practice, and the country was overrun with

unlicensed empirics, who have left no traces of their existence except in the traditionary stories of their audacious impositions. Not through such, surely, could we expect to have any fair account of the facts attending the progress of this, or any other novel malady visiting the country.

But the sweating sickness does not by any means afford the most striking example of the backwardness of our profession at this early period; the other disease, the French or Italian, as it was indifferently called, which broke out soon afterwards in England, as well as throughout Europe, affords the strongest negative testimony to the utter ignorance of our native practitioners. Here was a disease of a virulently contagious description, *all at once* seizing on numerous victims, resisting every mode of treatment then known; not confined to the poor, or those whose circumstances exposed them to more than ordinary hazard of infection, or who were destitute of the means of procuring advice, but exhibiting its rage, and exerting its baneful influence, over princes, cardinals, nobles of every rank, and ecclesiastics of both sexes, thus proving both its immitigable severity, and putting the novelty of its wide-spreading scourge beyond all doubt. We know not exactly, it is true, how the faculty in England acquitted themselves on this occasion; but, happily, we have ample testimony regarding the mode in which their contemporaries on the continent behaved; and it may not be amiss to take a very brief notice of the circumstances which then occurred: they so much resembled what has been lately passing under our own eyes, that in our thoughts on this subject we have sometimes, we own, confounded the identity of the Tomitanuses, the Peter Maynards, the Brasavoli, and the Petronii, with that of some of our own "eminent" contemporaries. There is a passage in the English historian of

physic*, where he treats of the origin of this "new plague," which may serve to characterize the proceedings of some of those worthy moderns to whom we allude:—"In the earliest appearance of this distemper, as well as since, there were many who, not being used to think or reason any further than as the ancients shewed them the way, took a great deal of pains to prove, that the disease was known both to the Greeks and Arabians, though but imperfectly described, and represented under different names. And here we have instances how the words of old authors may be wrested and perverted to serve the present purpose, and support a favourite opinion; for their method of arguing was *to quote by scraps, to pick out one symptom out of one treatise, another out of a second, and so on, till at last they dressed up such a disease as the ancients had not the least notion of;*" and the same mode of reasoning, he goes on to tell us, was used by the same people when the small-pox first made its appearance; and so, no doubt, it will ever be, where the herd of would-be reasoners is made up of the half-learned and the whole-conceited, and where, upon the occurrence of any unforeseen emergency, the love of notoriety, of whatever stamp, overbears the natural respect which is due to the dignified simplicity of truth. But with regard to the proceedings of our "reasoners."

The disputes that occurred concerning the new disease caused the physicians, we are informed, to lie under "very great ignominy and contempt;" and not without pretty good reason. It was not only violently contested by some, whether the malady in question came from the Indies or not—or whether it was not well known in every preceding age up to Moses and the Jewish patriarchs—whether it did not come on the wings of the wind—or from insects—or from earthy exhalations; but some

* Dr. Freind.

denied that there was such a disease in existence at all. One man, among the writers *de Morbo Gallico*, favours us with a whole chapter on the important question—*whether it be a distemper or no*—previously to his arriving at what he should have begun with—the symptoms of the disease. Another insists upon its including within itself no less than 234 different combinations or species. And, by the way, after upwards of three centuries of experience, and great changes and modifications, from a variety of assignable causes, having occurred in the nature of the complaint, are we entirely free from the mists that so early obscured its features? After it has been calculated that above a thousand treatises have been learnedly written on the subject, is all controversy set aside? On this, however, we will take leave to exercise our discretion, of saying as little as possible, or rather of being silent altogether.

It may be thought by some that we have digressed in some measure in noticing the occurrence of this disease under our present head, but we are confident that it is the dearth alone of professional writings characteristic of the period, that can at all justify the notion of its being unentitled to the appellation of one of the most formidable, as well as loathsome, of English pestilences. There were no registers of mortality kept at so early a date; and it is probable that if there had been, they could be but little relied on, owing to the secrecy that must have been generally observed, especially by the higher classes of sufferers. But we have ample authority for stating this fact, that in its earliest visitation it was not only highly malignant, but fully as fatal as the small-pox*, while it was infinitely more distressing during the progress of its symptoms. How severely it was felt, and how dangerous it was looked upon to be, even before the close of the 15th century, we may learn from

the fact, that in 1497 it gave rise to an *arrêt* of the parliament of Paris, which strictly enjoined all persons not residing in that capital, when seized with the disorder, to depart from the city under *the penalty of death*; and in the same year it is on record, that the King of Scotland (James IV.) issued a proclamation banishing all persons afflicted with the *grand gore* to the island of Inchkeith, over against the town of Leith. These particulars we have thought worth noting, in the dearth of more pertinent information concerning the state of things at the same period nearer home. We will now turn to a calamity of a still darker cast.

At what period our island became first acquainted with the true *Plague*, it is difficult to ascertain; but there is every reason to believe that it was not much later in visiting us than the sweatingsickness, if we had it not here indeed, long before. We have already stated our conviction that this latter disease was the Levant plague in a modified condition, and it is distinctly recorded at what intervals it visited the country in its new form. In the intermediate periods, however, it is certain that there was another pestilence in the land; and this was probably the genuine bubonic plague. In the *Flagellum Dei*, a curious little old tract in the library of the British Museum, and for a reference to which we are indebted to Mr. Marshall, we find the following notices of calamities, drawn up in a tabular form; after mentioning the great pestilence of 1348-9, and how, thirteen years after, there was another great mortality, by which the nobility greatly suffered, it then proceeds to record, that in Ann. Reg.

3 Rich. II. there was a desolating mortality in the north.

17—19 Edward IV. innumerable people died.

1 Hen. VII. Sweating sickness.

15 Hen. VII. 30,000 died of the plague in London. (This was A.D. 1500.)

* Freind.

9 Hen. VIII. Sweating sickness, carrying off multitudes of every class.

13 Hen. VIII. Great mortality.

20 Hen. VIII. Sweating sickness over the whole realm.

33 Hen. VIII. Hot agues and fluxes.

36 Hen. VIII. Great plague.

5 Edw. VI. Sweating sickness very general.

6 Eliz. Plague, of which 21,500 died in London.

36 Eliz. Plague, of which 17,890 died, including the Lord Mayor and three Aldermen.

1 James I. (A D. 1603). Plague, of which 30,578 died in London.

This brings us through the sixteenth century by a series of awful catastrophes probably unequalled in the annals of a similar period in any other country in the world. That the list, however, is rather under than exceeding the true state of the mortality, we have much reason to surmise; at all events, from what we find in Holinshed and other authorities, who treat of the latter years, we have every reason to put much faith in the simple table before us. Holinshed says, that in Ann. Reg. 19 Edw. IV. (1479), "there was great mortality and death by the pestilence, not only in London, but in divers parts of the realme, which began in the latter ende of September, in the yeare last before passed, and continued all this yeare till the beginning of November, which was about fourteen monthes; in which space died innumerable of people in the said citie and elsewhere." And again, in 15 Henry VII. (1500), "The next yeare after, there was a great plague, whereof men died in many places, verie sore; but specielle and most of all in the citie of London, where died in that year thirtie thousand." The *plague*, we may perceive, was now familiarly known in England; and London was seldom for ten years together exempt from some dreadful warning of its presence. Maitland distinctly mentions, that, for five-and-twenty years previous to the great fire, the metropolis was never wholly free from the disease: it would seem to us,

however, that if he had said at least seventy-five for twenty-five, he would not have erred in excess. The bills of mortality were temporarily commenced in 1593, by the worthy fraternity of St. Nicholas; more regularly and permanently from 1603, and, from the year 1629, the worshipful company of Parish Clerks have not only supplied us with lists of the weekly number of burials in London, but with a report of the diseases and casualties which have proved fatal within their jurisdiction. We some time since, in noticing Mr. Rickman's abstract of the population returns, extracted a passage rather reflecting on the utility of the "Bills of mortality" reports; but, perhaps, on more mature consideration, it were not advisable to condemn, in so wholesale a manner, those "short and simple annals," particularly in the lack of matter more trustworthy. The truth is, that if we deprive ourselves of the testimony of these contemporary, however imperfect, documents, we give up the only opportunity we have of instituting a comparative inquiry into the unhealthiness of certain periods, and there is nothing to rely upon, except the detached notices which are to be met with in the memoirs of individuals living about the particular periods in question. But to return to the state of pestilence in England.

The plague which raged in London in the year 1603, soon after the accession of James the First, is mentioned by several authorities as having been very dreadful. In the Memoirs of that Monarch and his times, by Miss Aikin, we are assured that fully 30,000 persons were cut off by it; and it may be worth noticing, as affording a ground of comparison between the religious feelings of that period and the present, that there was then proclaimed a *weekly* fast and humiliation, wherewith to avert the heavy judgments under which the people were labouring. Another

fact, from the same source, is also worth attending to : and by the way, how lamentable is it to reflect that we are so often beholden to some such almost accidental hints of intelligence for some of the most important facts connected with the subject of English medical history ! The plague of 1603 (as the work of Miss Aikin informs us) raged through all classes of the community, with little partiality for one set rather than another, during several months : and it was far from being confined to the lower orders, as the following extract from a contemporary letter will testify. Sir Thomas Edmonds writes to the Earl of Shrewsbury, and his letter is dated September 11th, in the year above-mentioned :—"The court hath been so continually haunted with *the sickness*, by reason of the disorderly company that do follow us, as we are forced to remove from place to place, and do infect all places where we come. We are now going within a few days to Winchester, to seek a purer air there, and by reason that *the Spanish Ambassador hath had one of his company lately dead of the plague, at Oxford*, his audience, which was appointed to have been given him here, is deferred till the King's coming to Winchester, which doth nothing please his greatness*."

The great plague years in the seventeenth century, after the one which has just been noticed, were 1625, 1636, and 1665 : in these years, there is no reason to doubt but that, of the plague alone, there died in London (within the bills of mortality) 35,417, 10,400, and 68,596, respectively ; and we have even sufficient authority for concluding that, in *all* London, there were cut off fully one-half as many more. Sir John Reresby, in his *Memoirs*, states the number that was swept away in the last-mentioned year, to be 97,309 persons ; and Lord Clarendon, in his *His-*

tory of his own Life, says, that " many who could compute very well, concluded there were double that number," (alluding to the number stated in the bills).

But it is perfectly awful to think what must have been the mortality of the metropolis from plague, during all the years of this century put together, in which the disease is known to have existed. In 1604, 900 perished ; in 1605, 400 ; in 1606, 2000 ; in 1607, 2000 ; in 1608, 2000 ; in 1609, 4000 ; in 1610, 1800 ; and from 1640 to 1648, the number was every year above 1000. Nor do we find that this dreadful scourge disappeared altogether from the bills until thirteen years after the great fire of 1666, which has been commonly supposed to have destroyed the germs of the pestilence. Above one hundred and fifty years of complete exemption from it have now, it is true, been enjoyed ; but what hopes of future security we are entitled to rest upon that ground, it would seem rather presumptuous in us to determine ; nor do we think ourselves justified in the expression of those gloomy inferences, which cannot but be too obvious to every reflecting mind. The crowded state of our population—particularly in the metropolis, and in the great towns ; the poverty and wretchedness of the lower classes ; the reluctance, if not open opposition, manifested by some of the higher, to the enforcement of the quarantine laws ; and, as we have recently seen, our perfect liability to be invaded by new forms of disease ; these are data that cannot but be fruitful of serious considerations : we leave them, however, in the hands of other inquirers.

It was our intention to have touched upon some other points connected with the mortality from disease, and the medical statistics generally, of this country, within the last century or two, and to have founded some of our observations upon the valuable works of

* Aikin's *James I.* p. 159, 160.

Mr. Marshall and Dr. Ayton, which have very recently been published: but we find that we must avail ourselves of another opportunity of putting our design in execution. The remarks, indeed, which we have to offer, relate principally to diseases not pestilential, or afflicting the country at large, like plague; they may therefore, we conceive, very well follow the subject of which we have been treating.

CENTRAL BOARD OF HEALTH.—SIR D. BARRY.

IN our last number we observed that it was not to be expected that the profession in England would be much influenced by the clinical instructions of a Board, "not an individual of which is at all acquainted with the diseases of that class of the community among whom the present epidemic prevails." It has been pointed out to us that this is not correct, inasmuch as Sir David Barry "has been for the last six years, and still is, in private practice as a physician in London; and during the last four years of that time, physician to a dispensary." We hasten, therefore, to make the above qualification in our statement—misrepresentation being no part of our object; at the same time we must take leave to add, that Sir David's name has been so much associated with his late physiological researches in Paris, and his very recent missions to Gibraltar and to St. Petersburg, that we cannot avoid surmising that his reputation as a private practitioner may, in the minds of his professional brethren—as certainly in ours—have been merged in the greater notoriety of his public services. We do not know to what dispensary Sir David is physician: the source, however, whence our present information has been derived, leaves us no doubt of its accuracy; and we willingly admit that an exception should have been made in his favour, to such an extent as the opportunities he has enjoyed may justly claim for him.

CHOLERA IN EDINBURGH.

THE cholera prevails at present at three different places, in immediate contact with the suburbs of Edinburgh, and has also manifested itself at one or two points in the centre of the city. At the "Water of Leith," not more than four hundred yards from the north-west boundary of the town, the disease has assumed a very malignant character:

the place is inhabited by a dissipated population, consisting of millers, quarrymen, dyers, weavers, and pig-keepers, and it is traversed by several streams. Not being within the bounds of the Edinburgh police, much less could be done towards ameliorating the condition of the people than in the city; and the consequence was, that in a population not exceeding 1500, the daily number of those attacked quickly increased to eight or nine on an average, almost all the cases being of the most virulent description. All possible steps were immediately taken to arrest its progress; the houses were cleaned and lime-washed, and the infected dwellings evacuated and purified, and *even the lanes and alleys fumigated with chlorine*. The sick were conveyed to hospitals, and 200 persons removed with their own consent to quarantine, while others of themselves sought for healthier quarters, in which they remain under the surveillance of the police. The disease has now suffered a manifest interruption, and there is reason to hope may be altogether arrested. Not more than six or seven have been attacked in quarantine, and some of the cases have been very trivial. It is impossible to read these accounts, which we have derived from the best authority, without being forcibly struck with the excellence of the arrangements.

CHOLERA IN IRELAND.

WE regret to state that cholera has made its appearance at Belfast, adding another to the numberless instances of its first attack in insular situations, being at a sea-port.

CHOLERA NOT IN PARIS.

WE have received accounts from Paris to the 17th instant, in which the health of the French capital is particularly spoken of. The dysenteric attacks which prevailed during the autumn and early part of the winter had disappeared, and been succeeded by catarrhal affections of the throat and chest. There had been less than usual of the low typhoid fevers which generally prevail there about this season. Hospitals had been prepared, and every thing was in readiness for the reception of cholera; but the disease had not been met with, though all the doctors were on the *qui vive*. It is obvious, therefore, that the announcements of cholera in Paris, which have appeared in some of the London papers, are premature.

STATE OF THE GANGLIONIC NERVES IN CHOLERA.

IN a late number we extracted, from the *Cholera Gazette*, the account given by M. Delpech of the certain morbid appearances which he stated that he had discovered in some of the ganglionic nerves, in dissecting the bodies of cholera patients. The week before last, the above distinguished foreigner undertook to demonstrate the parts in the presence of Mr. Harrison, of Dublin, Dr. P. H. Green, and several gentlemen connected with the Board of Health. Considerable difference of opinion existed as to whether the nerves exposed were or were not in a diseased condition; but no doubt was expressed as to the semilunar ganglion, as pointed out by M. Delpech, presenting a very unusual appearance. Mr. Harrison, who had some misgiving on the subject, now took the knife, and demonstrated to the conviction of all present the real semilunar ganglion at some distance from the supposed one, and in a healthy condition. What are we to say of the dissections made by M. Delpech in Scotland? Did the same mistake occur there likewise?

It is amusing to observe, that M. Halma-Grand has communicated the *discovery* of M. Delpech to the *Académie de Médecine* as his own, and without the slightest reference to his countryman!

CHOLERA SUBJECTS.

A SHORT time ago instructions were sent to an officer of engineers at Woolwich, to have a large grave prepared in that vicinity, for the interment of the bodies of some of those who had died of cholera, and which were sent down the river accordingly. Great excitement and apprehension were produced in the minds of the people in the neighbourhood of the spot; these were not a little increased when, a few days after, a second order of a similar nature was issued; and it was now thought that nothing less than a general contamination of the atmosphere for miles around could possibly be anticipated. Some circumstance, however, led those employed in preparing the second, to examine more particularly into the state of the first grave, when it was found that the whole of the bodies had been removed! The second supply, however, was duly deposited in the earth; and, we understand, that they also were discovered to have vanished when the ar-

rival of a third importation led to the fosse being again examined. It thus appears that the bodies of deceased cholera patients go down the river with one tide, and are brought up again to town with the next. They are, of course, intended for the dissecting rooms, and we could mention some at which they have made their appearance, being easily recognized by the quantity of pitch with which they are covered. This matter requires immediate attention on the part of the police.

ADDRESS TO THE MEDICAL PROFESSION, AND TO THE PUBLIC GENERALLY.

THE present alarm respecting the disease which is now said to exist in London and its vicinity under the name of cholera, is maintained by some to be unfounded or exaggerated; while by others it is asserted, that this alarm is neither without cause, nor disproportionate to the danger. Whence such diversity of opinion may have originated, has not been ascertained; nor has it been discovered whether there be any reasonable cause for the prevalence of opinions so opposed.

This unsettled state of the public mind has been productive of consequences the most pernicious. Our national relations have been injured; our internal prosperity has been shaken. The medical profession have been aspersed by one party, as encouraging panic without cause; and by another, as concealing from the public eye the real nature and the full extent of the disease.

To clear the character of the medical faculty of these aspersions, by inquiring into the actual causes of this general alarm, and to reconcile these conflicting opinions, a Medical Association has been formed, to investigate the nature, modes of propagation, extent and treatment of that disease, and lay candidly before the public the results of its inquiries. As this Association belongs to no exclusive party, it advocates no exclusive views. The practitioners, of whom it is composed, espouse no theory; they support no faction; their bond of union is the public good; their common object is the cause of truth. Their interests are promoted neither by encouraging excitement, nor by resisting just alarm. If the disease known under the name of cholera exist in Lon-

don, their object is to ascertain its extent, to elucidate its causes, to explain its management, and to restore the tranquillity of the public, by an honest statement of the truth.

An Association formed with motives so pure, and an object so important, can have no enemies but those of science and of mankind. They, therefore, look to the public for countenance, and to the profession for assistance. They more especially solicit the co-operation of the medical officers of parishes and guardians of the poor. As they intend no opposition, they anticipate no resistance. In the truest spirit of philanthropy have they been organized, and with the most rigid respect to truth and candour will all their labours be conducted. They feel that the principles which they follow can lead them only to truth, and they are persuaded that the plan upon which these principles shall proceed, can scarcely fail to ensure ultimate success.

The proceedings of the Association shall at all times be open to inspection; the register for the admission of members is now prepared for signatures; and while they express the most friendly feelings towards such as may decline to join them, the Association earnestly solicit, and confidently hope for the speedy co-operation of all who are zealously attached to the interests of medical science, and wish sincerely to promote the welfare of the human species.—L. STEWART, M.D. Chairman.

LONDON HOSPITAL.

Removal of the Superior Maxillary Bone, for Disease of the Antrum — Ligature of the Common Carotid Artery.

CHARLES SMITH, æt. 45, a notice of whose case was given in a former No. was admitted into the hospital Nov. 20th, under Mr. Scott, for disease of the right antrum. His habits have always been very intemperate. About four months ago he was attacked with symptoms resembling chorea, which came on gradually, and occurred sixteen or eighteen times a day. These fits, which were attended with much giddiness and pain in the head, left him in about a fortnight, and immediately afterwards he perceived a swelling in the situation of the right antrum, which continued to increase chiefly towards the nares, so as to press the septum of the nose towards the opposite side, and thereby obstruct the passage of air through the right nostril. The swelling is about the size of a small egg; and since its development he has complained of lancinating pain through, and numbness of, the upper lip. His general health

is good. On the 28th, the tumor had continued to increase, he was ordered

Decoct. Cinchon. c. Sod. Carbon. ter die.

In consequence of the tumor being frequently examined, inflammation followed by suppuration occurred in the tumor, which afterwards became somewhat diminished in size. In about two days afterwards erysipelatous inflammation extended over the right side of the face, but soon subsided. The tumor commenced again to increase; it occasioned absorption of part of the hard palate, and formed a projection on the right side of the roof of the mouth. A lancet was passed into it, and a probe introduced into the antrum, upon withdrawing which there was a free hemorrhage from the wound, which was stopped by slight compression.

On Wednesday, January 4th, Mr. Scott proceeded to remove the whole of the disease. The patient was placed upon the operating table on his left side, with a pillow under his head, and pressure was made upon the carotid artery by an assistant with his thumb. An incision was made from the internal angle of the eye to about a quarter of an inch from the commissure of the lip, and the integuments dissected from the surface of the tumor, and the ala of the nose separated from its connexion. In the performance of this part of the operation two vessels were secured; but the hemorrhage became so profuse from the vessels of the face, that it was thought advisable to secure the carotid artery, as pressure did not succeed in stopping the circulation through it. The common carotid was therefore taken up, in performing which some difficulty was experienced in passing the ligature around the artery, which was eventually accomplished by a bent eye probe. The first part of the operation was next concluded, and the contents of the orbit detached from their connexion with the bone. By means of a powerful pair of bone forceps the connexion of the superior maxillary to the malar bone, its nasal process, and its junction to the fellow bone at its symphysis, were next divided. This part of the operation was accomplished without much force, in consequence of the disease having occasioned considerable absorption of the bone. The soft parts attached to the bone at its back part being divided with a curved knife, it was removed. Several portions of fungus which were observed at the bottom of the chasm, were afterwards removed. The vacancy was filled up with small bits of lint, and the edges of the wound in the integuments were brought together, and retained in approximation by sutures and two hare lip pins, and then dressed. There was scarcely any blood lost after the carotid artery was secured. He was ordered to take forty drops of laudanum immediately, and twenty drops more to be repeated every half-hour until sleep was procured.

Nine P.M.—After taking ninety drops he had a little sleep. He now complains of pain in the throat when he swallows, and is very thirsty. Pulse full, but not frequent. Ordered to take when awake,

Mist. Salin. c. Træ. Opii, gttss. xx. quaque sextâ horâ.

5th, two P.M.—He has passed a good night. In the morning, soon after taking a dose of cal. and jalap, he was seized with a convulsive action of the muscles of the whole body, but chiefly on the left side; these still continue, with intermissions, and he complains of excessive thirst. Pulse 100, and full; skin moist. He wandered a little in the morning, but is now quite rational.

Træ. Opii, ʒj. Enema Cathart.

In about an hour his bowels were slightly relieved, and the convulsions ceased. He took thirty drops more laudanum, and soon afterwards fell asleep until ten o'clock in the evening, when he awoke, and expressed himself as feeling very comfortable. Pulse soft and calm; deglutition less difficult.

Rep. Træ. Opii.

6th.—He passed a good night. About half-past seven A.M. he was suddenly seized with a fit of convulsions; he soon became comatose; his respiration difficult, with the mucous rattle; and he died a few minutes before eight, forty-two hours after the operation.

Postmortem Examination, six hours after Death.—The edges of the wound in the face were entirely adherent, and there was no appearance of any disease remaining. A firm coagulum was situated above and below the ligature in the carotid artery. The jugular vein was much distended with blood, which was in a fluid state. The vessels of the brain were in a state of congestion, and its substance was remarkably vascular.

The disease was of a soft fungous structure, and was exceedingly vascular; it had occasioned absorption of nearly the whole of the superior maxillary bone, and most probably originated in the antrum.

SUPPOSED CHOLERA AT KENSINGTON.

WE have received a letter from Mr. F. C. Wright, of Kensington, in which he maintains that the cases detailed by Mr. Badgley, in our last number but one, were not cholera. Mr. Wright enters fully into a detail of the part he took at the Board of Health; but we trust he will forgive us if we decline inserting his paper, being really pressed for room; but what is of more importance, because the discussion is of no interest to the profession: we record his statement that the cases in question were not cholera; and any thing like vindication of himself for holding this opinion appears to be quite unnecessary.

REPORT OF CHOLERA IN GREAT BRITAIN, UP TO FRIDAY, MARCH 23, 1832.

New cases since our last report, 387
Deaths 227

	Cases from Commencement.	Deaths from Commencement.
Afloat in the River.	60	26
Poplar	10	3
Shadwell	15	4
Limehouse	21	17
Ratcliffe	8	4
Rotherhithe	21	10
Bermondsey	86	40
Southwark	491	245
Newington Butts...	77	38
Camberwell	4	3
Lambeth	87	62
Christchurch	41	24
Westminster	35	17
Chelsea	21	16
St. George, Han. Sq.	5	4
St. Marylebone ...	62	16
Paddington Canal.	5	4
St. Pancras	8	6
St. Giles's	37	24
Whitechapel	36	21
St. George's East .	19	11
St. Luke's	20	11
Bethnal Green ...	5	3
Brentford	18	6
Wandsworth	4	1
Total	1165	614
Cases from places before reported }	48	33
Grand Total	1213	647

Total number of cases throughout Great Britain since the commencement of the disease 7902
Deaths... .. 2774

METEOROLOGICAL JOURNAL,

Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.

March 1832.	THERMOMETER.	BAROMETER.
Thursday . 15	from 31 to 43	29.29 to 29.66
Friday . . 16	25 44	29.76 29.73
Saturday . 17	30 51	29.65 29.42
Sunday . . 18	33 51½	29.49 31.1
Monday . . 19	32 50	29.66 29.76
Tuesday . 20	36 51	29.48 29.76
Wednesday 21	33 57	30.00 30.08

Wind variable, S.W. prevailing.
Except the 20th and 21st, generally cloudy, with frequent rain.
Rain fallen, .6 of an inch.

CHARLES HENRY ADAMS.

W. WILSON, Printer, 57, Skinner-Street, London.

THE
LONDON MEDICAL GAZETTE,

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WEEKLY JOURNAL

OF
Medicine and the Collateral Sciences.

SATURDAY, MARCH 31, 1832.

ON THE
EFFECTS OF MINUTE DOSES OF
MERCURY IN RESTORING THE
VITAL FUNCTIONS.

By A. P. W. PHILIP, M.D., F.R.S. L. & E.

It is now about five-and-twenty years since I first began to employ minute and frequently-repeated doses of mercury. I was led to them by observing, that the more the dose was lessened, and its frequency increased, the alterative effects of the medicine were the greater, pretty much in the same proportion as the immediate effects of each dose became less. It has long been my wish to lay before the profession the whole of the circumstances which influence this practice; but these are so numerous, and some of them, from the nature of disease, so complicated, that I have found it difficult to arrange the subject in a way that would give to others a clear and full view of it, and have thus hitherto been deterred from the attempt.

Having, in the early part of this practice, met with a few fortunate cases, I imagined that it was difficult to assign limits to its beneficial effects in a certain description of diseases. By degrees, however, these limits became apparent, but they have left a field more than sufficient to compensate for the pains I have bestowed on the subject. I have found, that with an attention to the circumstances which I am about to state, the minute doses never do harm, and this I believe cannot be said of any other mode of exhibiting mercury, and that in a wide

range of cases, both original and sympathetic, they effect what cannot be effected by any other means with which we are acquainted; and it is particularly grateful to my feelings that I have now obtained the testimony of many of my professional brethren to confirm my belief that they have been the means of saving many lives, which could not otherwise have been saved.

Large doses of mercury cannot be frequently repeated without often rendering the remedy as pernicious as the disease, and sometimes more so; and when they are given at distant intervals, the effect of one dose is frequently lost before another is taken; so that it often happens that little or no progress is made in the cure, and there is nothing but temporary relief to compensate for the debilitating effects of each dose; while, with respect to the minute doses, although each does little, this little it does without any strain to the constitution, and the next dose comes before the effect is lost; so that a gradual accumulation of the beneficial effect is obtained, and that, if the circumstances I am about to point out be attended to, without any injurious effects to deduct from it.

I need hardly say to those acquainted with our profession, that, in attempting to introduce the plan of treatment I am about to lay before the reader, I have had much opposition to contend with. This I have always regarded as a necessary effect of the nature of that profession, and have borne it patiently. When new plans are every day suggested, of which not one in a thousand proves to be of any value, we are naturally inclined to turn a deaf ear to the sanguine expressions of our brethren, when they

think they have hit upon any improvement of the usual means. But I owe it to them to say, that, with very few exceptions, I have found them as ready to admit the truth as I had any reason to expect, and in many instances anxious to ascertain it. Some, from the cause just stated, have joked in a good-natured way; and I could point out one or two who have sneered ill-naturedly enough—for there are necessarily men of narrow mind, as well as feeling, in every community; but when I say, that, in attempting to modify the employment of one of our most important means of cure, I have every reason to be satisfied with the reception I have met with from my professional brethren, I pay them a just, and, considering the nature of our profession, no trifling compliment; and, let our neighbours say as they will of our jealousies, I have never known any instance in which a sincere and rational attempt to improve the treatment of disease, has not met with a similar reception.

In the following observations I shall, in the first place, consider the *modus operandi* of mercury in general, which is necessary to a clear understanding of the principle on which the minute doses operate—then that of the minute and frequently repeated doses, compared with the usual mode of employing the medicine—and lastly, I shall point out the cases to which those doses are adapted.

Of the Modus Operandi of Mercury.

It appears, from every thing we know of the effects of mercury, and the laws of the living animal body, that it acts in two ways. It has a local and general operation, and its general operation is of two kinds. Like all other substances capable of affecting the living animal, it at the same time operates on the part to which it is applied, and on the system in general through the nerves of that part. But, as it is one of those medicines which are capable of being absorbed, it also influences the whole habit, by circulating with the blood, and thus directly acting on the various organs, by its immediate application to them; and although, as we might have foreseen, from the laws of the animal economy, it is capable, by its action on the part to which it is applied, of affecting every other part, the nervous system forming the

living animal body into a whole, which cannot be impressed in any one part without all others, more or less, feeling the impression, it is in the latter way that it most effectually influences them. Its operation is, more or less, that of a stimulant; for, according as circumstances direct it to particular organs, we find it exciting them to an increased performance of their functions. While it retains the active form in which it is introduced, it seems incapable of remaining in the system. If it be prevented from running off by one excretory, it finds its way by another; thus we see it exciting the skin, kidneys, salivary glands, &c. Like all other metals, in its metallic and insoluble form, it is inert; and it can only remain in the system when deposited in the cellular substance in that form, to which it is reduced by the chemical powers of the constitution; for, in whatever state it is given, these powers always reduce it to its original metallic form. It is well known that gold and silver are amalgamated with mercury, if worn by a person whose system is impregnated with it.

When taken internally, it is doubly applied to the stomach and bowels, immediately, and through the medium of the circulation, for we often have to contend with its irritating effects on the alimentary canal, when it is only introduced by the skin. In this canal and the salivary glands alone its passage excites sensible irritation, which, if considerable, causes inflammation; in the former only superficial, and generally in a slight degree, but in the latter often such as to affect all the neighbouring parts.

In both cases, as it generally increases the natural secretion of the parts affected, the increased discharge, like all other discharges, tends to relieve the inflammatory action; it is where the discharge is least—that is, where there is some impediment to the free operation of the mercury in increasing the secretion from the part, that the inflammatory tendency is greatest.

Such are the more prominent effects of mercury introduced into the system; but I have, in my *Inquiry into the Laws of the Vital Functions*, been at much pains to point out that there is no agent capable of affecting the living animal body that does not possess both a stimulant and sedative power with respect to it, according to the degree in

which it is applied, and the state of the body at the time of its application; the stimulant arising from the less, the sedative from the greater application of it; and that the degree in which each agent possesses the stimulant and sedative power is in no determinate proportion in different agents, but exists in every possible proportion. Thus spirit of wine possesses a great degree of stimulant, compared with its sedative tendency, which only appears when it is applied in excess; while tobacco possesses a great degree of the sedative, and little stimulant tendency, which appears only when it is applied in very minute quantity.

The sedative effect of some agents, as of opium, is chiefly exerted on the sensibility; of others, as tobacco, on the moving powers of the animal system. While the influence of the former, therefore, may be salutary, that of the latter, except under very peculiar circumstances, is always pernicious.

There may be some objection to using the term sedative for agents of both descriptions. In this sense, however, it is used by writers, although not constantly, but I think it is better thus to employ it than introduce a new term, as after this explanation no ambiguity can arise from it. Besides, as both act by diminishing the vital powers, it is convenient that there should be an appellation common to both, and what I am about to say will be sufficiently distinct, without a term to designate either alone. By sedative, then, I mean whatever depresses the powers of the system, whether sensitive or motive, and whether it affects both or either, although the more common use of the term confines it to the agents which impair the sensibility. No agent can impair the sensitive without more or less impairing the motive powers, because the latter in many instances depend on the former; but it is very possible to impair the motive without causing any diminution of the sensitive powers, and even with the effect of a morbid increase in them, because the derangements which accompany the weakened powers of life often prove to the sensitive powers a fruitful source of irritation. Thus that class of sedatives whose operation is on the motive powers alone, are often doubly pernicious.

Mercury, like other agents, possesses the sedative as well as the stimulant

property; and its sedative property appears to be wholly exerted on the motive powers—for when it appears to lessen the sensibility, this effect seems to arise merely from its removing some cause of irritation. Its sedative tendency is very different in different constitutions; and in some it exists to a degree that wholly precludes its employment.

Thus the injurious effects of mercury may be divided into two classes—those which arise from an excess of its stimulant, and those which depend on its sedative effect. By the former it may cause all the evils of extreme irritation; by the latter it tends more directly to impair the powers of life; and these effects admit of every degree, from that of a very mild to that of the most destructive agent, according to the quantity employed, the form in which it is given, and the state of the particular constitution.

Let us now consider what are the virtues of this medicine, which, notwithstanding its injurious tendencies, still render its use in this country more general than that of any other we possess; for it would be absurd to suppose that it had obtained this general employment, without possessing some extraordinary beneficial powers to compensate for its evil tendencies.

It will readily be supposed that a medicine, possessed of so great a power of exciting the various secreting surfaces, must prove a means of relief in many states of disease, especially those attended with a general failure of power in these surfaces. To this effect, for example, we are in a great degree to ascribe its beneficial operation in fever, particularly when it excites the bowels, or is determined to the skin, the most extensive of all the secreting surfaces; and in the various forms of dropsy, and other cases connected with failure of power in the extreme vessels.

In most instances, however, the failure in secreting surfaces is but the secondary part of the disease, depending on some more partial and specific derangement. If therefore the beneficial effects of mercury were confined to its influence on the secreting surfaces, the relief afforded by it would in most cases be imperfect and temporary; and this is often the case, when the original derangement is of a nature which it cannot influence.

But we find in many such cases, that it is often capable of permanent relief; it must, therefore, possess some beneficial tendency besides that of a mere stimulant to those surfaces. It is necessary, therefore, in order to understand the nature of the extensive influence of mercury in the cure of disease, to look for some other principle of action; and in the peculiar effects of this medicine, compared with the well-established laws of our frame, we shall find such a principle.

Although all substances capable of affecting the living animal act as a stimulant or sedative, according to the degree in which they are applied, yet there is in the effect of each something peculiar to itself. Thus we have just seen that the proportion in which they possess the stimulant and sedative powers is different in different agents, and that the latter in some agents is chiefly exerted on the sensitive—in others, on the motive powers. These are differences easily observed and readily classified. But there is an infinite variety both in the stimulant and sedative effects of different agents, which from their number and indistinctness cannot be reduced to any principle of classification; and physicians have attempted nothing further than to divide medicines into those best suited to influence the state of different organs. Thus we speak of aperients, expectorants, diaphoretics, diuretics, &c. and we have no means of knowing the peculiar properties of each particular agent, but by observing the effect it produces.

The most remarkable of the effects peculiar to mercury, is its influence on the liver. It is not surprising that a medicine which so powerfully influences the secreting organs in general, should influence its secreting power; but, independently of this effect, it has a specific operation on this organ, a power not merely of exciting its function, but of correcting the various derangements of that function in a way which it does not possess with respect to any other organ, and which no other medicine possesses with respect to the liver; and that even to such a degree as not only to restore a healthy state of the bile in various deviations of this fluid, but often even to correct the most formidable change of structure in the organ which secretes it.

In my treatise on Indigestion, I have

had occasion to point out at length the intimate sympathy which exists between the stomach, liver, and duodenum, the three chief digestive organs, which so constantly partake of the affections of each other, that all are injured or relieved by causes affecting any one. Thus mercury possessing no particular power of relieving the affections of the stomach or duodenum, and even ungrateful to both, often becomes indirectly the best means of relieving their derangements, so often caused or supported by a disordered action of the liver.

When, therefore, we consider that the sympathies of the digestive organs are more extensive than any other, so that there is hardly any disease of which they do not partake, and whose course is not influenced by them; we are at no loss to find one cause of the extensive effects of the medicine which so essentially influences them.

But a principal cause of this extensive sympathy of the digestive organs, is the peculiar sympathy of the liver itself with the chief source of nervous power, the brain; in consequence of which, all the affections of the one are immediately felt by the other. However severe inflammation of the stomach and bowels, the intellects remain unaffected. I have seen it prove fatal within twenty-four hours, the mind remaining entire to the last; while acute inflammation of the liver is generally attended with delirium. Melancholy even takes its name from a morbid state of the bile; and severe blows on the head are more apt to excite inflammation of the liver than of other organs.

When it was supposed that the office of the brain was chiefly confined to the mental functions, and that its principal relation to other parts was that of bestowing sensibility on them—when it was supposed incapable of directly influencing either the heart or blood-vessels, on which the vital powers so evidently depend, we had a very inadequate idea of the importance of this organ in the animal economy; but now that we know that it is not only capable of directly influencing the action of the heart and blood-vessels, and that to their minutest ramifications in every part of the system, and that the secreting and other assimilating processes are not merely influenced by, but wholly dependent on it and the spinal marrow,

we can easily understand how its affections control all the functions of life*, and can feel no surprise that whatever essentially influences it, should also extensively influence the phenomena of disease.

These are fruitful sources of the influence of the liver in diseased states of the system; but even these are not its only sources. All other parts receive their blood directly from the heart; it receives the principal part of its blood from the other abdominal viscera. It is thus also, as well as by its sympathy with the brain, intimately connected with the whole tract of the alimentary canal, the internal surface of the body, and through it, in consequence of the intimate sympathy which exists between it and the external surface, with this surface also. No affection of either can take place, without more or less affecting it through both the nervous and sanguiferous systems; and by the state of these surfaces, more than any other cause, the phenomena both of health and disease are influenced. The great extent of the liver is also to be ranked among the causes which contribute to its influence in the animal economy, in consequence of which, whatever influences the distribution of the blood in it, more or less influences its distribution in every other part. Such are the conclusions, respecting the influence of the liver, to which we should, *à priori*, be led by a knowledge of the structure and functions of our frame, and they are amply confirmed by direct observation.

All who have had extensive opportunities of observing the phenomena of disease, must be struck with the manner in which the state of the liver influences, and is influenced by them, to whatever class they belong. It is unusual in any formidable disease, whether general or local, not to find the function of the liver more or less disturbed; and wherever it is influenced, the proper treatment of the disease more or less depends on the state of this organ. It is by no means uncommon to find diseases, particularly of the vital organs, intractable till the accompanying derangement of the liver has been observed and corrected; and I have often in such cases seen, not only the patient, but the medical attendant, surprised at the immediate relief thus obtained.

When, for example, inflammatory affections of the chest have been but im-

perfectly relieved by the usual means, and have constantly continued to recur; or the patient has laboured under an obstinate though languid fever, with confusion of mind, and sometimes a low muttering delirium, a fulness and tenderness of the hepatic region have been discovered; on relieving which, by the usual means, the whole of the symptoms have immediately yielded.

Thus it is that in warm climates, where the sympathies of our frame are most active, not only in all febrile diseases, but even in all chronic deviations of health, affections of the liver become the leading feature.

I have for many years past, in every case, whether acute or chronic, been in the habit of examining the state of the hepatic region as regularly as that of the pulse; and I think all who will take the trouble to do so, will confess that the one examination is often of as much importance as the other, and in many cases the former the most important of the two.

It is the sympathy of the liver with the general source of nervous power, and the other circumstances relating to this organ which have been enumerated, that, even more than the sympathies of the digestive organs in general, gives to the medicine that so powerfully controls it, its extensive influence in the treatment of disease.

Such I conceive to be the causes which have rendered the employment of mercury so general in the practice of this country; which, resting on more extended as well as more accurate principles, it is not assuming too much to say, is more effective than that of perhaps any other. If the state of the liver be so extensively connected with that of all other parts of the system, and, in particular, so powerfully influence the other digestive organs, with all their extensive sympathies; is it surprising that a medicine which has so great a power in controlling the affections of this organ, should hold a chief place amongst the means of cure; and that in the country where the practice of medicine is best understood, it should be found in most general employment?

It will be admitted from all that has been laid before the reader, that, to say nothing of its effects in the disease for which it was first introduced, the treatment of which chiefly belongs to the surgeon, it is well worth while to inquire into the best mode of employing so essential and powerful a means; and

* Inquiry into the Laws of the Vital Functions.

this is the more necessary, because, like other means capable of great good, it is also capable of great injury. Its beneficial effects have naturally led to too incautious a use of it; by which much mischief has been done, and the remedy itself, with those who either lack opportunities or correct powers of observation, brought into discredit.

The result of my own experience (and there are few whose attention has been more directed to the subject) is, that although there are many circumstances under which large doses of mercury are not only beneficial but essential; the quantity employed in this country has on the whole been at least ten times greater than that from which its most beneficial effects would accrue. It unluckily happens, that, in a large proportion of cases its most beneficial employment is not always that which produces the most immediate benefit; and in grasping at too much, we often not only lose the advantage of the remedy, but convert it into a source of injury.

It is a law of its action, that when it is directed to one outlet it is less inclined to pass by others; thus, when it is passing off rapidly by the skin, which is known by the scarcity of urine, —if, in consequence of taking cold, or an increased quantity of the medicine, it is thrown on the salivary glands, the usual secretion of urine is restored, indicating that it is no longer powerfully exciting the skin, or perhaps increased, for the salivary glands affording but a narrow outlet, it still in part tends to pass by other channels. It seems to be on this principle that salivation renders it more powerful with respect to the disease; but it seldom produces this effect unless the system is highly impregnated with it, and then—to say nothing of the irritation occasioned by the salivation itself, which is often great—its sedative effect is frequently much felt, and the whole powers of the constitution are, for the time, enfeebled by it.

It was a maxim of the older practitioners, that its beneficial effects are proportioned to the degree of salivation it excites; and I have heard the late Dr. Monro, of Edinburgh, state the quantity of saliva which must be discharged daily, in order to eradicate particular affections. Can we be surprised that, when such maxims prevailed, the remedy proved sometimes

strong a prepossession against it has arisen?

I have said that before salivation takes place the system is generally much impregnated with the medicine. This is not always the case. In particular constitutions the smallest dose immediately affects the salivary glands. Thus, although in general we find mercury most effectual when it produces salivation, in some habits this occurs so readily as wholly to preclude its employment, and consequently to render it useless as a remedy. In others the same consequence ensues from the sedative effect immediately arising from such minute doses that, from this cause also, its injurious effects alone are attainable.

The following are the most remarkable instances of these peculiarities which I have met with. From the fear of hurting the feelings of individuals in the few remarkable cases I shall have occasion cursorily to relate, for the purpose of illustration, I shall abstain from giving the names. This is the more necessary, because in a large proportion of these cases the patients were members of families well known to the public. I am ready, however, to state them, and any other circumstances of these cases, to any respectable practitioner.

I was requested to see a lady, whose apothecary informed me that although her case was bilious, she had been obliged to abstain from mercurial medicines, in consequence of only half a grain of blue pill, and that, after she had for some time recovered from the effects of former doses, having occasioned salivation. Finding that her mouth at the time I saw her had been well for some weeks, I advised him to repeat the mercurial, but still to lessen the dose. She took a quarter of a grain of blue pill on going to bed, and the next day was in a state of salivation.

In the following case the degree of sedative effect produced by all mercurials, equally precluded their employment. A lady had long laboured under bilious affections, which were gradually impairing all the powers of her constitution. The cause of their obstinacy was, that she was incapable of bearing even the smallest doses of mercury without a degree of irritation and depression which had precluded its employment; and no other means had been found an effectual substitute for it. Calomel, on the whole, seemed to produce less depression than the blue pill;

dose was more and more reduced, her husband and myself, that we might be assured of the accuracy of the medicine, saw one grain of calomel equally distributed in eighty pills of extract of liquorice. One of them was given to the patient without letting her know that it contained any mercurial, and in two or three hours she said she knew she had taken mercury, for she felt the symptoms which nothing else ever produced. She afterwards visited various parts of England, for the sake of change, and was under the care of various physicians, who all attempted to find some mode of exhibiting mercury which she could bear, but in vain; and her disease proved fatal for no other reason than that she was unable to bear the only effectual means of relief; for in the first instance it was nothing more than the common bilious affection which, in the beginning, in most constitutions yields with certainty to a very moderate use of this medicine; but even this slight affection we had no means of arresting without it, so that it gradually preyed on the constitution. The diseased action of the liver by the laws of sympathy extended to other organs, and at length destroyed all the powers of life.

No case can more strongly evince the value of the medicine. This lady died because her constitution was wholly unfitted for it, and we had nothing which could supply its place.

Those who are prepossessed against it and other powerful medicines, in their fear of the medicine are too apt to lose the fear of the disease. They are insensible to the risk of delay, which often converts a disease of easy cure into one altogether hopeless.

The effects of mercury, we have seen, may be divided into two classes—its local and general effects. Many of its effects, both good and bad, depend on its operation on the parts to which it is applied. It may appear at first view extraordinary that any of the effects of mercury should arise from its action on the particular part of the skin to which it is applied, and any effect which can be ascribed to this cause I believe to be very slight; but, under certain circumstances, it is sensibly felt. We know, that however free from any direct communication, either by nerves or vessels, neighbouring parts of the animal body may be, they never fail to sympathise, one among many proofs that the

power of sympathy is referable to the central parts of the nervous system; and although a mercurial plaister worn on the region of the liver be too feeble a remedy to be sensibly felt in a case of severity, and where more powerful means are employed; yet in removing the last remains of debility of the liver, and in preventing its return, I have found it a means of considerable efficacy; and have known instances where health was preserved by it, and the patient constantly had a recurrence of bilious symptoms on laying it aside. I am inclined, from what I have seen, to ascribe a great part of the effect of this remedy to the local effect of the mercury in the neighbourhood of the liver. A very minute portion of the mercury, no doubt, is absorbed*, but many times the same quantity thrown into the system in any other way would not produce the same effect. To the same cause we must, I think, in some degree ascribe the greater efficacy of mercurial friction on the region of the liver in confirmed organic disease of that organ, than of any other way of employing this remedy. Something here is doubtless to be ascribed to the friction; but I believe the same friction on this part will not render other modes of exhibiting the medicine equally efficacious.

Whatever may be said of the local effects of mercury applied to the skin, there can be no doubt of its local effects, both good and bad, when applied to the stomach and bowels; and I shall say the more on this subject, because I have found it one of the points of most consequence in regulating the employment of this medicine.

It is the circumstance which appears to render the internal so much more beneficial than the external use of mercury, where it is not our object to produce any considerable impregnation of the system.

We have a familiar instance of the effect of the local action of mercury on the stomach and bowels, in controlling the action of the liver, in that of a few grains of calomel allowed to pass through the canal, however rapidly, in emulging the ducts and restoring the due secreting power of this organ. To the same cause I ascribe the greater efficacy (which I have ascertained by many trials) of the

* I have seen the gums affected by a mercurial plaister.

same dose when an aperient is taken six or eight hours after the mercurial than when it is taken with it; in which case, it is too much hurried through the stomach and first intestine to produce its full local effect on the liver.

Hence the good effects of the usual practice of giving the mercurial at night and the aperient in the morning. A few grains of blue pill given at night, and carried off by an aperient in the morning, will have a decidedly greater effect in restoring the action of the liver than when the aperient is taken with it, although the mercury may remain as long in the canal. In the latter case, it is conveyed more quickly to the lower bowels, which have not the same sympathy with the liver as the stomach and duodenum have. With calomel the difference, though still perceptible, is less evident; because the calomel, by its own action, passes more quickly than the blue pill, and is on this, as well as some other accounts, as I shall have occasion more particularly to point out, a less beneficial alternative.

This mode of employing mercury was very generally adopted by Mr. Abernethy. He gave a few grains of the blue pill every second night, and carried it off by an aperient in the morning; and his employment of these means was so general that he has been accused of an almost indiscriminate use of them. How far this accusation is well-founded, it is difficult to judge; but many who smiled at his practice were less acquainted with the laws of the animal economy. He knew, and has shewn, in his excellent treatise *On the Constitutional Origin of Local Diseases*, how various, and often even opposite, are the effects proceeding from the state of the digestive organs, depending on their extensive sympathies and peculiarities of constitution; and he found his mode of exhibiting mercury powerful in relieving them, and his experience in this respect I have found amply confirmed by my own. But I shall, in a future part of this communication, attempt to point out the limit to which its efficacy extends, which he had not ascertained; and the plan of treatment which must then be adopted, in order to produce the same good effects. The two plans have this in common, that they are both arranged with a view to avoid the sedative effects of the medicine; which is an essential princi-

ple in its employment in all cases, except where life is in immediate danger, and for want of other means such a contest between the disease and the remedy becomes necessary as sometimes almost makes it doubtful from which the risk is greatest. Such a use of mercury is only justifiable where we know the continuance of the disease to be certainly fatal.

From the local effects of mercury little is to be apprehended, if the dose be not too frequently repeated. In this it resembles the effect of an emetic. If an over-dose be given, the only effect in general is, that it operates the more speedily and effectually. In this way we may explain the alleged safety of doses of twenty or thirty grains of calomel, of which I have no experience, but which, some years ago, were not unfrequently given in this country; the rapidity with which they were carried off compensating for the greatness of the dose. Various accidents, however, may render such excessive doses unsafe, and I believe they are now little employed, and that, in this country at least, all their good effects may be obtained by much smaller quantities. It is from the constitutional operation of mercury, however—or I would rather say its constitutional and local operation combined, the former always more or less including the latter—that its most important effects, whether good or bad, arise.

It is a remarkable fact, that as there are some constitutions in which mercury, as we have seen, even in the smallest doses, acts as a poison; there are others which will bear an excessive use of it without injury; as in some the sedative effect arises from the most minute doses, in others it seems incapable of any great degree of this effect in the largest quantity in which we ever employ it. The following is the most remarkable instance of this kind I have met with, which I shall concisely lay before the reader, before I enter farther on the principles on which this medicine appears to operate.

The patient was a boy of about twelve or thirteen years of age, labouring under dropsy of the belly. All diuretics had failed, and several gallons of water were drawn off from him by tapping. The liver was found indurated, and occupying a large portion of the flaccid abdomen, and the rest of it felt like a

cluster of grapes; so much enlarged and indurated were all the smaller abdominal glands. In about eight days, although the patient drank but little, a collection of water to the same amount was formed. Such had been the rapid absorption of moisture from the air by the skin and lungs, the only source from which it could be derived. All diuretics still failing, I then, in conjunction with a well-informed and highly experienced apothecary, told the father of the patient that the only chance of recovery was a mercurial course, carried as far as the constitution could bear. I warned him that its effects would be severe, and its success doubtful; and proposed a consultation of physicians before entering on the plan. This he declined, and placed his only child wholly in our hands. The patient was of a scrofulous habit, which I have, contrary to what we should at first view have supposed, generally found favourable to the use of mercury. It may easily be supposed that, under such circumstances, we found great difficulty in producing an affection of the mouth. The apothecary, although advanced in life and in the most extensive practice, had never seen so great a quantity of mercury used as in this case. It was used both externally and internally.

As soon as the mouth became decidedly affected, the diuretics began to act; and in the space of some months the whole of the water was discharged in the natural way. But we had an obstacle to contend with which still threatened a fatal termination. As often as we attempted to lessen the mercurial influence, the water again began to collect. This I regarded as proving the glandular obstruction not to be yet wholly subdued, although the abdomen had now the natural feeling to the hand; and the question was, whether the patient could bear the mercurial course long enough wholly to subdue it. In the meantime we were encouraged to proceed, as his strength, as well as his health, continued to improve under it. In all, he was kept in a state of salivation for a year and a half, before it was possible to lay aside the mercury without a return of the disease. At the end of this time he was free from complaint, and remained so without the further aid of medicine. But the most remarkable circumstance of the case was, that, although he had

always been so sickly a child that from his birth there were hardly any hopes of rearing him, he became, after this course, one of the stoutest and healthiest young men I knew; and the only bad consequence of so unusual an employment of mercury, was the loss of two or three of his grinders, which came out during the long protracted salivation. No case can more strikingly illustrate the power of mercury in glandular disease, under which it was evident this boy had laboured from birth.

If we except the function of propelling the blood, all the functions on which life depends take place in the minute extremities of the nerves and blood-vessels; and even the motion of the blood in the immediate organs of those functions depends on the vessels employed in them, and in no other respect on the heart and larger vessels than that from them they receive their supply of blood. Till the supply fails, the circulation goes on in the capillaries, even after death; and with the same vigour although a ligature is thrown round all the vessels attached to the heart, and this organ cut out, as when the circulating system is entire*. Hence the emptiness of the arteries some time after death; and if the capillary vessels of any part, during life, be deprived of their power by causes confined to them, the blood immediately becomes stationary in them, although the heart and every other part of the animal be entire and vigorous†.

Now the great value of mercury arises from its influencing the action of the extreme parts of the nervous and sanguiferous systems more powerfully than any other medicine we possess; and to the same cause, as appears from what has been said, all its injurious effects, with the exception of the sedative effect in which it appears to operate on all parts of the system, may be ascribed. The discharges by which it debilitates are from secreting surfaces, and it is by its effects on them that it occasionally causes so much irritation; and the greater liability to the effects of taking cold, produced by it, is from the open state it maintains in them.

Let us inquire under what circumstances it is most apt to produce its various effects, and by what means the fa-

* Inquiry into the Laws of the Vital Functions.

† Ibid.

vourable may be secured, and the unfavourable guarded against.

ITS rapid effects on the extreme parts of the nervous and sanguiferous systems, and particularly those of the liver, render it a powerful means of temporary relief under a variety of circumstances; and the permanency of its effects, under proper management, bestows upon it great power as an alterative.

Of its various preparations, some are better adapted to the former, others to the latter effect. Calomel is best adapted to produce the more rapid effects, particularly the sudden excitement of the liver and the secreting surface of the alimentary canal; but its milder preparations answer better as an alterative. Peculiarity of constitution is sometimes as remarkable in the different effects of its particular preparations as in the general effects of the medicine itself. In some, the smallest dose of blue pill occasions nausea. I have almost always found, in such cases, that the stomach bears calomel better: but it is more apt to irritate the bowels; and from this cause, and probably its passing more quickly along the alimentary canal, it does not answer the purpose of an alterative so well. The tendency of mercurials to oppress the stomach is generally found in the opposite proportion to their activity. Blue pill is lighter than the hydrargyrum cum creta, calomel than blue pill, and oxymuriate of mercury, which can only be given in extremely small doses, will often agree with the stomach when all other mercurial preparations oppress it. It is only, however, in a few constitutions that the blue pill, especially in small doses, has much of this effect, and it is, on the whole, the most convenient, as well as effectual alterative. It is more effectual, and in general lighter on the stomach, than the hydrargyrum cum creta, without being much more inclined to irritate the bowels, and it has much less of this tendency than the more active preparations.

On the more temporary effects of mercury, I need not dwell. Every medical man is aware of the benefit often derived from suddenly exciting the liver and determining the fluids downwards by its operation on the extensive surface of the alimentary canal. Here, except in constitutions peculiarly sensible to its sedative effect, any injury that may arise depending simply on the irri-

tation and discharge it occasions, may be lessened, by its less frequent employment; but the chief difficulty here is to judge when the peculiar effects of this mode of exhibiting the medicine are obtained, and we are called upon either to abstain from it altogether, or employ it in a different way.

The sudden advantage often obtained by this mode of exhibiting mercury, is flattering both to the practitioner and to the patient; and I have often seen it persevered in when its debilitating effects greatly exceeded any advantage derived from it; for from the nature of this mode of employing it, its benefit must always be greatest at first, when the accumulations, whether of blood or vitiated secretions, are greatest, and the patient's strength most entire.

After the due distribution of the blood is restored, and the vessels of the liver emulged, there is nothing more to be expected from the temporary effects of this medicine. It is only necessary to renew them when the evils which called for them recur. If the removal of these evils do not restore the patient, we may be assured that he labours under a permanent derangement of function somewhere, if not of structure also, which may be aggravated, but cannot be counteracted by such means; and that if the fault be such as mercury is capable of correcting, it must be by rendering the effects of the medicine, like the disease it is intended to relieve, more permanent. We must abandon the hope of doing much in a little time, and attempt by gentler means to solicit a return of the healthy action. Under such circumstances the practitioner always finds that as the benefit of his active doses is lessened by their repetition, their debilitating effects increase, and soon begin to compensate, and at no great distance of time more than compensate, for any advantage derived from them. He is thus obliged to relax his plan, and not unfrequently wholly to lay aside the remedy before the disease is subdued.

Many of the same observations apply to an alterative course, when the doses are considerable. If these also fail to afford permanent relief, and the disease be one of immediate danger, requiring the prompt and powerful effects of mercury, and such as no other medicine can produce, the chance of recovery is then at an end; and we have nothing more in our power than to hus-

hand the remaining strength, and contribute to the patient's comfort for the short time that he has to live; but if the disease be more or less of a chronic nature, we have still a resource. Time may be given to effect by degrees what cannot be effected rapidly.

The question is, whether in lessening the dose of the medicine, we lose as much as we gain; whether in rendering it less debilitating, we in the same proportion render it less effectual. This we have reason to believe is the case with respect to each particular dose, but we obtain the great advantage of being able without injury to repeat the dose more frequently, and however small may be the beneficial effect of each dose, if we can give another before that effect is wholly lost, the benefit necessarily accumulates.

Such are the principles which gradually led me to minute, and frequently repeated doses of mercury. It remains to consider more particularly their *modus operandi* as compared with that of larger and less frequently repeated doses, and point out the cases in which I have found them most beneficial.

Of the Modus Operandi of minute and frequently repeated doses of Mercury.

I had occasion to observe, in speaking of Mr. Abernethy's plan of exhibiting mercury in diseases of continuance, that when it fails there is another which is often successful. This consists simply in greatly lessening the dose, and in the same proportion increasing its frequency.

The benefit derived from this change depends on the most fundamental laws of our frame. I have already had occasion to point out that all agents capable of affecting the living animal body, act both as stimulant and sedative, according to the quantities employed, and we call them by one or other of these names, according as they are best fitted to produce the one or other effect.

Spirit of wine we call a stimulant, because in all moderate quantities it produces the stimulant effect, but there is a quantity of it, as appeared in the riots of Lord George Gordon, when many of the mob took draughts of spirits of wine, mistaking it for common gin, which produced instant death without any degree of previous excitement. Digitalis we call a sedative, because in

ordinary doses it acts as such, but it appears from very accurate experiments that in extremely minute doses it acts as a stimulant.

Mercury is one of those agents in which the stimulant and sedative effects are so nearly balanced that neither predominates so much as to obtain for it either appellation, but it observes the same law as all other agents capable of affecting the living animal body. In small or moderate doses the stimulant effect prevails; in excessive doses the sedative; and the repetition of even moderate doses, unless at very distant intervals, seldom fails at length to produce more or less of the latter. Is there a dose, then, so small as to produce little or no sedative effect, and yet capable of the stimulant effect on which we have reason to believe the beneficial tendency of this medicine always depends? Is there a dose in which, in most constitutions, the sedative effect either does not take place, or takes place so slowly that the cure may be far advanced before the patient begins to experience any degree of it?

The result of my experience is, that there is such a dose.

With respect to its amount, it varies from half a grain of blue pill, the largest dose I almost ever employ with a view to the alterative effect, to the eighth part of a grain, the smallest dose from which in general much good can arise, though there are cases in particular constitutions in which I believe much smaller doses than even this will be found beneficial, and the interval at which the dose has been given is from six to eight hours. By these doses, given at such intervals, we can in most constitutions, and for a considerable length of time, in some for an unlimited time, obtain the stimulant without the sedative effect of the medicine, which is the great object in the employment of mercury. We thus, as far as the particular constitution is capable of it, secure the whole of its beneficial, without any degree of its injurious, effect.

As soon as the latter is perceived, it must either be obviated or the medicine laid aside, for nothing will compensate for the sedative effect of this medicine, whether produced by large or small doses. It is as certainly injurious as the disease, but as in the case of the medicine, it is always in our power to remove the offending cause; in the hands

of a prudent practitioner we have the chance of benefit without the risk of injury, unless the certainly fatal nature of the disease makes it advisable to risk to a certain extent the sedative effect of the only means of cure.

With respect to the minute and frequently repeated doses, I would say without hesitation, and from ample experience, that in all cases except where the great discharges, or other rapid effects this medicine is capable of producing are required, they are not only the safest but the most effectual mode of exhibiting it. Their operation appears to be that of a gentle but constant stimulant, exciting to due action the various organs of assimilation, and particularly the liver. The following case is strikingly illustrative of their power in producing the peculiar effects of the medicine, and I could add many others of a similar nature.

A lady came from a great distance to London, for the purpose, she said, of being salivated, which she had been told would cure her of a bilious complaint, under which she had laboured for many years. For this purpose she had taken in vain, in the country, very large doses of mercury, much beyond the largest usually given in this climate. I saw no occasion for salivation, but directed for her, with other means, half a grain of blue pill three times a day. Her case did not require frequent visits, and not being then so well acquainted with the effects of the plan, I thought, as the mouth had resisted such doses, that no precautions respecting it were necessary, when, at one of my visits, after she had taken the medicine for about a fortnight, I found her in a state of severe salivation, the whole of the face was swelled, and she was for a considerable time confined to bed. At no great distance of time she left London well, and I learned from her sister, who two years afterwards was placed under my care, that she remained so.

It is not difficult to perceive the principle on which the minute doses are so powerful. From their little aperient tendency they readily enter the system, and, from the little irritation and excitement they occasion, they are not apt to be thrown off by it. Such cases as the preceding, and I could mention many, in which a certain affection of the mouth arose from even smaller quantities, which, had they

been continued, would soon have produced the same effect as in the preceding case, prove that the peculiar effects of mercury may be obtained in any degree we please by such doses. What, then, can be the motive for employing larger ones, unless, either from the nature of the symptoms or the urgency of the case, the more active effects of the medicine, or the more rapid impregnation of the system be required; or, as happens in certain diseases, the powers of the absorbing vessels are so impaired, that minute doses are no longer capable of exciting them, and therefore cannot enter the system.

The cause of the minute doses sometimes producing an effect on the gums, when larger doses fail, arises from the latter being so much more powerful in exciting the excretories, by which, in certain constitutions, they are often thrown off as fast as they are taken, and thus little impression is made either on the mouth, or the disease. It is to the thorough manner in which the minute doses are received into, and retained in the system, and the general, steady, and gentle impression they make, that they owe an efficacy which surprises those who have not been accustomed to see their effects.

Such doses, of course, have little effect in suddenly emulging the biliary ducts, and thus discharging collections of vitiated bile, and, until they succeed in restoring the due action of the liver, which in general requires some time, these collections are in many cases more or less apt to form under their use, and occasionally to require the operation of a more active mercurial, the necessity for which is different in different cases, according to the tendency to such accumulations. Where there is no tendency of this kind, the active dose is unnecessary, and its frequent repetition is seldom proper. Calomel, we have seen, generally answers the purpose of the larger dose better than the blue pill; although, in the more obstinate cases, I have sometimes found a combination of the plan I am now describing, and that of Mr. Abernethy, the most successful.

Of the Cases in which the minute and frequently repeated doses of Mercury are adapted, and the circumstances to be attended to in their employment.

I am now to point out more particu-

larly the cases, both acute and chronic, in which I have found minute and frequently-repeated doses of mercury most successful; and the various circumstances to be attended to in their exhibition. I shall, in the first place, speak of the chronic cases. In them I was first led to this use of mercury, and its effects in them gradually led to it in acute cases.

It appears, from what has been said, that the cases in which mercurial medicines are most successful are those (a very numerous class) in which the state of the liver is more or less essentially concerned.

Indigestion, which forms an important part of so many chronic diseases, either as a cause or a consequence, generally begins when it is the original disease, at least in this country, with debility of the stomach, which spreads to the liver and duodenum, and thence, more or less, to the rest of the canal. While it is confined to the stomach, it is for the most part readily relieved, by a proper attention to diet, and what are called stomachic and tonic medicines. If not relieved, the debility always spreads to the liver.

The disease, being then of a complicated nature, begins to obey different laws. The various affections of each organ by sympathy influence the other, and the evil increases in a double proportion, and will no longer yield to the simple means, in the first instance generally successful.

The action of the stomach is now more impeded, by the effects of the disordered liver, an organ of more intimate and extensive sympathy than the stomach, and which must be restored before the latter can recover its tone, than by its own original debility; and thus, in cases of long standing, the cure depends more on the relief of the liver than of the stomach itself. The affection of the former not only renders the symptoms both more complicated and more severe, but also more obstinate. It is the chief impediment in our way.

The greater part of the suffering in such cases, indeed, depends more directly on the state of the liver than the stomach, arising from its influence on the centre of nervous power, and the action of the first intestine immediately depending on the state of the bile. The state of this intestine is the best, and, as far as I know, the only accurate mea-

sure of the due action of the liver. In proportion as the bile deviates from its healthy properties, the action of the duodenum languishes; it fails duly to carry on its contents, allowing the imperfectly digested food to accumulate in it, which causes a fulness in the region of this organ that may always be readily distinguished by comparing it with the corresponding part of the left side, and proving a source of a great part of the nervous irritation which attends protracted cases of indigestion*.

As the fulness of the region of the duodenum is here the best measure of the state of the liver, it is necessarily the best measure of the effects of the alterative. As the languor of the duodenum arises from a vitiated state of the bile, it is reasonable to suppose that a more healthy bile will relieve it; and we find, in fact, that precisely as the due action of the liver is restored, the activity of this intestine returns, an effect not to be procured by any ordinary aperients, however powerful, and without which it is in vain to expect any material abatement in the symptoms of the disease.

Now it is under such circumstances that I have found the minute and frequently-repeated doses of mercury so effectual in indigestion, and the various cases which arise from, or are supported by, this state of the digestive organs; and in the more obstinate cases in which habit has confirmed the disease, the larger doses, necessarily given at longer intervals, almost uniformly fail, so that it has been customary to regard them as incurable; and when the disease is confined to the digestive organs, the patient is advised to be cautious respecting diet, keep the bowels regular, amuse the mind, and think as little of his complaints as he can—the last an attempt in which he is generally most unsuccessful; for, from the great sympathy between the liver and brain, he generally broods over them, till, by degrees, they occupy his whole attention.

* This subject is fully considered in my Treatise on Indigestion, and my reasons pointed out for believing that the fulness in question arises from the state of the duodenum. But whether it arises from the state of this intestine or not, I know, from the careful observation of more than twenty years, that its degree may be confidently relied on as a measure of the state of the secreting power of the liver, and I believe there is no other such measure: all others I have found more or less fallacious. By practice the hand becomes sensible to the slightest morbid fulness of the region in question.

Here, in such cases, is the limit at which I have found Mr. Abernethy's plan almost uniformly unsuccessful, and where the employment of the minute doses, as far as I know, is the only effectual means of relief.

I have, in my *Treatise on Indigestion*, been at great pains to point out a change which almost always takes place in the progress of that disease, and, if we except the state of the liver, more than any other circumstance influences the treatment of protracted cases. The long-continued nervous irritation which attends it, here, as in all similar cases, produces more or less of an inflammatory tendency, and a tenderness on pressure is generally perceived in the region of the pylorus and left edge of the liver. The pulse always becomes more or less tight, and the patient often, especially in the evening and early part of the night, experiences more or less tendency to increased heat, which frequently shews itself in a burning of the hands and feet, although this tendency is by no means so uniform a symptom as the tight pulse. The state here described essentially influences the effect of the mercurial. I have had occasion to point out that the favourable effect of mercury is its stimulating effect. The more inflammatory the state of the habit, the less, of course, it is suited to this effect.

The larger the dose this is the more felt, and it is one of the advantages of the minute doses, that they have little tendency to increase the inflammatory disposition; but still their beneficial operation is sensibly impeded by any considerable degree of it. Every practitioner has remarked the proneness of mercury to produce a feverish state, and that in inflammatory cases it is chiefly useful when given in such a way as to occasion a copious discharge from the bowels, which compensates for this tendency.

I have found the tightness of the pulse no obstacle to the exhibition of the minute doses, which never appear to increase it, and often, especially when they act much on the skin, sensibly relieve it; but when the inflammatory tendency goes so far as to occasion much tenderness in the region of the pylorus and left edge of the liver which lies upon it, where the tenderness always in such cases, as I have just observed, first shews itself,

the operation of the alterative is greatly impeded; and it is necessary by local means to relieve this tenderness, in order to obtain its usual good effects.

It is of great consequence in all plans of treatment, to render them as effectual as possible on their first employment. When the means are of an active nature, there is a necessary limit to their repetition; and where they are such as may continue to be borne without injury, their effect is impaired by their continuance. The constitution becomes accustomed to the favourable, as well as unfavourable effect of medicines, and the first impression is always the most powerful.

In having recourse to the minute doses of the alterative, therefore, we must correct as quickly as possible all the causes which counteract their operation. Besides, in many habits their long-continued use gives rise to more or less of the sedative effect, which, when it once occurs, is always more readily produced afterwards.

Of the means of relieving the tenderness of the epigastrium, local blood-letting is the most powerful; and it is of great consequence to abstract the blood as much as possible, from the most tender part.

I have had occasion to refer to that law of our frame, by which all neighbouring parts sympathize. In bleeding from the skin in the neighbourhood of a diseased organ, we take no blood directly from the vessels of that organ, but they so immediately partake of the state of the neighbouring vessels, that a few ounces of blood taken from the latter will often give greater relief in local affections, particularly those of a chronic nature, which depend more on the distention of the vessels of the part than any morbid increase of the force of circulation, than many times the quantity taken from a distant part, and which can affect the diseased part only through that medium. I have even repeatedly found, that in other cases where the tender part is one of considerable extent, the relief from the local blood-letting has been chiefly felt in its immediate vicinity; so that it has been necessary to repeat it, in order to relieve other tender parts of the same region.

Every practitioner is aware of the essential difference in the effects of blood-letting and blistering in acute cases—the one being better calculated to relieve

the distention of the vessels, the other to excite them to a due performance of their function; and in cases of great excitement the latter, without the necessary previous blood-letting to reduce the general excitement, is often even injurious, by its irritation tending to increase that excitement.

In chronic cases, where there is little or no increased excitement, these different tendencies are less apparent, but they equally exist. In proportion as the tenderness is great, local blood-letting becomes the more beneficial; and in proportion as it is trifling, and the lesion of function great, more advantage is to be expected from blistering. To this observation, however, there is one exception: local blood-letting, like all other remedies, loses its effect by frequent repetition. The sympathy by which its effects take place, active at first, becomes weakened in protracted cases; and did it still equally operate, the vessels of the part, by long-continued or frequently-repeated distention, have their contractile power impaired, and often become incapable of maintaining the healthy diameter against the force of the circulation, even when they are capable for the moment of attaining it. Thus it is that in old cases, even when the tenderness continues to be great, or great tenderness has frequently recurred, local blood-letting often proves to be of little service. Whatever be the degree of tenderness, therefore, blisters are then the appropriate remedy; and if they have not frequently been repeated, are generally in a greater or less degree successful, when local blood-letting has failed. When in long-protracted cases both fail, a permanent drain from the part is often of service. This, however, is better suited to remove the last remains of the disease, and prevent its recurrence, than to assist while the symptoms are urgent.

If we cannot by such means relieve the tenderness in the seat of the disease, the effect of the alterative will be greatly impeded; but when the tenderness is not very great, the alterative itself, by exciting the part to the due performance of its functions, tends to relieve it; and local measures will never, in the cases to which the alterative is adapted, finally subdue the tenderness without its aid.

In this effect it may, in most cases, be greatly assisted by other medicines.

Even in the most chronic cases, where the tenderness is great, it is generally accompanied by some occasional tendency to increased heat; and however rare, and of short continuance, this tendency may be, I have found that it always more or less indicates the employment of some saline medicine, and nitrate of potash I have found the best, where it does not disagree with the stomach, which in a few constitutions it is apt to do in a great degree. In almost all cases I have found its effect improved by the addition of a little mucilage, which tends to defend the stomach against its tendency to irritate. A large quantity of mucilage generally oppresses, and I have known a few who were oppressed by the smallest quantity. In one case even five drops of mucilage of acacia always produced this effect.

Saline medicines essentially aid the alterative, by more effectually subduing the inflammatory tendency. Where the recurrence of the heat is at all frequent, we shall with their aid succeed with half the quantity of mercury which is necessary without it.

The regulation of the saline medicine in such cases often requires great consideration. If more than is necessary is used, it tends to debilitate, where its constant employment is long required; and if the heats be allowed to recur, they not only essentially impede the cure, but debilitate more than the means which relieve them. I have generally found from six to twelve grains of the nitrate of potash, given with each dose of the alterative, sufficient; and in the cases which require it, by reducing, and at length for the most part preventing the recurrence of the heats, it has often essentially contributed to restore the strength; and in many cases, more than any opiate, to secure good nights; for, as I have just observed, it is in the nights, and particularly in the early parts of them, that the heats are most apt to recur, and they never fail to occasion more or less oppression and restlessness. When there is no tendency to them, if the patient be tolerably vigorous, the effects of the alterative will still be promoted by its combination with small doses of saline medicine; but where there is much debility, unless the tendency to heat be considerable, and in all cases where, as sometimes happens, there is a constant tendency

to chilliness, the latter should be wholly abstained from.

These are the cases in which stimulants and tonics are best borne and of most use. In most cases, however, light stomachic medicines, when they have no heating tendency, should be combined with the saline medicine; they do not seem to impair its alterative, and what is remarkable, unless they are of a very heating nature, or the constitution unfavourable, often very little, its cooling effect.

The use of stimulants and tonics, under the circumstances I am now speaking of, is greatly influenced by constitution; some patients bearing them a great deal better than others. Their due regulation is quite as essential as that of the refrigerants. As debilitating measures are only to be employed as far as they are necessary, strengthening means, on the contrary, are to be employed as far as this can be done without injury.

The more purely stimulating medicines, the effect of which is immediate, and in a great degree transitory, may generally be employed with little precaution wherever a sense of debility prevails, although there are some constitutions which cannot bear even these without increased heat and its usual accompaniment—a sense of oppression; and I have seen many who greatly required their cordial effect, thus wholly precluded from them; for there is nothing more injurious, in the cases I am speaking of, than a perseverance in any thing which has this effect. It never fails to increase the inflammatory, the worst tendency of such cases. In some instances, where other stimulants cannot be borne, the fetid gums (which possess a soothing effect with respect to the nerves) are borne without inconvenience, and prove highly beneficial. A combination of assafoetida and castor, I have, on the whole, found the best.

In almost all cases, except where there was much tendency to affection of the head, it has been found advantageous, whatever other means are employed, to combine with the alterative a slight anodyne; for it is always of consequence to allay irritation, the source of the inflammatory tendency, and consequently of the heat and restlessness. It is on this principle that a slight opiate often assists the refrige-

rant. Hyosciamus has appeared to answer best. It has little tendency either to obstruct the bile or impede the bowels. In some constitutions it is aperient, and even in doses of two or three grains may be used in place of other cathartics; and in irritable habits I have sometimes found it the best. When a more decided opiate has been required, the compound powder ipecacuanha has appeared, on the whole, the best; and Battley's anodyne liquor, the black drop, and the acetate of morphia, will sometimes agree with the patient, when simple opium or laudanum will not. Of these preparations, some suit one constitution and some another; and I have seen a few instances in which simple opium was the least offensive. Whatever be the opiate, it is only in very minute quantity that it is serviceable. I have known great advantage derived from a combination of hyosciamus with the fetid gums, especially where the former was aperient, which has produced a degree of composure no other means afforded.

As there are some constitutions which, in the cases I am speaking of, cannot bear the smallest doses of the most transitory stimulants, so there are others which can bear even the continued use of the most permanent; which is extremely rare.

I cannot help here remarking that one of the greatest errors (and perhaps of all errors the most frequent) in the treatment of such diseases, is making general inferences from the effect of medicines in particular cases. The great principle of distinction between the practice in acute and chronic diseases, is, that the latter are infinitely more influenced than the former by peculiarity of constitution. The causes which produce acute diseases are so powerful and sudden in their effects, that they obscure the operation of all concurrent causes; and, let the constitution of the patient be what it may, very nearly the same plan of treatment is applicable in all cases. In chronic diseases it is otherwise. The cause being less powerful, and operating less rapidly, its effects are essentially influenced by such causes. The most unobservant must perceive how much chronic diseases are influenced by situation, and particularly by change, or whatever else is capable of a powerful or permanent impression;

and yet peculiarity of constitution, for the most part the most powerful of all concurrent causes, is overlooked; and rules are laid down for the treatment of chronic diseases with the same precision as for cases of fever and inflammation. The little success which often attends the usual plans of treatment in the former, is in a great degree to be ascribed to this cause; for the same chronic disease in different constitutions, although there are general principles applicable to all cases, often in other respects require, not only different, but opposite plans of treatment. This observation is more or less applicable to all cases requiring an alterative plan.

When we find a practitioner laying down rules of treatment for chronic cases in the same way in which we are enabled to do in acute diseases, we may be assured that he has not sufficiently considered them. This, in the nature of things, is impossible. It is here as necessary, in each particular case, to determine the peculiarities of the constitution as the principles of the treatment; without which, if the practice be successful, it is merely a matter of chance. Hence a principal cause of the uncertainty which has prevailed in the treatment of such cases. With due attention to the constitution, and the circumstances in which the patient is placed, the practice may be rendered nearly as determinate as in the most acute diseases.

In the greater number of cases, what are called tonic medicines can generally be borne for two or three days without inconvenience; and even this temporary use of them, where the debility is great, is often of consequence, but if they be persevered in after they in any degree produce increased heat or restlessness, much injury accrues, as I have often witnessed, from the patient, or even the practitioner, not being aware that the injury proceeded from the tonic.

In some constitutions, where the frequent use of the tonic cannot be borne, its less frequent employment often proves serviceable. This is especially the case with the more tonic bitters, particularly after the use of the alterative. In such cases I have seen both the appetite and strength very quickly improved by moderate doses of such medicines as the compound tincture of gentian, in any of the distilled waters, taken once, or at most twice a-day, at

early hours, when its more frequent employment could not be borne. In other constitutions, even this use of any thing which deserves the name of tonic is precluded. Many attempt, by increasing the discharge from the bowels, to enable the patient to bear the tonic, and in some constitutions the object is attained; but in others, although the case be of the same nature, the attempt altogether fails—the patient is either unable to bear the free discharge, or no discharge is capable of obviating the injurious tendency of the tonic.

Of the more powerful tonics, iron is that which can be most generally borne; and the carbonate and the ammoniated tincture have appeared to me the best preparations, where the source of the disease is in the digestive organs;—but the most powerful, and therefore the best in the few cases in which it can be borne, is the bark, and the sulphate of quinine is, by many degrees, its best preparation. This is scarcely ever the case where a tendency to increased heat prevails, and very rarely when it does not; for even when the medicine shews no tendency to produce it, its continued use seldom fails to increase the restlessness and oppression.

Such are the principal observations I have to make respecting the combination of refrigerants and stimulants with the alterative, the effects of which greatly depend upon the proper management of these two classes of its assistants.

The limits of this paper do not permit me to enter on some other means which often greatly assist the alterative. For the effects of antimony in particular, I must refer to my *Treatise on Indigestion*, and that on *Organic Diseases*.

I need hardly say, that, as under all other plans of treatment, the state of the bowels requires constant attention. I have already had occasion to observe that the minute doses of mercury, although, as far as I am capable of judging, by far the most powerful means of restoring the habitual healthy action of the digestive organs, are ill fitted to carry off accumulations either in the hepatic system or the bowels; and that, on this account, during their use, the occasional employment of a more active mercurial dose is often necessary. In the commencement of the treatment it very frequently is so; but

in some habits the alterative soon begins to produce its favourable effects, and further accumulation is prevented. In others its operation is slower, and an occasional active dose is longer necessary. This, I believe, is the only circumstance which, independently of the use of tonics, renders any very active measures with respect to the bowels requisite, unless the head particularly suffers; but there is no case in which attention to their regularity is more essential.

In a large proportion of instances, however, when we are obliged to depend wholly on artificial means to excite the bowels, which is often the case, it answers better to excite them every second than every day. There seems to be always some degree of irritation, even where there is no pain, in the artificial excitement of the bowels, and it is generally a source of relief to the patient to let them rest every second day.

In some habits we find it otherwise, and their daily excitement is more beneficial. This, as just hinted, is particularly apt to be the case where the sympathetic part of the disease is determined to the head. We have seen how much the liver, by its sympathy with the source of nervous power, influences the symptoms of indigestion, and cannot be surprised to find that affections of the head are frequently complicated with those of this organ; and as the sympathy of the liver, on the other hand, with affections of the head, is equally strong, the secondary affection particularly tends to aggravate and confirm the original disease; an effect which it always more or less produces, whatever be its seat. Hence the treatment of the secondary affection is doubly important, and it often considerably modifies that of the original disease.

When the secondary disease chiefly affects the head, the most guarded use of opiates is generally precluded: even the hyosciamus is found objectionable. In such cases, neither the direct effect of the opiate, however small the dose, particularly if frequently repeated, on the brain itself, nor its effect in retarding the free action of the bowels, can be borne. These cases, for a similar reason, require a freer action of the bowels than others, and the patient generally bears it well; the derivation of the fluids from the head more than compensating for any debilitating effect.

All medical men must have observed the unusual determination of blood to the head in debilitated states of the digestive organs. This, in general, has been ascribed to the distended state of the stomach and bowels, from flatulence, and an accumulation of undigested food causing more than usual pressure on the descending aorta; and that this, in many cases, and at certain times, adds to the evil, there can be no doubt, but that it is not its principal cause, appears from its permanency and its existence where there is no morbid distention of either.

I have been at considerable pains, in the treatises above referred to, to point out the cause of this determination, and have adduced such facts as appear to me to leave no room to doubt that it arises from irritation of the digestive organs, in certain constitutions, through the medium of the nervous system, debilitating the vessels of the head; in consequence of which they suffer a slight degree of distention from the force of circulation, and thus receive a larger than due proportion of blood. This principle appears to be one of extensive influence in the phenomena of disease; for I believe it would not be difficult to shew that on it sympathetic diseases of an inflammatory nature, of whatever part, always depend; the part affected being determined by some peculiar sympathy, as in the present case, or other cause, rendering it more liable to be affected than other parts.

Inflammation consists in a debility and consequent morbid distention of the capillary vessels of the part, and excitement of other parts of the sanguiferous system, in consequence of that debility, and the great sympathy which exists between the nervous system and capillaries, by which the larger vessels of the part are excited to increased action; an effect which, if the inflammation be considerable, or seated in a vital organ, is extended to the whole sanguiferous system; the final cause of which is evidently to support the circulation in the debilitated vessels, and excite them to a due performance of their function*.

The secreting surface which, next to the bowels, most influences the progress of chronic disease, is that of the

* Introduction to my Treatise on Symptomatic Fevers, where the experiments and observations, on which these opinions are founded, are detailed.

skin; and from the state of this organ we can judge better of that of the constitution, and consequently of the measures most likely to suit it, than by any other means with which I am acquainted. While the system is under the influence of permanent failure of function, and still more of organic disease in any of the vital organs, it is usual for the skin to be much drier than in health; and it is sometimes obstinately arid, and, particularly in children, even shrivelled. It is almost uniformly dry when any considerable inflammatory tendency prevails; but in many cases, particularly where the derangement is only functional, and the inflammatory tendency inconsiderable, especially in those in whom nervous symptoms most prevail, it is in the opposite state—almost constantly bathed in perspiration. In both cases the secreting power of this organ is debilitated, the sweat in the latter case being the effect of relaxation.

In functional disease, although the symptoms may be more frequently severe when the skin is dry, I have almost uniformly found them most obstinate when it is constantly, or almost constantly, moist; and I never, indeed, saw a case of this kind in which they did not prove more or less so. We have means of almost certain success for bringing the system into a favourable state for the alterative, and assisting its effects when the obstacle is the inflammatory tendency with a dry skin; but here the alterative is generally very ill borne, being much more apt to produce the sedative effect; and in such cases we have few means which either add to its power, or assist the patient in bearing it. The whole train of anti-inflammatory, with the exception of a cautious use of local measures, is generally out of the question. Even where there is a good deal of local tenderness, the patient bears them ill, and, fortunately, the constant discharge from the skin renders them less necessary. The great characteristic of such cases is nervous debility; and yet it very often happens that strengthening means are very ill borne, the oppression they occasion adding to the sinking and debility. In general, however, patients of this description bear them better than others, in proportion as they require them more. The most beneficial are such as tend to invigorate the skin,

and restrain the discharge from it; and of these I have found the sulphuric acid the best.

When it agrees with the stomach and bowels, it often proves an important resource in such cases. I have never found it necessary to discontinue the minute mercurial doses on account of it, but, on the contrary, it has often been the means of enabling the patient to bear them; nor do I recollect any case in which it caused them to gripe, although, if they have had this effect, it is apt to increase it. The more powerful tonics, in general, are also borne better than where the skin is dry, but often fail in affording effectual relief, and, as I have just had occasion to observe, sometimes increase the sufferings.

These, and other details relating to the use of the alterative into which I enter, may appear tedious, but, except in the most favourable constitutions, which only now and then present themselves, an attention to them is essential to its success.

The effects of minute doses of mercury just mentioned, the griping, and other symptoms of irritation in the bowels, are among the most formidable obstacles we have to contend with in their employment. In the majority of cases, fortunately, they do not occur. So great is the injurious effect of this irritation, that, if it cannot be allayed, the alterative, at whatever expense, must be abandoned, for such cases will bear no serious cause of continued irritation, and in the use of opiates, we have seen, we are greatly restricted. The henbane, like the rest, is often not sufficiently powerful in any dose that is not injurious. If neither this, nor any other opiate, in very small doses, will answer the purpose, we have no resource but still lessening the dose of the alterative, having recourse to a milder preparation, or increasing the interval at which it is given; and if these means will not leave such a dose as is still capable of making some impression on the disease, the medicine must be laid aside.

In functional disease the gums are never allowed to become affected. There is no occasion for so considerable an effect of the alterative; and any thing like salivation always does harm, where all causes of irritation are particularly injurious. Many have expressed their surprise that any caution of this kind is requisite in employing such

doses as half a grain of blue pill, yet the reader has seen a case in which they produced severe salivation, where the largest doses had failed to produce any degree of this effect. The rule I follow is, to direct the patient, when the least uneasiness in eating any hard substance is felt, to discontinue the pills for one or two days, which is generally sufficient to remove it, or for whatever time is necessary for this purpose; and, although a slight degree of tenderness is often felt, I never have any trouble from this cause; for, as I have already had occasion to observe, I have never in any instance seen, in the use of such doses, the sudden salivation which sometimes supervenes when this medicine is taken in the usual way, in which case, when any cause suddenly checks its free passage out of the system by the skin, or other excretory, a large quantity is suddenly thrown on the mouth. For a similar reason the patient is much less liable to the effects of cold than under the usual modes of giving mercury, the minute doses having comparatively little effect in opening the skin. Confinement is never necessary on account of the medicine; and the only precautions I have found requisite in this respect are, not to get wet, nor to be much out at night. The foregoing circumstances, and particularly the tendency to salivation, always giving sufficient warning, afford a degree of security under their use, which is not to be obtained when larger doses are employed. The observation just made, respecting the gums, applies also to the bowels. The patient never experiences the sudden and severe affections of them which are apt to arise under the use of larger doses. The quantity of the medicine is too small to produce any considerable effect, unless its use be continued after the irritation has supervened.

It appears from all that has been said, that the injurious effects of mercury, depending on peculiarity of constitution, are of three kinds—the irritation of the alimentary canal, too great a tendency to affect the salivary glands, and a general state of irritation and debility, arising from the sedative effect sometimes of even the most minute doses; and I frankly confess, that I have never been able to discover any means of effectually obviating any one of these tendencies when they have existed in a considerable degree; and under such

circumstances I have always found it necessary very soon to lay aside the medicine. The first, we have just seen, may in some degree be obviated by opiates, but the assistance they afford, in any dose in which they can be given without injury, is never long effectual, and the treatment under their influence, unless small doses of henbane answer the purpose, which is rarely the case, never proceeds well.

On the unusual tendency to affect the salivary glands, little need be said; whether the alternative can be continued depends on the degree in which it exists, not on any means we possess of obviating it: it fortunately very rarely occurs in such a degree as greatly to interfere with the plan of treatment; and such a tendency to it as only shews itself slightly, and is removed by discontinuance of the medicine for one or two days, is generally favourable. It is a proof that it is well retained in the system, which is essential to its powerful operation as an alternative.

On the last head—the tendency to the sedative effect—which must always be watched with great care, it is necessary here to make some additional observations.

In some constitutions, we have seen the freest employment of mercury seems to have little tendency of this kind; when this is the case, and it produces neither irritation of the bowels, nor shews an unusual tendency to affect the salivary glands, we see the full powers of the medicine in restoring the vital functions, compared to which those of any other we possess hardly deserve to be mentioned. But it is almost as rare to meet with constitutions thus admirably adapted to it, as with those which wholly preclude its use. I have laid before the reader a case of acute disease, requiring the largest doses, in which the constitution was thus favourable, and shall presently have occasion to lay before him a case of the same kind, in which the minute doses alone, but for a great length of time, were required.

Chronic cases of this kind are not unfrequent, because the minute doses, in proportion to the smallness of the quantity, are less apt to produce the sedative effect, however long continued—one of their greatest advantages. In the generality of constitutions this effect is seldom observed in the commencement

of the course. In many instances, however, it sooner or later shews itself, and if not immediately attended to, always does mischief. The symptoms are a general languor and debility, often attended by a great degree of nervous irritation, the increase of which nothing can effectually prevent, if the medicine be continued in the same dose and frequency; and when it has once taken place, we have seen still smaller doses are apt to renew it.

Such strengthening medicines, it has already been observed, as the patient can bear well to a certain degree, relieve it; but I know from a very extensive experience, that they seldom wholly prevent it when it exists to any considerable degree. They rather obscure than lessen the mischief; and the medicine ceasing to be a wholesome alterative, little or no progress is made in the cure. All medicines whose sedative effect is exerted on the powers of the system alone, and in no degree on the sensibility, are pernicious as soon as this prevails over the stimulant effect, which alone is salutary; and this change will take place if the dose be either too great, or too long continued, for the particular constitution.

Nothing has served more to prevent the beneficial employment of mercury, and other powerful medicines, than the little attention which the influence of constitutions in determining their effects has obtained. The observations I have had occasion to make on the influence of peculiarity of constitution on the treatment of diseases, apply in no instance more strongly than to the effects of such medicines.

Those who have never seen the effects of minute doses of mercury, will suppose we have little reason to dread the sedative effect of such doses as half a grain of blue pill given three times a day, however long continued. My reply is, that in certain constitutions I have seen the eighth part of a grain, taken twice a day, produce such a degree of this effect, as would have risked the patient's life had it been persevered in. Can it surprise us that such should be the case, when one dose of a quarter of a grain in others is capable of producing salivation? Others may imagine that a medicine capable of such effects in such doses, must be more or less injurious in all cases; these objectors I should refer to such cases as one I

am acquainted with; in which one dish of tea has produced as bad and more rapid effects; yet nothing can be more innocent to most people. Such is the power of idiosyncrasy in determining the effects of medicines, and even articles of diet. The late Dr. Gregory had a fit of palpitation so severe as to confine him for several days, from eating the seed of an apple, and any bitter produced the same effect upon him. It is on the general effects of medicines, not on the exceptions, that our judgment of them must be formed.

I have just had occasion to observe, that when the sedative effect of mercury has once been produced, it is apt to shew itself from much smaller doses than those which first occasioned it. The patient in whom the eighth part of a grain of blue pill produced so pernicious an effect, had in the first instance for some time taken, without inconvenience, half a grain three times a-day. In this case it was necessary, occasionally, altogether to lay aside the medicine; yet, even here, it was the chief means of restoring a degree of health which no plan of treatment, under the most skilful physicians, had for more than twelve years been able to procure.

When the sedative effect appears in the employment of minute doses of mercury, one of three plans is generally found to a certain extent successful; for when this effect occurs to any considerable degree, the relief is never so speedy, and seldom so perfect, as in more favourable constitutions. The first thing which should be tried, is still to lessen the dose. If even the smallest dose which can be supposed to have any effect has still that of a sedative, when given frequently, the medicine must then either for a certain time be abandoned, or larger doses at longer intervals must be employed; which sometimes succeed, after the effect of the more frequent minute doses, where they have previously failed. When the minute doses have, after being employed for some time, produced the sedative effect, the patient generally experiences great relief on abstaining from the use of the alterative, and then only we can judge of the good it has done. In such cases, while he continues to improve under means which increase his strength and tend to mitigate the occasional symptoms, the alterative should not be resumed; but

as soon as symptoms of relapse shew themselves, it is necessary again to have recourse to it, but in still smaller doses. If the sedative effect again shews itself, the only resource, before again discontinuing it, is that just mentioned—a trial of larger doses at longer intervals.

If by none of these plans the effect of the alterative can be so modified as to suit the constitution, and make the necessary impression on the disease, in the cases I am speaking of, whether long-continued functional disease, or such organic disease as mercury can influence, we can in general do little more for the patient than attempt to support his strength and afford him temporary relief.

Such are the chief points to be attended to in the employment of minute and frequently-repeated doses of mercury in chronic cases, in which they may be regarded as the appropriate remedy. The present communication does not admit of my entering more particularly into the nature of these cases. They comprehend more than two-thirds of all the chronic diseases to which we are subject, and are characterized by the digestive organs having more or less share either in the causes which have produced, or those which prolong them. One of these cases, which I have frequently seen prove fatal, I shall briefly notice; because, as far as I know, it has been overlooked by writers, or confounded with other diseases.

It consists in a languid inflammatory affection of the brain, if it deserves that name, and is, as far as my experience of it goes, essentially influenced by no plan of treatment without the co-operation of the minute mercurial doses. It betrays itself by a derangement of function, not remarkable for its severity, but its universality and its obstinacy; and assumes very much the form of common nervous complaints produced by irritation of the digestive organs: but it is more steady in its course, and influences the functions more generally. The only inflammatory indication is a certain degree of tightness of pulse, and an occasional recurrence of feverish heats, except (as not unfrequently happens) the temperature of the body is uniformly below the healthy standard. Yet, even then, traces of inflammation of the brain, or its membranes, are no less found after death which is occasion-

ed by organic disease of this organ, the consequence of its inflammatory state, or by languid inflammation and similar change of structure in one or more of the other vital organs, excited by the long-continued irritation of the brain. I have, in a treatise *on the Preservation of Health, and particularly the Prevention of Organic Diseases*, entered at considerable length into the diagnosis of this disease, and its plan of treatment. I need hardly say that the liver is always more or less affected in it; and where it chiefly arises from obstinate debility and irritation of the digestive organs, which appears to be its most frequent source, the minute and frequently-repeated doses of mercury, with such other means as the more urgent symptoms require, constitute the only plan of treatment which I have found, not only at all successful, but capable of making any decided impression on it. I also beg leave to refer the reader to the same treatise for the account of a disease of the lungs, which appears at advanced periods of life, of very frequent occurrence; respecting the treatment of which, after it is decidedly established, I would make the same observations.

I shall close this part of the subject by cursorily relating a case exhibiting the effects of, and mode of regulating, the minute doses of mercury in a favourable constitution, where an unusual continuance of the treatment was required, but I had none of the difficulties above stated to contend with.

An officer, between thirty and forty years of age, returned from India in a state of great debility. His countenance was sallow, and at once informed the experienced eye that he laboured under organic disease. The liver was much enlarged and indurated; he was subject to severe inflammatory attacks in it and the neighbouring parts, which greatly increased his debility, and frequently brought him into immediate danger; and the whole of his state was such as is supposed rarely to admit of a perfect restoration to health. The temporary attacks were relieved by local blood-letting and such means as allayed the pain and quickly restored a freer secretion of bile; and in the intervals he was desired to take half a grain of blue pill and a grain of the extract of henbane, three times a-day, with such medicines as allayed the tendency to

fever. The most nutritious diet, of easy digestion, which his state admitted of, was enjoined, and he was desired to be in the open air as much as he could without any degree of fatigue or the risk of taking cold; and, as his strength improved, to make walking his principal exercise. In a short time he experienced a sensible improvement in his health, the severity of the inflammatory attacks abated, and in the space of some months ceased to return. He could now move about with more ease, although the enlargement of the liver was still considerable; and, after being made acquainted with the circumstances necessary to be attended to, he was not prevented from going to the country, and to the Continent, to which his affairs called him.

His recovery gradually advancing, he repeatedly thought himself well enough, according to the directions I had given him, to permit the alterative to be discontinued, but was constantly obliged to return to its use, in consequence of a return of the symptoms. I saw him from time to time, without finding any reason to change his plan of treatment, assuring him that the time would come when the means of cure might be laid aside without a return of the disease, and that it only required the slight remains of his disease to be subdued, and the habit of health maintained by the medicines for a certain length of time, in order to render it permanent without their aid. It was now six or eight months from the commencement of the treatment, and hardly any enlargement of the liver could be perceived; and at each interval at which I saw him the improvement, both of his looks and strength, was apparent. I still advised him, from time to time, to try how far the alterative could be laid aside, but to return to it as soon as he perceived the least threatening of a renewal of his symptoms.

At the end of more than two years from the time he had begun the employment of it, during which he was uniformly recovering both his strength and healthy appearance, he found my prediction verified. He no longer required the use of medicine. All enlargement of the liver had disappeared, and he had in all respects regained both the appearance and habits of health. He returned to the service, but not to India; and although three or four

years have now elapsed, he has experienced no symptom of his disease, and has, within the last twelvemonth, gone abroad in a high official situation.

Of the effects of minute doses of Mercury in acute cases.

I am now to lay before the reader such observations as my experience has afforded respecting the effects of minute and frequently repeated doses of mercury in acute cases. It is evident, from what has been said, that they cannot supply the place of the more active doses of this medicine, which are often employed with such advantage in their early stages, with a view to excite the torpid liver, relieve its loaded vessels, and cause a general determination of the fluids downwards, where, from the excited state of the circulation, there is too great a determination to the head. While these and other means of great and immediate effect are to any considerable degree necessary, the minute doses have generally no sensible operation, and therefore no place; but in a large proportion of acute diseases, after they are to a certain degree mitigated, the symptoms are prolonged by the obstinacy of local affections, which have either produced, or been produced by, the state of general excitement. The liver, from its extensive sympathies, often shares in the original cause; and still more frequently the state of this organ, induced by the disease, tends to prolong or renew it, and the patient thus falls into a state of less acute but obstinate suffering.

The cases most apt to degenerate into such a state, are those of protracted fever and affections of the organs which most sympathise with the liver—particularly the brain and lungs. Every physician must have met with cases of fever which neither subsided as usual, nor were followed, as happens in favourable cases, by a good appetite and a more or less rapid recovery of strength. Either the febrile symptoms continue to recur, or the patient remains languid and dispirited, and what are called the remains of the disease, hang about him. In by far the majority of such cases, it will be found that more or less permanent functional disorder of the liver has been established; and although, from the chronic nature of this affection, it has not prevented the subsiding of the

more urgent symptoms, it supports a constant tendency to their renewal; and where it is not sufficient to produce this effect, it frequently prevents the recovery of the appetite, and always of the strength and spirits.

The state of the liver can only be with certainty ascertained by an examination of the regions of this organ, and of the duodenum, where some tenderness or fulness will be discovered, if the cause which impedes the recovery exists in the liver, which it will be found to do in at least nineteen such cases out of twenty.

Every one will agree, that, under such circumstances, all vigorous measures of a debilitating nature are out of the question; but it is not at all uncommon to see them aggravated by the attempt of the practitioner to restore the strength by powerful tonics, by which both the tendency to a recurrence of the fever and the oppression and restlessness are increased; and I have seen many such instances, in which the patient, guided by the effects of these means, has refused to pursue them. In the most favourable cases they tend only to support the patient under his disease, not to relieve it, and, if their effects on the liver be not counteracted by the efforts of the constitution itself, never fail eventually to increase the mischief. The only effectual means are those which restore this organ, which is only to be here attempted by such as suit the debilitated state of the patient.

In many instances it may be effected by a few grains of the blue pill, taken every second night, and gently carried off by the bowels on the succeeding morning, combined with means which prevent the return of the febrile symptoms, and such stimulants as the patient can bear without any tendency of this kind, or any increase of the restlessness and oppression. In the more obstinate cases such means fail; and then I know of none which will succeed except the substitution of the minute and frequently repeated, for the occasional larger mercurial doses, combined with the other means just mentioned, and regulated on the principles I have explained. The existence of such a case as that I am describing is, I believe, always the effect of the state of the liver having been overlooked in the course of the fever, and the frequency

of such cases points out in a striking manner the necessity of attending to the state of this organ in all diseases of prolonged excitement—a necessity still more strikingly exemplified by what I am about to say of those cases in which such excitement is supported by a local cause.

As the blood is returned from the brain by canals which cannot partake of the generally increased excitement of the sanguiferous system, such excitement is necessarily accompanied by a tendency to an accumulation of blood in the vessels of this organ, which, within certain limits for the time increasing its powers, appears to be a provision of Nature, for bestowing on us greater than usual nervous energy at the times it is most called for, as under the impression of the exciting passions, or in running, wrestling, &c. Hence in all diseases of increased excitement more or less tendency to accumulation of blood in the head is a constant attendant. To this we must refer the pain, and many other affections of the head, which are so apt to attend fever.

The tendency to these subsides, of course, as the excitement of the sanguiferous system abates, unless the brain or its vessels have sustained some more permanent injury during the period of excitement. It seems frequently to happen, however, especially when the excitement is severe and long continued, that even where the due action of the brain itself is restored, the organs, which most sympathise with it, suffer more permanent derangement of function. It seems to be in this way, as well as from their vessels partaking of the state of general excitement, that various local derangements arise in fever, affecting the parts which happen to be most liable to disease.

No organ is so liable to suffer in this way as the liver, and in some climates this liability is so great, that its affections become the leading feature in almost all febrile diseases; and even in such climates as our own, functional derangement of this organ is the most frequent of all the local affections which supervene in fever. Such appears to be the origin of the cases I have just referred to, the hepatic affection remaining after the fever has subsided.

It is when it becomes evident in the course of the fever, and when it does so, it always aggravates and prolongs it,

and has resisted occasional active doses of mercury; that I have found the minute doses of this medicine, in conjunction with the usual means, so beneficial. The amelioration of the symptoms, and a more favourable course of the whole disease, may, under such circumstances, be with confidence expected from them, particularly when combined with local measures in the neighbourhood of the liver; and I have never seen an instance in which they in any degree tended to increase the febrile symptoms. By their means, with or without the continued aid of occasional more active mercurial doses, according to circumstances, the hepatic affection may always be controlled and brought to terminate with the fever; so that such cases become as manageable, and as little liable to leave remains behind them, as when no hepatic affection has attended.

In acute cases, I generally give the mercurial every six instead of eight hours. In these, especially where the excitement is still considerable, it rarely shews any tendency to affect the mouth, and, what is remarkable, very rarely to produce any degree of the sedative effect to which, in consequence of the more powerful impression of the disease, the constitution is for the time very little liable. It very seldom, indeed, produces any sensible effect but that of a gradual mitigation of the symptoms.

Towards the decline of fever, where such an affection of the liver has supervened, the fever often seems to be supported by this cause alone; and even in cases where it has proved most obstinate, will immediately begin to abate on the system being brought gently under the influence of the alterative.

There is another class of acute diseases in which the minute mercurial doses are of great use;—I allude to acute inflammation of the liver itself, or of those organs which most sympathise with it—particularly the brain and lungs.

Their benefit in acute *hepatitis* is not confined to the more advanced stages; they are of essential use as soon as the canal is effectually cleared by more powerful mercurials, combined with proper purgatives. When larger doses are given with any other view than the evacuations they occasion, they tend to increase the inflammatory symptoms; yet, in this case, the constitutional as

well as the local effects of the mercurial are required, and by the minute doses it is obtained, as far as I have observed, without any tendency of this kind even in the most inflammatory cases.

When the affection of the liver is secondary, the local operation of the larger doses is often sufficient to restore its function. It is only when it degenerates more or less into a chronic state that the constitutional effect of the medicine is required; but in the case before us, we want all the aids which the great influence of mercury on the part here primarily affected is capable of affording; and this is the more necessary because the disease is apt to assume more or less of a chronic form, and become more obstinate in proportion as it does so; the speedy relief of which will depend on the degree in which the system is prepared to meet it. We have seen in the last case, detailed above, that in proportion as the system was brought under the influence of the minute doses, the attacks of *hepatitis* which threatened the patient's life declined, and at length were wholly prevented.

In inflammation of the brain, the function of the liver is always more or less affected, and this organ not unfrequently partakes of the inflammation; and as its affection has its cause in the powerful sympathy which exists between these organs, it cannot fail to influence the original disease. Thus it is that, in inflammation of the brain, the state of the liver always demands attention—is often the means of supporting and renewing the inflammatory affection of the former organ; and when, as frequently happens, habitual derangement of function in the liver has been originally the means of determining the inflammation to the brain, the treatment of the hepatic affection, after the more severe inflammatory symptoms are relieved by proper evacuations, constitutes the most important part of the means of cure.

We have a familiar instance of the effects on the brain of continued irritation of the digestive organs in the internal water of the head in children, which, in nineteen cases in twenty—I may say, indeed, in all which are not the consequence of mechanical injury of the brain—arises from this cause, and which, without the aid of mercury, may, even in its earlier stages, be regarded as very nearly an incurable disease, so unsuc-

cessful are all our other means without it.

There is a peculiarity in the effects of mercury in children. In them, calomel is generally its best preparation, and the frequent repetition of minute doses has much less sanative effect in them than in adults. The same principle, however, holds in the treatment of their diseases. Except where they require the immediate and powerful effects of the medicine, comparatively small doses at comparatively short intervals is the most effectual mode of employing it. I have found half a grain of calomel, given at the interval of eight or twelve hours in the more urgent, and twenty-four or forty-eight hours in the less urgent cases, combined with more or less rhubarb, or some other cathartic, according to the state of the bowels, the best alternative in them; for here it should always be made to act more or less as a purgative. Its local operation seems to be that which best suits the constitution of children.

Advantage is rarely obtained, but often great distress and even danger, from the mercury affecting the mouth, particularly in very young children, which every practitioner knows is, fortunately, very little apt to happen. Whether given in large or small doses, it must be given as a purgative only, except in a very few acute cases where life is immediately threatened, and the immediate and most powerful effect of the medicine is required; and even here the bowels must be kept in the most open state, which the large doses, sometimes necessary even at short intervals, seldom fail to do. This practice, which is, I believe, often carried farther than is either necessary or proper, is in general borne far better than we should *a priori* expect. The free secretion of mucus in the bowels of children, seems both to defend them against the irritation of the calomel, and to prevent its absorption.

In the less acute cases, it is surprising from what states infants may be restored by the alternative use of calomel, given in the way just mentioned. When the skin has become yellow and shrivelled, and hung loosely round the emaciated limbs, in which there remained scarce a trace of muscle, the wrinkled countenance assuming an expression of extreme old age—a remarkable feature in the worst stage of

certain chronic diseases of infants—while the distended and in many parts indurated abdomen has appeared to have drawn to itself almost all the remaining substance of the enfeebled frame, the change effected by such means in a favourable habit—for we cannot always expect such a result—has surprised all who witnessed it. As the abdomen decreased and became of a natural softness, the limbs have become plump, and the countenance of a healthy colour and natural expression.

From all that has been said of the influence of the brain in the vital functions, and consequently the influence of their organs on it, and from what was observed above respecting its chronic inflammatory affections, the reader will perceive how essential the employment of mercury, given in the way adapted to the state of the case, and particularly that of the attending hepatic affection, is in all long-continued diseases of this organ. Some affection of the liver always attends in such cases, and sometimes, as we have seen happens in stomach affections, even where the state of this organ has had no share in producing that of the brain, the latter is both supported and aggravated by it, so that the cure of the sympathetic not only becomes essential to that of the original disease, but often the most important part of the treatment.

From the great extent of the sympathies of the liver, many of the foregoing observations apply to a great variety of other cases. It would swell the present paper to too great a size, were I to attempt even cursorily to consider all; I shall therefore close it with a few, and but a few, observations on affections of the heart and lungs.

Next to the brain, there is no organ which so powerfully sympathises with the liver as the lungs. It is now above five-and-twenty years since I first, in my *Treatise on Symptomatic Fevers*, endeavoured to distinguish that species of pulmonary consumption which arises from affections of the liver, and to point out its appropriate treatment. A few years afterwards I presented to the Medico-Chirurgical Society a paper on this subject, which appeared in their Transactions, and which about ten years ago was re-published in my *Treatise on Indigestion*; and in a *Treatise on Organic Diseases*, published in 1830, I have en-

tered into the subject more generally, and pointed out other affections of the lungs which often have the same origin.

Although it is much less common for disease to spread from the lungs to the liver than *vice versâ*, yet this not unfrequently happens, and the affection of the liver then re-acting on the lungs, never fails to add to the obstinacy of the original affection, and to increase its tendency to return; so that, as we have seen with respect to the stomach and brain, the treatment of the sympathetic affection, in protracted cases, sometimes becomes the most essential, recovery being prevented by it alone; for, from the various causes which have been mentioned, wherever affections of the liver supervene, they are apt to become the most obstinate part of the disease. On this part of the subject I must also decline further to enter, the present paper having already extended beyond the limits I had assigned to it; I shall, therefore, only observe, that it affords many proofs illustrating what has been said of the state of the liver in diseases which, at first view, appear to have little relation to it. The liver being, on the one hand, an organ of little sensibility, and on the other of the most extensive sympathies, the reader will easily perceive how readily, in complicated cases, it may essentially influence the disease, and yet escape the attention of a practitioner who is not sufficiently aware of its peculiarities. I need hardly add, that in all cases in which it is concerned, whether primarily or secondarily, the medicine which so powerfully controls it forms a more or less essential part of the treatment, not only as far as relates to the speedy or protracted recovery, but often to the favourable or fatal termination.

The sympathy of the heart with the stomach is greater than with the liver, as many of the phenomena of disease evince; but such is the immediate sympathy of the stomach and liver, that, in many cases, the heart is almost as much influenced by the state of the latter as if its sympathy with this organ were more direct; and as we have no means of restoring habitual debility of the stomach without restoring the function of the liver, the treatment is pretty nearly the same in protracted functional disease of the heart as if it more immediately depended on the state of that organ.

It is of the first importance in the treatment of affections of the heart to inquire how far its organic diseases can be traced to long-continued functional derangement, and what share affections of the digestive organs have in supporting that derangement. These questions I have considered in a treatise just mentioned, to which I must refer the reader. Much of what is there said is strikingly illustrated by the case of Mr. Hobson, of Mary-le-bone Street, a well-known and highly respected member of our profession, related by himself in the number of this journal for the 22d of October, 1831, page 79. This case is equally illustrative of the power of sympathy in determining the phenomena of disease, and that of the minute and frequently repeated doses of mercury in controlling them.

Mr. Hobson had for thirty-four years laboured under symptoms of diseased heart, to which all the powers of his constitution were yielding. He had become pale and œdematous, with habitually oppressed breathing, which in a great degree incapacitated him for all active duties, and rendered him subject to frequent attacks that immediately threatened his life. Being a medical practitioner of the metropolis, he of course had the advice of many, and those the most skilful, and was regarded by all as labouring under confirmed organic disease of the heart, so that no attempt was made but with a view to present relief, and he had not for some years left his house without his name and address in his hat, fearing that he might not return alive. I was led from many circumstances, notwithstanding the severity and long-continuance of the symptoms, to regard the affection of the heart as chiefly sympathetic. The function of the liver was always more or less, and occasionally, much disordered; and several of the symptoms led me to believe that if organic disease of the heart did exist, it was not in sufficient extent to cause the effects I witnessed.

For many months he steadily pursued the plan of treatment which I have laid before the reader, taking half a grain of blue pill three times a-day, combined with such other means as tended to restore the digestive organs, and relieve the occasional more severe attacks. This plan had not been continued for many weeks before symptoms of amendment appeared, and in the course of a

twelvemonth I had the satisfaction to see him relieved from every symptom of diseased heart. His colour became healthy; the dropsical swellings left him, and he was restored both to the appearance and functions of health. He was so impressed with the plan of treatment which had been pursued that he published the account of his case to which I have just referred, and which, as he was in every respect so well qualified, will make a stronger impression on the reader than any account of it I could give.

I could mention several similar cases, though of less continuance, in one of which the symptoms of organic disease of the heart were quite as strongly marked, and which, after a continuance of many years, yielded as perfectly to the same means.

Enough has now been said to illustrate the two great objects I had in view in the present paper. The extensive influence of sympathy in the phenomena, and consequently the necessity of never losing sight of it in the treatment of diseases; and the power of mercury in influencing their progress, arising from its influence on the extreme parts of the sanguiferous and nervous systems, on which the functions of life immediately depend, and its controlling the affections of the organ whose sympathies are at once the most extensive and the most powerful.

It would not be difficult to shew, that on the power of sympathy the phenomena of general diseases wholly depend; and all internal local diseases may be so simulated by their corresponding sympathetic affections, that there is often great difficulty in distinguishing them—a difficulty not a little increased by the constant tendency of the sympathetic to change into the real disease, which frequently, in regulating the treatment of the different stages, renders morbid dissection itself, in general our surest guide, fallacious.

The effects of sympathy, which under all circumstances is constantly operating in our frame, have never been disregarded by physicians; and the powers of mercury have in many respects been well understood by them. It is not difficult to prove, however, that the former have not sufficiently commanded their attention, nor have they sufficiently investigated the latter.

Even in the present state of medicine in this country, one of the most frequent and fatal errors is the treatment of the sympathetic, which in a great proportion of cases becomes the most prominent, disease, without the necessary attention to the original affection, obscured, but not removed by it; for it is in vain to attempt to obviate a consequence, if the cause be still allowed to operate, a far less degree of which will support than that which originally caused the train of symptoms to which the attention of the practitioner is too often unfortunately confined.

The principles on which mercury operates have, as far as I am capable of judging, been in several respects misunderstood, and consequently the means by which we may as much as possible obtain the good and avoid the bad effects of this medicine, in several ways mistaken. The attainment of these objects the reader must have perceived, if I have been so fortunate as to carry him along with me, in a great measure depends on a correct knowledge of the laws of sympathy. Without this knowledge, however well the operation of mercury may be understood, its correct application is impossible.

No practitioner can avoid seeing, under certain circumstances, the beneficial effects of this medicine, employed, either as an active and present, or as an eventual and more slowly operating means of cure. These are often apparent to the least observant, but to distinguish all the cases adapted to its employment, and to regulate it in the most beneficial manner, require a more extensive knowledge of the nature of disease, and the effects of the medicine, than at first view appears. No one can be more sensible than myself how much remains to be done in these subjects. All I have attempted is to give the result of my own experience relating to them, and point out what appears to me to be the proper mode of investigating them; and particularly to press on the reader that while our attention is confined to the more prominent symptoms of disease, and the more immediate effects of the medicine; we may do much mischief by so active a means, and we shall certainly lose much of the advantages it is capable of bestowing.

MEDICAL GAZETTE.

Saturday, March 31, 1832.

"Licet omnibus, licet etiam mihi, dignitatem *Ar-tis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."—CICERO.

COMPARATIVE MORTALITY OF THE DISEASES OF LONDON*.

FROM the repeated opportunities which we have taken of late to call the attention of the profession to the subject of medical statistics, it will be judged (and judged rightly) that the importance is not small which we are inclined to attach to this—we have no doubt to many of our readers—both novel and interesting branch of knowledge. In truth, the value of the species of information which it affords has long been most egregiously slighted. Century after century has been passing away—theories and systems have been rising and setting—and the conjectural nature of medicine, with its almost entire dependence on experimental research, has been pretty generally admitted; yet, of the advantages about which, with regard to final results, statistics alone can supply them with solid information, medical theorists have in very few instances condescended to avail themselves. The materials, it is true, have long been lying in an almost unmanageable condition; but this excuse will hardly serve their turn in future. The volume, of which we have given the title below, contains a treasure of in-

struction luminously disposed, and which, if rightly used, may, in more senses than one, have a beneficial influence in remedying the diseases of the nation. Need we add, that the sense in which *we* would be understood to speak, is one totally devoid of all metaphor?

There is an obvious and curious subject on which the work before us could, we are sure, throw much light, and we are not aware that it has ever been attempted to elucidate it before—owing to the want, no doubt, of such data as Mr. Marshall's volume now so compendiously supplies: we allude to something in the nature of an inquiry into the relative merits of the systems of practice which have prevailed in this metropolis within the last two hundred years. To this topic we may, on some future occasion, probably revert; but at present we prefer to confine ourselves, as much as possible, to some of the most striking facts which arrest our attention in turning over Mr. Marshall's pages.

With the history of the bills of mortality, our readers must be all familiar; we shall therefore merely remind them that, at the first registering of those documents with any sort of regularity, which was about the year 1603, they merely consisted of lists of christenings and burials; but in 1629 they assumed a very superior degree of importance, containing, in addition, the fatal diseases and casualties of the metropolis, and being returned ever since, in that form, weekly, to the Secretary of State for the Home Department, as well as to the Lord Mayor. With such a high destiny, the bills *ought*, as Mr. Marshall justly observes, to be at least respectable; and we are ready to go all the way with him in expressing a certain degree of veneration for the worshipful company of parish clerks—the ancient fraternity of St. Nicholas;

* Mortality of the Metropolis. A statistical view of the number of persons reported to have died of each of more than 100 kinds of diseases and casualties, within the Bills of Mortality, in each of the two hundred and four years, 1629-1831, shewing the proportion reported to have died in each of thirteen gradations of ages in each of the 105 years, 1728-1831, and the total number buried in each parish within the bills of mortality, in each of the 176 years, 1657-1831. Accompanied with a variety of Statistical Accounts, &c. &c. By J. Marshall, Esq. 4to. Treuttell, Wurtz, and Richter.

but our gallantry, we confess it, will not bear us out in tendering an equal degree of attachment to the "old ladies" who perform the function of *searchers*: there we leave Mr. M. to enact the champion alone. Seriously, however, we cannot consent, from all we have heard of those old angels of the dead, even when understood to be on their best behaviour, to liken them, as the above gentleman does, to the *sages-femmes* of our continental neighbours; though we are perfectly willing to allow that in the supposed humble, if not disreputable, nature of their employment, those searchers of the metropolis have been quite as good as could well have been expected. Were these functionaries, indeed, better qualified for their office—and, in short, could the returns which constitute the bills be vouched for as accurate and scientific—we should undoubtedly possess a body of facts the most valuable, perhaps, ever submitted to the public attention. Perfection of this sort, however, we have yet unfortunately to seek.

In comparing the respective mortality of different periods, it is obvious that regard ought to be had to the population of the metropolis at those periods; but it is to be regretted that the population returns cannot be depended upon as possessing any commendable degree of accuracy until so late as 1801; so slow, even with a strong political motive, has the nation been in adopting the advantages of statistical research. In the absence, however, of more exact returns, we are not without occasional resources, of which we may avail ourselves. The reader will recollect that, in the few short remarks which follow, we speak of that part only of the metropolis which is within the bills of mortality; and this includes (and has included, we believe, since 1629) 123 parishes—97 within the walls of the city, and 26

without. Now, of this portion of London, the return of the

Population in 1631 was	130,178
..... 1700 ...	208,300

And it will be useful to state further, that the

Population in 1801 was	364,526
..... 1811 ...	498,719
..... 1821 ...	616,628
.. ... 1831 ...	761,348

The first thing now that we shall notice is the deaths in "child-bed." The annual average of this mortality, during what Mr. M. calls the first period of his display—that is to say, from 1629 to 1689—was 234; during the fourth or last period (1790-1831,) it was but 201. Considered with reference to the population in these respective periods, the decrease of the mortality is remarkable—and, no doubt, highly creditable to the improved resources of the obstetrical branch of our art.

The numbers of children cut off by "teething,"—about fourteen or fifteen hundred annually, on an average, from about the year 1636 to 1752—is also very remarkable; especially as contrasted with the gradual diminution since the latter period,—for many years back, the annual mortality not averaging above three or four hundred.

"Rickets" also, we observe, have gradually become almost totally extinct, after having prevailed to a very extraordinary degree; at one time (between 1647 and 1715) there being regularly set down four or five hundred deaths from this complaint alone annually.

"Convulsions" are still very fatal; though, since 1740, the deaths from this source have diminished in the bills from 8500 to about 2500, at which they stand for the last few years.

The diseases popularly classed under the head of cough, hooping-cough, and croup, have had a most determined rate of progress since 1790. "Croup" is not returned till 1793, when there were

twelve fatal cases of it reported; and the same year there were 352 fatal cases of cough and hooping-cough. Now let us observe the increase at the present period: the deaths from croup in the year 1830, were 126; and the cases of cough and hooping-cough, terminating fatally, were stated to be, last year, 1738!

With regard to hydrocephalus, we fear there cannot be much confidence reposed in the inferences which might otherwise be drawn from these bills; but it is classed very variously up to the year 1790, under the titles of *head-mold-shot*, or *mold-shot head*, and *water in the head*. From 1715 to 1752, the mortality increased from about 25 annually to 150; it then gradually diminished to about 50 annually, at which rate it stood in 1790; and since the latter year it has been gaining ground rather alarmingly—for the last few years ranging generally at about 800 per annum.

“Consumption” is too much mixed up with asthma and “phthisis” in these tables, at certain periods (for instance, from 1690 to 1700, and from 1729 to 1739), to leave us as unembarrassed, as we should wish, in our inferences. Prior to 1690, asthma, it is remarkable, does not seem to be taken into account. But there is something, certainly, very singular in the returns of consumption for the last hundred years: there is scarcely a single year within that period in which the number of deaths does not stand at between four and five thousand. The report, however, cannot by any means be looked upon as unfavourable, seeing the great increase of population (more than double) within the bills, during the period just mentioned, and if we bear in mind also that this, if any, is our peculiarly endemic malady.

From 1629 to 1710 the small-pox and measles seem to have been as remarkable for their variableness, as some other

diseases for their uniformity. Subsequent to the introduction of inoculation, it clearly appears that the small-pox became far more virulent and extended than at any former period, and a seriously-increased rate of mortality continued from it, up to the time that the vaccine came into use; since then it has gradually abated. In 1796 we find 3,500 deaths from small-pox recorded. The annual mortality now, from this disease, is about five or six hundred. But with respect to measles, it is a fact that deserves our best attention, that the mortality from the disease, which continued exceedingly variable and moderate down to 1800, since then has become greatly increased and confirmed in its virulence. In 1801 the deaths from measles were reported as 136; in 1831 they were 750.

Deaths from “fever” have been very gradually and considerably diminishing since the middle of the last century: in 1750 they were 4,294; in 1800 they were 2,713; and last year they only amounted to 965.

“Dropsy” appears to have progressively increased from 1629 down to 1750, subsequent to which date it seems to have somewhat abated; but it manifests a tendency to increase again since 1810. In the latter year the deaths were rated at 778; they were returned last year, after a gradual increase during the intermediate period, at 1,108. And we deem it worth noticing, too, that the deaths from “palsy” are becoming gradually more frequent since about the year 1790; they were then stated to be 80; they were last year 246. This would lead to some important considerations; but we have been carried somewhat farther than we intended, and must now bring our remarks to a close.

It is needless, we think, to add many words on the very interesting and useful nature of the researches into which we have been examining. To all who de-

light in enlarged views of the objects about which the profession is conversant, we are sure this work of Mr. Marshall's will afford great satisfaction; and we have only, in conclusion, to express our hope, that the example which is here set by that clever statistical writer will not be lost sight of. If accurate returns such as he recommends, and of which he has, in the work before us, supplied us with the properest forms, be kept but for a few years henceforth, medicine, we have no doubt, will be most signally aided in its protection of the public health, while it will have advanced considerably towards the rank of the exact sciences, which, without such accurate and copious statistics, it can never hope to attain.

PROGRESS OF CHOLERA.

THE returns which we have published each week must have convinced those who have examined them that the cholera has been for some time steadily, though not very rapidly, on the increase. Few, indeed, are now bold enough to deny either its existence or its identity with the disease which has gradually been extending from the east till it has now reached nearly the western limits of Europe; and the few who do venture to persist in their scepticism are almost without exception persons who had foolishly staked their reputations on its non-appearance in Britain. We this day present to our readers some very important tables, which we have thought would find an appropriate place at the end of our volume. By these it will appear with regard to the north of England, that in those places whence we have been able to procure returns, the mortality of this season during the time cholera prevailed is to an equal and corresponding period of preceding seasons as the usual mortality added to the number of deaths from cholera. With regard to the metropolis, it will be per-

ceived that in the parishes which cholera has not yet visited, there has been no increase of mortality this season; while in the four parishes of Southwark, the mortality has somewhat exceeded the mean of the last few years and the number of deaths from cholera jointly. Thus it is *demonstrated*, 1. That the mortality has been increased in the places visited by cholera. 2. That the increase has been in proportion to, and dependent upon the number of victims to cholera. And, 3. That cholera does not merely take off those who would have died of other diseases, inasmuch as the number of deaths from other diseases remains apparently unaffected*.

MEDICAL ASSOCIATION AND THE CENTRAL BOARD.

AN interview took place a few days ago between the members of the Central Board and a deputation from the "Association" lately instituted to inquire into the nature of the present epidemic. We understand the meeting was rather a ludicrous one; and we cannot help thinking that it shewed a good deal of *nerve* on the part of the deputies, to require the co-operation and support of the Central Board in conducting an investigation, the proposal to institute which originated in the assumption that the said Board was incompetent. Accordingly the visitors seemed to feel rather awkward, and the gentlemen at the Council Office were somewhat shy, throwing an air of absurdity over the whole performance, which, as might be supposed, proved entirely nugatory. The Central Board, we believe, have it not in their power to grant as solicited extraordinary facilities to determine the very *puzzling* question, whether there be really any cholera in London or not—nor can we conceive any such to be requisite. Let the members of the Association visit the hovels and the hospitals where cases of the epidemic are to be found, and they will be very soon satisfied, if they be sincere in their professions of impartiality. One thing we must add—that if they would main-

* See the Tables, pp. 897, 898, 899, of present No.

tain any claim to respectability, they ought instantly to scratch out of their list the name of every one who, not content with his share in the general proceedings, must needs indite letters in the newspapers on his own proper account. Was Dr. Uwins' recent lucubration in the *Times* intended to convince the profession that he was impartial, or otherwise capable of investigating the question in a satisfactory manner?

FRUITS OF THE "NO CHOLERA" DOCTRINE.

A FEMALE, living with a man named Sandilans, in the parish of St. George's, Hanover Square, was seized with cholera, in a violent form, on Sunday evening, and died on Monday. During her illness, her son, a boy of about five years of age, was seized and died on Tuesday morning. The reputed husband, Sandilans, and his friends, averred that the woman, who was on the verge of confinement, had died from parturition, which had been mistaken for cholera, and that she had been "murdered" by the medical gentleman who attended her. An inquest was held, and a postmortem examination was made in presence of eight or ten physicians and surgeons, comprising Dr. James Johnson and Dr. Sigmund, *who, as opposed to the idea of the existence of cholera in the metropolis, had been engaged to attend on the part of Sandilans.* It was proved that she had not died of parturition, as labour had not commenced, and it was the unanimous opinion of all present, on connecting the appearances on dissection with the symptoms as described by those who witnessed them, that she died of the "prevailing epidemic,"—or cholera; but this was not stated in the report sent to the coroner, because most of those who signed that report had not seen the symptoms, and they were not authorised to give in any thing in evidence which they had not witnessed. The jury returned a verdict, "Died by the visitation of God." Sandilans and his friends still maintained that she had been murdered, and between twelve and one o'clock on Wednesday morning he was haranguing a mob to that effect, uttering frightful imprecations against her medical attendant, and vowing his destruction. At five o'clock he presented himself to the gentleman in question, requesting his assistance, being even then affected with cholera. He was admitted to the St. George's cholera hospital, and in eight hours was a corpse, having presented before death a degree of blueness (indigo) which is rarely witnessed.

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Three weeks previously he had been affected with a slight degree of cholera; but having himself practised as a quack among the Irish, he denied that such was the nature of his case; and in proof of his assertion, and to shew that his medical attendant had been mistaken, he stated in triumph that he had thrown his medicines away, and taken his own (a pint of cold water morning and evening, and Hydr. c. creta, with pulv. cretae c. ter die.) With a view of proving that the woman had not had cholera, he also stated that her symptoms resembled his, and he frequently embraced the body of the deceased.

The preceding history is nothing more than one of those occurrences which are the natural and almost necessary results of the part taken by a certain portion of the press, and some prejudiced members of the medical profession, on the subject of cholera. The rabble has been goaded on to acts of violence, and if some strong measures be not adopted, the safety of those employed in attending the poor will become seriously endangered. Even the jurymen—persons not generally remarkable for their wisdom—returned a verdict, in the case of Sandilans, of "death by the prevailing epidemic;" but the *Times*, in some editorial comments on the subject, speaks of "*supposed cholera in the vicinity of Grosvenor-Square,*" and adds, "*it is probable that some common disease may have produced the deaths.*" The prejudice and folly of this can only be equalled by that of the ignorant and misguided rabble, whose passions have been excited by the systematic misrepresentations made by the daily press on the subject of cholera. It must be very gratifying to Dr. James Johnson and Dr. Sigmund, to find that their fame has extended to the parties by whom their attendance was required.

COLLEGE OF PHYSICIANS,

Monday, March 26, 1832.

SIR HENRY HALFORD, BART. PRESIDENT
IN THE CHAIR.

THE meeting was very numerously attended, and we observed among the visitors some distinguished men, both of the church and of the law. A valuable paper on the use of opium in fever, by Dr. P. M. Latham, was read, of which it would be difficult to give any satisfactory account in the shape of abstract, but which we shall publish in full in our next or the following number.

ROYAL INSTITUTION.

Friday, March, 2d, 1832.

WHITLOCK NICHOLL, M.D., VICE-PRESIDENT, IN THE CHAIR.

Mr. Faraday's Electro-magnetic Experiments.

THIS evening, Mr. Faraday continued the account of his electro-magnetic experiments, of which a report will be found in a late number of the Gazette. His chief object was to demonstrate, that a phenomenon discovered by M. Arago in 1824, but the cause of which was not by its discoverer understood, was explicable on the principles previously detailed. It will be remembered by our readers that in the year 1824 the Royal Society of London rewarded M. Arago for the discovery of a means of exciting magnetic action in bodies not containing iron, and his experiment consisted in suspending a magnet above a plate of copper, which latter being made to revolve rapidly, the magnet followed the revolution; but that copper, except in motion, had no such influence. This, and other similar experiments, Mr. Faraday this evening exhibited as further illustrations of the principles explained on the former occasion.

In the library, among a very beautiful Zoological collection, and other curiosities, we noticed several of the double cocoa nuts, which so long excited surprise, and the native country of which has only lately been discovered.

Friday, March 9th.

SIR GEORGE DUCKETT, Bart. IN THE CHAIR.

*Mr. Foggo on the causes of the superiority of the Grecian art.**D'Arce's Apparatus for Extracting Gelatine.*

WE this evening heard nothing to the purpose, and we therefore purpose to say nothing about it. After the lecture was concluded, M. D'Arce's improved apparatus for extracting gelatine from bones was exhibited, and some of the soup made in the theatre. It is an improvement of Papin's digester. It is a pity that these machines are not more used in England, for we heard that 800,000 rations of soup are made in Paris weekly from bones; and we are informed, that if the bones of an ox were put into the digester, and the whole of the flesh into any other vessels, that the bones would yield one-third more gelatine for soup than the whole of the meat; i. e. the proportions of the former would be as three, the latter as two. The fibrine, of course, would be eatable and useful: it is of the soluble matter only that account is here taken. The refuse of the bones, after the gelatine is removed, forms excellent materials for making animal charcoal.

The announcement for Friday, the 10th, was, "Mr. Wheatstone on the vibrations of columns of air." On account of Mr. Brande's continued indisposition, his lectures on electricity have been postponed, and Mr. Burnett commenced on Tuesday last his course on the purposes of botany.

MEDICO-CHIRURGICAL SOCIETY.

February—March, 1832.

MR. LAWRENCE IN THE CHAIR.

On the Operation of Bronchotomy, and the Treatment of Wounds in the Throat. By — Wood, Esq.

THE reading of this paper occupied the Society for three successive nights of meeting; it gave rise, however, to no discussion of any consequence. The author, while house-surgeon of St. Bartholomew's, enjoyed peculiar opportunities for pursuing the necessary study for the illustration of his subject; but it seemed to us, upon looking over our notes, and awakening our recollections of what we heard communicated by him to the Society, that there was little or no originality in it, and that the author was more indebted, for the chief bulk of his paper, to his reading than to his actual experience. There were, indeed, copious extracts given from the writings of Mr. Porter, Mr. Lawrence, Sir Charles Bell, and other eminent authorities, both native and foreign; but there was no striking truth or novelty, that we can remember, elicited from the author himself. He advocated the adoption of bronchotomy in more frequent instances, and more freely, than is usually done by the generality of practitioners; and, on the whole, he appeared to impress his auditors with a very favourable idea of his industry and his merits.

STATISTICAL RETURNS ON CHOLERA.

THE Central Board have issued a set of forms for returns which they recommend to be kept by medical practitioners having cases of cholera under their care. This proceeding is in exact accordance with the observations made in this journal a fortnight ago, and we only regret that a methodical plan of registering the most important facts was not adopted at an earlier period. We are told that the measure was in contemplation before our strictures appeared: very likely; but it is just possible that we may have had some share in hastening the rather tardy operations of the Board. We earnestly recommend the general adoption of the forms in question, (particularly No. 5) as calculated to give something like system to the observations made on the prevailing disease.

COMPARATIVE MORTALITY AT NEWCASTLE-UPON-TYNE,

During four Months of the present and corresponding Period of last Season. Population of the places included in the subjoined Table is about 55,000.

Nov. and Dec. 1830; Jan. and Feb. 1831; and same months 1831-32.	Deaths in the first 4 Months.	Deaths in second 4 Months.	Cholera Funerals included in the adjoining Table.	
			Males.	Females.
St. Nicholas'	23	67	13	19
St. John's	74	96	4	7
St. Andrew's	87	104	5	8
All Saints'	31	144	28	59
St. Anne's	11	66	31	29
Westgate	47	103	18	17
Ballast Hills	248	341	30	48
Total in Newcastle	521	921	129	187
Increase of the last November, December, January, and February, over the former year,	400		Total deaths from cholera...	
				316

Total Burials in all Parishes.

Nov. 1830.....	132.	Nov. 1831.....	132*.	
Dec. 1830, 141	} 266.	Dec. 1831, 298	} 615.—Excess, 379.	} Total Excess,
Jan. 1831, 125		Jan. 1832, 347		
Feb. ... 123.		Feb. ... 144		
			Excess, 21.	400.
	389		789	

The above shews—

1. The whole of the deaths in all the parishes for the four months.

2. Totals of the above contrasted.

3. Number of deaths from cholera 316, being less than the excess of 400; probably some cholera patients were not reported, because the official reports only apply to the parts of the parishes in the town, whereas the cholera extended into the country.

4. Proportion of sexes.

The official reports give less than the number of burials from cholera. 298 reported; actual number of burials from the disease reported, 316.

The disease broke out so as to be officially reported December 9.

Number of deaths in November of the two

years precisely the same. In December and January it prevailed most, and was nearly over in February. Excess in three months, 400.

Number in all four months of first year not materially varying; November and December least healthy. Deaths nearly the same, if 16 are included, the probable number in All Saints'. January and February nearly the same. Average of the four months, 125. In February, the disease having nearly ceased, the deaths do not much exceed the average, the excess over the former year in that month being only 21.

In December and January, when it principally raged, the excess of the cholera year was 379.

Proportion of females to males: excess of former, 58.

COMPARATIVE MORTALITY AT MUSSELBURGH,

During the prevalence of Cholera†.

It is melancholy to think, three weeks having only yet elapsed, that the deaths from cholera alone should exceed the average annual mortality of the parish.

Feb. 22d.—Total cases 485; Deaths 193; Cures 235.

* Exclusive of burials in All Saints' not reported in 1830, and therefore not included in 1831, sixteen in number.

† From Practical Observations on Cholera, by D. M. Moir.

COMPARATIVE MORTALITY IN SOUTHWARK,

During five weeks in the three last years, (including the parishes of St. George, St. John, St. Olave, and St. Saviour). The period corresponds to that during which cholera has prevailed in that district.

1830.	1831.	1832.
Feb. 16 to 23. 50	Feb. 15 to 22. 49	Feb. 14 to 21. 30
Feb. 23 to Mar. 2. 47	Feb. 22 to Mar. 1. 22	Feb. 21 to 28. 63
Mar. 2 to Mar. 9. 45	Mar. 1 to Mar. 8. 32	Feb. 28 to Mar. 6. 75
Mar. 9 to 16. 52	Mar. 8 to 15. 50	Mar. 6 to 13. 113
Mar. 16 to 23. 31	Mar. 15 to 22. 92	Mar. 13 to 20. 96
Total, 245	Total, 245	Total, 377

REMARKS.

It thus appears that, taking the mean of the two seasons preceding the present, the mortality during the five weeks above given was 235, that of the present year being 377; so that there is an excess in the mortality of 142 in five weeks. But there occurred 216 deaths in the above period from cholera, so that here, as at Newcastle, &c. the mortality of the present season, during the period when cholera prevailed, has equalled the ordinary mortality, + that from cholera.

COMPARATIVE MORTALITY IN LONDON,

During the same period, in Parishes where Cholera has not prevailed.

In the following parishes, containing by the last returns an aggregate population of 74,290 souls, no cholera has existed, and the returns of the burials in the bills of mortality shew that there has been no increase of deaths. They are selected out of a large

number of parishes, equally exempt from cholera, solely on account of the regularity of the returns; in almost every other case the returns of burials are given in so irregularly, that it is impossible to institute a comparison.

	Population.	Burials in the 5 weeks ending March 22, 1831.	Burials in the 5 weeks ending March 20, 1832.
St. Sepulchre, without Newgate	7,710	22	23
Christchurch, Spitalfields	17,949	54	58
St. Clement Danes.....	11,578	61	54
St. James's, Westminster	37,053	140	104
	74,290	277	239

It thus appears, that, in those parts of the metropolis which cholera has not yet reached, no increase of mortality has taken place during the present season.

SUMMARY OF THE PROGRESS OF CHOLERA IN SUNDERLAND.

THERE were occasional cases at Sunderland, affirmed to be the Asiatic cholera, and then denied, for several weeks before it broke out. October 26th, 1831, there were five deaths. No more occurred till November 3d. The reports began November 13th, 1831.

	New Cases.	Deaths.	Recovered.	Remain.
1831.—November 13th, 14th, 15th, 16th.....	30	15	11	38
... 17th to 23d	69	22	32	22
... 25th to December 1st	68	56	31	38
December 2d to 8th	68	26	32	48
... 9th to 15th	67	40	67	18
... 16th to 22d	50	31	32	7
... 23d to 29th	14	8	6	7
... 30th to January 5th	2	1	3	1

The above table is taken from the reports published, but they were not regularly issued at first. November 24th is omitted. After January 5th, there were only a few scattered cases, the last having occurred March 8th.

Total Cases at Sunderland, 536—Total Deaths, 202—Population a little above 17,000.

SUMMARY OF THE PROGRESS OF CHOLERA AT NEWCASTLE.

IN Newcastle-upon-Tyne, *Armstrong* died at the Cowgate; the disease was first said to be cholera—then stated to be inflammation of the lungs. November 5th, *Oswald Read* died in Sandgate; first called Asiatic cholera, then English cholera. *Jordan* died in Sandgate; his case at the time was considered to be doubtful. One case occurred so late as March 12th: none since.

	New Cases.	Deaths.	Recovered	Remain
1831.—December 9th to 15th	61	23	4	31
... 16th to 22d	103	36	59	44
... 23d to 29th	143	48	71	67
... 30th to January 5th	214	54	106	120
1832.—January 6th to 12th	152	49	144	87
... 13th to 19th	107	36	88	67
... 20th to 26th	75	24	61	54
... 27th to February 2d	48	19	62	26
February 3d to 9th	29	5	29	11
... 10th to 16th	14	2	17	6
... 17th to 23d	14	5	8	6
... 24th to March 1st	6	1	7	3
March 2d to 8th	5	1	5	3
Total.....	971	299	661	

The reports for Newcastle included Byker, Dent's Hole, and the Westgate Township, being the whole that is marked in the map of Newcastle; so that the population may be taken at about 55,000, which makes about six deaths in 1000. But cholera raged most violently in the low parts, near the river; and though it broke out in the High-Street, it did not spread much, except in the Castle Garth, a close, crowded street. Many parts of Newcastle, therefore, continuing quite free, in those parts where it did rage the proportion must have been much larger; seven deaths frequently occurred in one family.

So far as the two preceding tables go, they would seem to shew that the disease in the above, as in many places abroad, had attained its height about the fourth week.

SUMMARY OF THE PROGRESS OF CHOLERA AT GATESHEAD.

IN Gateshead there was one doubtful case about the 14th of December. On the 24th of December one case (a death); on the 25th, three or four cases; on the 26th and 27th, above ninety cases, and fifty deaths. In Gateshead there are usually four or five funerals in a week, the population being a little above 15,000.

	New Cases.	Deaths.	Recovered.	Remain.
1831.—December 25th to 29th	192	68	35	69
... 30th to January 5th	133	34	103	98
1832.—January 6th to 12th	44	24	79	28
... 13th to 19th	14	8	30	3
... 20th to 26th	9	5	5	4
... 27th to February 2d	9	2	1	9
February 6th	2	2	7	0
March 3d	6	3	3	0

Nearly the whole of the cases in Gateshead have been traced to communication with persons ill of the cholera in Newcastle; the last six were from infection from Swalwell; the last occurred March 5th.

Gateshead presents the anomaly of the disease beginning with its greatest degree of fury, and progressively diminishing from the period of its first invasion.

REPORT OF CHOLERA IN GREAT
BRITAIN, UP TO FRIDAY, MARCH
29, 1832.

New cases since our last report, 316
Deaths 268

	Cases from Commence- ment.	Deaths from Commence- ment.
Tower	4	2
Afloat in the River.	86	36
Poplar	18	6
Shadwell	20	17
Limehouse	29	19
Bromley	1	1
Ratcliffe	13	7
Bermondsey	138	63
Deptford	10	6
Southwark	670	326
Newington Butts...	101	52
Mile End O. Town	2	1
Lambeth	124	90
Christchurch	66	30
Westminster	40	22
Chelsea	28	21
St. George, Han. Sq.	12	9
St. Marylebone ...	73	23
St. Pancras	11	10
St. Giles's	62	36
St. Andrew's, Holb.	3	2
Whitechapel	64	43
St. George's East .	27	16
Wapping	6	4
St. Botolph, Aldg.	11	5
St. Luke's	22	13
Bethnal Green ...	10	5
Spitalfields	4	2
Wandsworth	9	3
Woolwich	4	2
Total	1668	872
Cases from places } before reported }	61	43
Grand Total	1729	915

Total number of cases throughout }
Great Britain since the com- } 8815
mencement of the disease }
Deaths... 3220

CHOLERA AT PARIS AND DUBLIN.

LETTERS received from Paris yesterday, (Friday) state that some cases, supposed to be cholera, had occurred during the early part of the present week. Several bodies had been examined in the presence of the Minister of Public Works, whose business it is to attend to the health of the city. The activity displayed on the occasion, and the publicity of the proceedings, shew that cholera has not up to the present time been prevalent in Paris, and concealed by the government, as stated in some of the English papers.

We regret to state that the disease has also broken out in Dublin.

CHOLERA AT MUSSELBURGH.

[FROM a pamphlet by Dr. Brown, of Musselburgh.]

The town is open and well aired, and its soil in general dry, and is nearly equal in warmth, and has less rain, than perhaps any parish in Great Britain. Notwithstanding these advantages, our population may be described as vicious, immoral, and miserable; a full half being liable to the vice of all others the most destructive to religion and morals—I mean drunkenness.

For these last twelve months, poverty, conjoined with the vice of drinking, and wretched accommodation, has been again operating very extensively; and we were suffering considerably from typhus when this horrible pestilence (cholera) appeared, and which has, in the short period of little more than five weeks, carried off nearly 250 of the inhabitants, being about one in every thirty-five of the parish, and nearly equal to twenty months of the usual mortality; and before we finally get rid of this dreadful scourge, our deaths, I have no doubt, will amount to 500, being nearly one in thirty of the whole population.

It is also deserving of particular notice, that there has not been one individual affected with the cholera in Musselburgh, who lived in a front house, or was unconnected with closes or alleys.

In the second stage, viz. cholera spasmodica, which I should mark with vomiting and purging having commenced, to the extent of having produced spasms, and the pulse much affected, and frequently not much better than a flutter, with considerable coldness, and the eyes somewhat sunk; the greatest exertion is necessary to save our patient from the state of complete collapse and asphyxia. It is in this state that I allude to our having placed in a state of safety nine out of ten. The plan here followed is, to lay aside the thought of every medicine but opium, and every cordial but brandy, if at hand. I instantly endeavour to get over an opium pill proportioned to the age of the patient; and if even a child, I prefer this form, and almost always, with a little exertion, succeed. If it should so happen that I cannot get over the opiate in a solid form, I then directly drop the dose of laudanum in a little powdered sugar, and succeed in that manner quite readily; having succeeded in getting over the anodyne, I do not allow a single drop of liquid, not even brandy and water, to be given for nearly an hour, but continue merely drawing a tea-spoon, or feather, dipped in pure cold water, occasionally through the lips; and when the opiate has been retained that time, there is a general cessation of all the alarming symptoms, and not only small quantities of such brandy and water may be allowed, but even panada, with a little brandy, may be com-

menced giving by the end of two hours, at which time our patient is placed in a state of comparative safety; but if the symptoms should be obstinate—in case the vomiting and purging still continue, and seriously threaten a tendency to complete collapse,—I lose no time in giving every nourishing and cordial enema, composed of strong beef brue, two glasses of good sherry, a proper quantity of flour, and as much laudanum as will be considered equal to a dose given by the mouth, which is in my opinion exactly double; and this remedy will be followed with the most immediate benefit if only kept an hour or two, which I have in general been able to effect, by taking care that it was *only tepid*, and by pressure upon the anus continued for an hour; and the case, if cautiously managed, will end in a rapid recovery, and if any symptom of disordered bowels afterwards occur, they can be very easily managed.

MERCURIAL INHALER.

Mr. GREEN, of Great Marlborough-Street, has invented an inhaler for mercurial fumes, by which the system is very rapidly brought under the influence of the medicine. "I took nine inspirations (says Mr. G.) from a drachm of volatilized grey oxide of mercury: during the remainder of the day I had an increased secretion of saliva, and in the night was awoken by severe pain about the face, and on trying to put my jaws together, I found that the whole of my teeth were sore." The ptyalism lasted two or three days. Mr. Searle, who alluded to rather a rough mode of effecting mercurial inhalation in a paper inserted in a former number, seems to have taken his idea from Mr. Green, who states that he shewed him his instrument when in a rude and imperfect form. As now improved, it appears to us well calculated to be of use when rapid salivation is desired.

METEOROLOGICAL JOURNAL,

Kept at EDMONTON, Latitude $51^{\circ} 37' 32''$ N.
Longitude $0^{\circ} 3' 51''$ W. of Greenwich.

March 1832.	THERMOMETER.	BAROMETER.
Thursday . 22	from 35 to 54	30.10 to 30.00
Friday . . 23	43 54	29.92 29.87
Saturday . 24	30 30	29.72 29.92
Sunday . . 25	30 48	30.04 30.14
Monday . . 26	30 49	30.12 30.01
Tuesday . 27	33 54	29.91 29.99
Wednesday 28	31 50	30.03 29.97

Wind variable, S.W. and N. by E. prevailing.
Except the 22d, 23d, and 26th, generally cloudy.
Rain on the 23d; a little snow on the morning of the 24th.
Rain fallen, .1 of an inch.

CHARLES HENRY ADAMS.

* To this is to be added the extra Number, paged with asterisks.

BOOKS RECEIVED FOR REVIEW.

The Dublin Journal of Medical and Chemical Science, No. 1, March 1832.

Mr. Boyle's Practical Medico-Historical Account of the Western Coast of Africa.

Dr. Ayton's Dissertation on Malaria, Contagion, and Cholera.

Dr. Blundell's Lectures on Midwifery and the Diseases of Women and Children.

Mr. Dickson on the Epidemic Cholera of India.

Dr. McCabe on the Epidemic Cholera of Asia and Europe.

Mr. Greenbow on Cholera in Newcastle and Gateshead.

Dr. Aldis on the Medical Properties of Iodine.

NOTICES TO CORRESPONDENTS.

Dr. W. Philip will perceive that we have fulfilled the promise made two months ago of giving his very important and elaborate paper in the present volume: its unusual extent, however, has compelled us to add an extra half-sheet to the present number.

Mr. Middlemore's paper came to hand, but only about ten days ago: it shall have early insertion.

Dr. Gregory's report on Small-pox, and the document on the same subject from Ceylon, are in hand.

Papers have also been received from the following gentlemen:—Dr. P. M. Latham, Dr. Howison, Mr. Salmon, Dr. Hayercraft, Dr. Copland, Mr. Currie, Mr. Nash, Mr. Robbs, Mr. Curtis, Mr. Camplin, Mr. McDivitt, Mr. McEwan, Mr. Leadman, Mr. Bythell, Mr. Harroldt, Mr. Lloyd, Dr. Yates, Mr. Mayson, Mr. Saumarez, Mr. Bury, Mr. Thornhill, Mr. Todd, Mr. Bennett, Mr. Bateman, Mr. Greetham, Dr. Gault, Mr. Busk, Dr. Badham, Mr. Keate, Mr. Wayte, Mr. Rogerson, Mr. Franklin, and Dr. Watson.

The letters and communications of Philo-Galen, J. F. E., a constant reader (Northumberland), J. S. D., B. P., Q., J. F. F., have been received, and shall be duly attended to.

Mr. Wade's paper has been received.

Lectures of Dr. Elliotson and Mr. Earle next week.

ERRATA.

In Mr. Mackenzie's communication, p. 738, 2d col. line 19, for "1831," read "1821;" 2d col. line 42, for "the sphincter," read "a sphincter;" p. 739, 1st col. line 30, for "ascillatory," read "oscillatory;" p. 739, 1st col. line 50, for "this state," read "the state;" p. 740, line 5 from bottom, for "nergleichenden," read "vergleichenden."

W. WILSON, Printer, 57, Skinner-Street, London.

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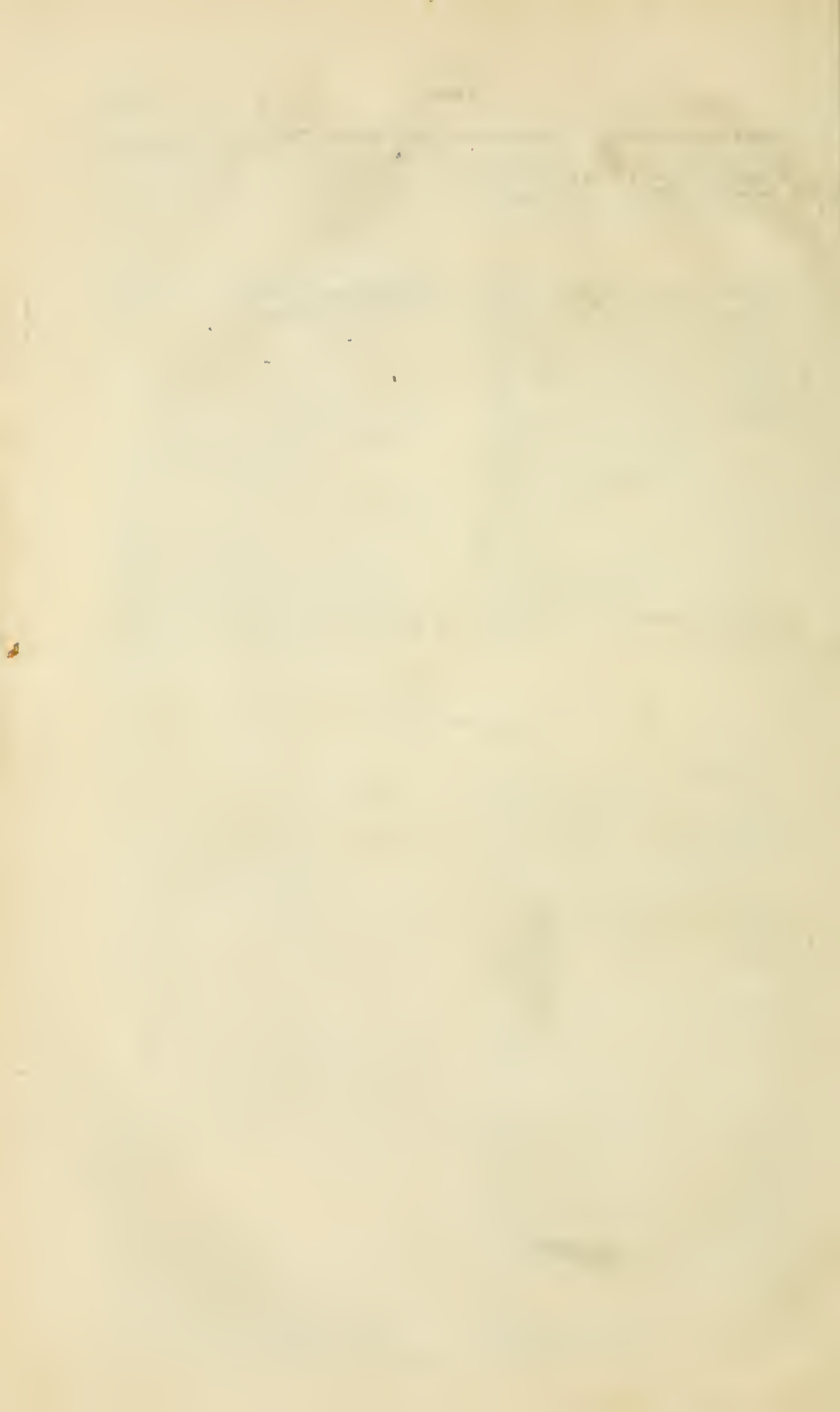
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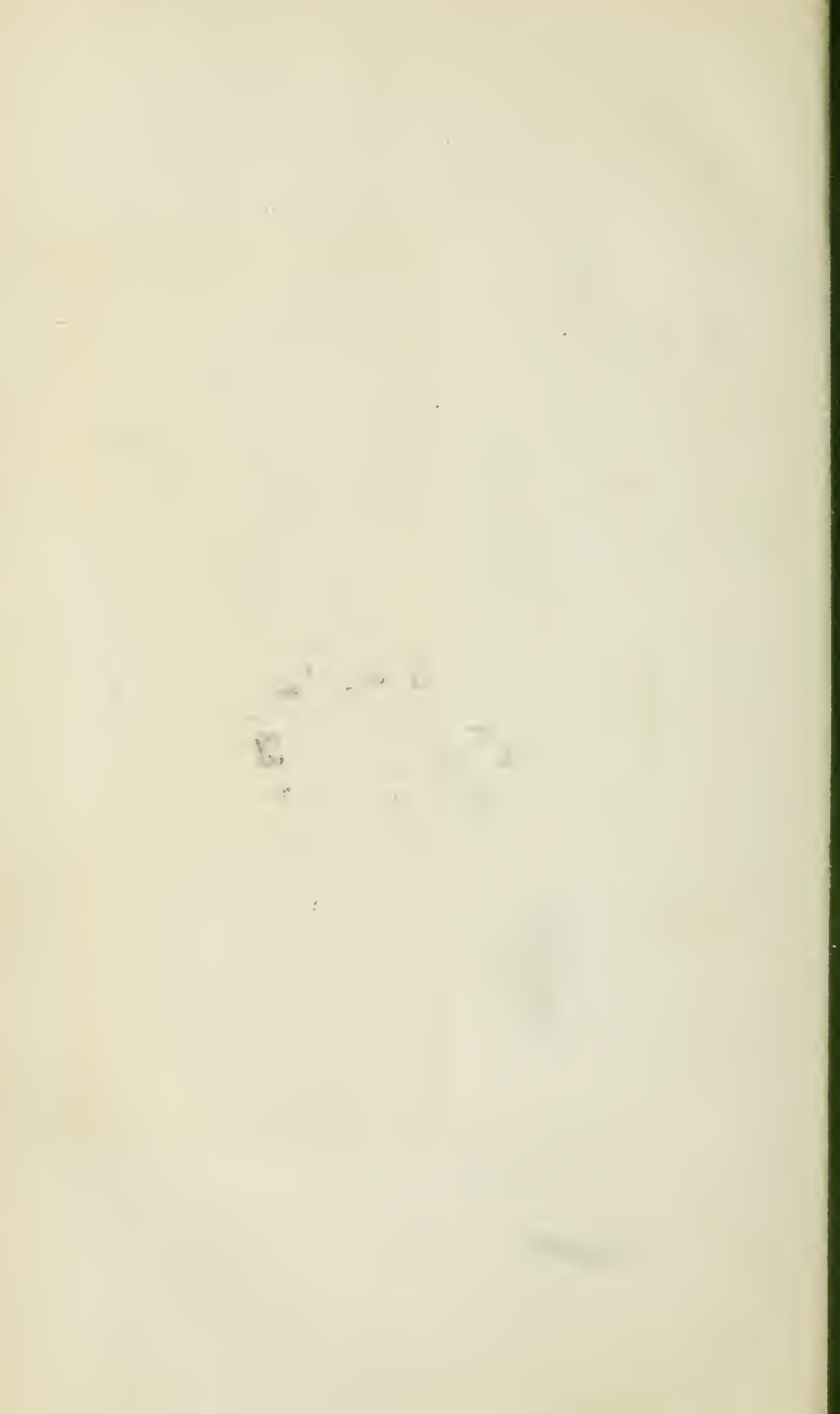
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